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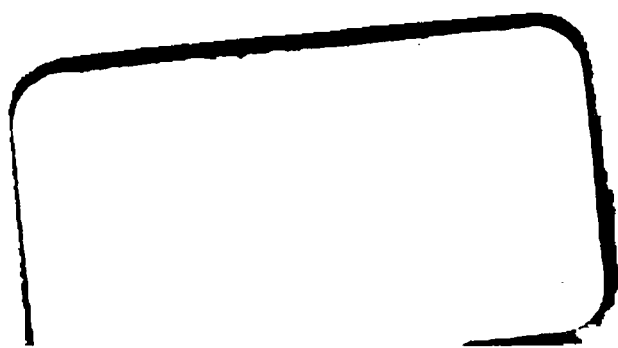
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THE
BRITISH JOURNAL
OF
HOMŒOPATHY.

(WITH WHICH THE ANNALS OF THE BRITISH HOMŒOPATHIC SOCIETY AND THE ANNALS OF
THE LONDON HOMŒOPATHIC HOSPITAL ARE INCORPORATED.)

EDITED BY
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AND
RICHARD HUGHES, L.R.C.P.

VOL. XXIX.

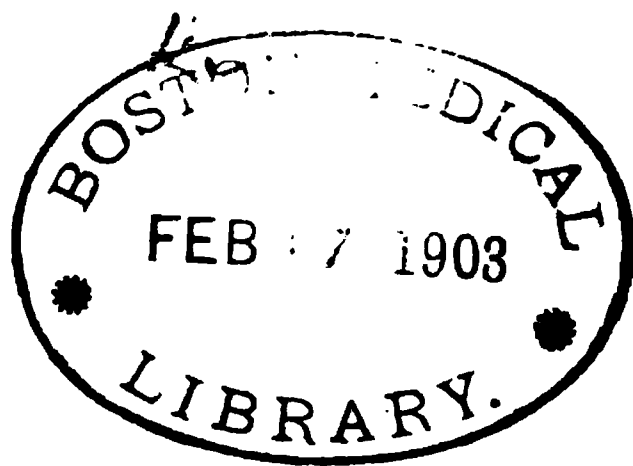
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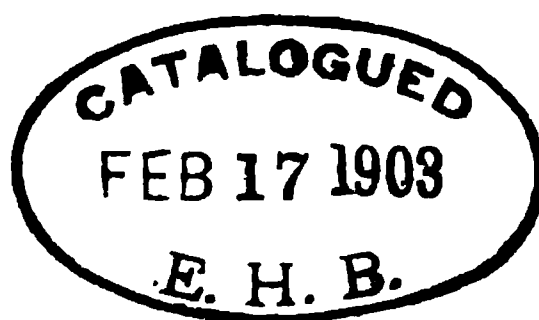
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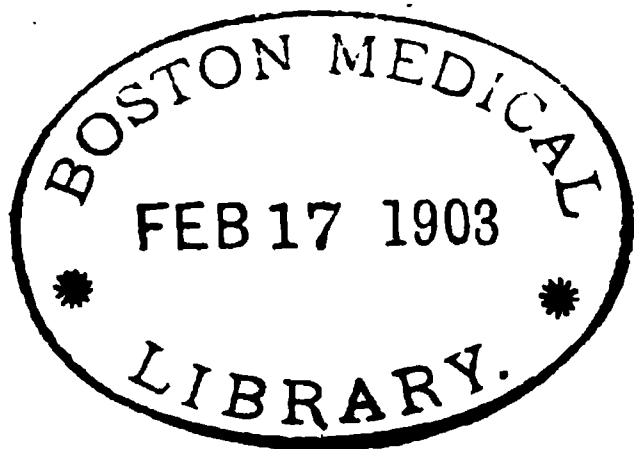
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THE
BRITISH JOURNAL
OF
HOMŒOPATHY.

ON SOME OF THE MEDICAL DISEASES OF THE
BLADDER.

By ROBERT T. COOPER, A.B., M.B., M.D., M.Ch., T.C.D.

(Concluded from Vol. XXVIII, page 664.)

WE now come to consider the wetting the bed generally observed in children, and hence commonly known as children's enuresis. We have termed it *ENURESIS NOCTURNA in SOMNO*, choosing this name because its remarkable and distinctive feature of presenting itself only when the patient is asleep distinguishes it from all other varieties.

The occurrence of micturition while in an unconscious state, and so entirely beyond control, constitutes an essential and striking peculiarity. In some mental affections, and occasionally, though comparatively rarely, in atonied bladder, unconnected with mental influences, will we find a form of enuresis resembling it, and in these the concomitant symptoms, as well as the history of the patients, will be amply sufficient to preclude even the possibility of confounding them.

The term involuntary is very commonly applied to this
VOL. XXIX, NO. CXV.—JANUARY, 1871. A

affection, but we think incorrectly so. There is present, it is obvious, practically no will, this being, at all events in respect to urination, completely incapacitated for controlling purposes, being, therefore, practically absent; consequently the micturition cannot be an act performed against it. Unless, therefore, we alter our meaning of the word, its employment under such circumstances is unjustifiable.*

I would suggest the not wholly unobjectionable term unconscious instead of involuntary, as better conveying our meaning; involuntary being confined to those cases in which, notwithstanding the opposition preferred by the will—of which the patient has, at the time, more or less possession—micturition is excited; unconscious only to cases in which the patient is really ignorant of what is occurring, the will exercising no influence whatever.

If, looking back, we take a retrospective survey of the various expedients that at one time or other have been vaunted as remedial against this affection, we shall find little that is not the offspring of ignorance and credulity. The affection is at first sight insignificant, exciting more disgust than compassion, and yet we find such paraphernalia as the skins of serpents, lizards, and live frogs disgustingly placed in contact with the bodies of the unhappy little wretches who unwittingly offended against comfort and cleanliness, and whose habits these charms were supposed to correct. In addition to these odious curative proceedings corporal punishment was unsparingly resorted to, and many and various deprivations and restrictions were enjoined by pretended authorities, who did not scruple to ascribe to wanton laziness or evil disposition, habits, the result of an enervated or specifically affected constitution.

It is by no means easy to ascertain in most cases what is the primary exciting cause of this too often obscure and

* In physiology an action is involuntary which takes place with the sanction of the will, but which the will has no power to control. In medicine an action is involuntary which, usually under the control of the will, takes place in direct opposition to it. Much confusion has arisen from neglecting this distinction. However, this subject is one we leave to be dealt with by abler hands.

unmanageable affection. We frequently find ascarides and other irritants in the rectum, attended with nightly incontinence; besides, in debilitating illnesses, we constantly find it to exist, but more often still we are obliged to confess we know of nothing more satisfactory to which to ascribe it than a peculiarity in the diathesis, be it hereditary or not. Hereditary it certainly sometimes is: thus, for example, Prout met with an entire family afflicted in this way, the majority of whom were females. And although we generally find that, on the advent of puberty, this unfortunate habit usually disappears, yet we must not invariably calculate upon such a comparatively favorable result; on the contrary, in some individuals it often continues to harass them during lifetime, and it is in such that we so often find a family tendency to it.

The primary exciting cause, as we meet the affection in children, mostly depends upon some form of intestinal irritation; but often, even when this has to all appearances ceased, the bed will be wet quite as much as before. It is in such instances that we have much trouble in choosing the appropriate remedy, but giving due weight to the character of the miasm that prevails in the family, we ought to select accordingly.

Authors, Coulson, for example, mention among its exciting causes irritation set up by a contracted state of the prepuce, and recommend circumcision; narrowing of the urethral orifice will also often be attended with equally annoying symptoms, and here Fleming advises a gradual dilation of the outlet by means of pieces of sponge, &c. One surgeon—his name has escaped my memory—has been daring enough to advise us to keep continually sore the orifice of the urethra by the constant application to it of lunar caustic, so as to have the patient every now and then awakened by the pain occasioned by the trickling of the urine over the sore part. It is hardly necessary to add that we could not approve of such a suggestion. Various malformations of the urethra will give rise to this distress—hypospadias and epispadias are often accompanied by it; we are, therefore, obliged to make careful inquiries to ascertain the existence of

such deformities. It has been suggested that the pressing upwards of the urethra against the pubic arch by tying a piece of a catheter or cork along and underneath the raphé of the perineum would prevent the flow of urine through the urethra, and by distending it would awaken the patient from the inconvenience occasioned. Some have been bold enough to draw down the prepuce and tie a string tightly across it, but this practice is said to be liable to be followed by paralysis of the bladder. The sleep of persons affected with this form of enuresis is generally very heavy, partaking almost of the character of somnambulism, and, according to Sir Henry Thompson, they are either very quick, intelligent children or else quite the reverse, being dull, sombre, and lugubrious, eschewing the society of their playmates. Anything weakening the system generally, or any particular part of the nervous system, will have a tendency to increase this distress; thus, during and after acute affections, exhausting diarrhoeas, &c., it manifests an exacerbation. Still, there are many weak children, and many scrofulous ones as well, who enjoy a complete immunity. In some instances we shall find a decided weakness of the bladder-sphincter, which will be apparent at all times when the patient coughs or uses violent exertion of any kind, but this weakness is by no means invariably associated with it. A want of development in other parts of the body, either congenital or acquired, is often attended with incontinence when asleep.

As regards treatment I really feel perplexed what to advise in the way of remedies. Sometimes one drug will be successful, at others, and with apparently a similar train of attending symptoms, another, while very frequently medicines appear to be altogether powerless. From *Silicea* I have certainly seen good effects, and *Iron*, *Arsenic*, and *Nuxvomica* respectively have had their adherents. *Hyoscyamus* and *Belladonna* I have heard warmly praised, and although we are willing to admit the soundness, as a general rule, of the advice given by Hahnemann to prescribe according to the symptoms, yet this is by no means an easy task where but one abnormal symptom, as frequently happens,

is present upon which we can rely, in our selection of the appropriate remedy. On the whole I am more inclined to trust to the state of the health than to any weakness of the bladder itself, and to give a high standing to hygienic measures, and such a judicious regulation of the feelings as will create disgust at the persistence of an offensive habit, and a determination to discontinue what causes intolerable household inconvenience.

Change of air, a residence at the sea when accustomed to country life, or *vice versa*, is most necessary. Healthy outdoor exercise must be partaken in, and everything that conduces to a stimulation of the energies.

It is well, too, to oblige the patient to abstain from drink before retiring to bed, and to awaken him, about the hour when there is supposed to be an accumulation in the bladder. We must be careful also to see that the bed is as cool and hard as possible, one made of straw with a thick canvas covering is much better than luxurious feather beds, and is much less likely to become offensive and can be more easily changed than one made of animal material. As these children sleep heavily wherever placed, it is often well to allow them nothing more comfortable than a hard deal board over which is placed a thick sheet of canvas, while their bodies are covered with thick coarse linen sheeting, next to which is dried bass-matting, such as is used for protecting fruit trees, and above this any clothing that is rendered necessary by the state of the weather. In this way the bed is kept cool, and though not comfortable, which, it must be remembered, is an advantage, is clean and healthy.

Sir C. Bell recommends that the patient "accustom himself to sleep on his face or side," for in this position, says he, "the urine is not passed, nor is he excited to dream of making urine." In adults we might be able to put this in practice, but its advantage would appear to be very dubious.

And now we must conclude, for the present, at all events, our observations upon some of the medical diseases of the bladder; the surgical will be found, at least in regard to the symptoms presented to us at first sight, less capable of being

classified and individualised, these not being so uniform or distinctive as in the medical varieties.

Throughout the aim has been to supply deficiencies rather than exhaust description, and everywhere by careful consideration of the initiatory symptoms to enable ourselves to be armed against the possible sequelæ likely to ensue, or where this was impossible from our limited acquaintance with the seat of the disorder, or from other causes, to endeavour to suggest a remedy or supply a palliative. In doing so we have had once or twice to refer more at length than the title of the paper might appear to warrant, to the actions of drugs, but for this no apology is tendered, for it is our firm belief, that, in order to develop the advantages derivable from a sound pathology and a scientific system of medicine we must amalgamate, not disunite; and that future advances must of necessity be made by combining both in such a way that the one will hinge upon the other, and appointing to each its due quantum of importance.

The aim and object of every philanthropic physician ought to be the curing of disease, and where, from the intractable nature of the distemper, or the peculiar circumstance under which the patient is placed, this is impossible, he ought to try to palliate the pains and assuage the agonies endured, and should success attend these his benevolent efforts, he is bound not only to give God the glory, but endeavour by recording facts, to extend the sphere of his usefulness beyond the circle of his everyday acquaintances, and, however feebly and insignificantly, to enrich the science of healing.

“Hoc opus, hoc studium, parvi properemus et ampli.”

NOTES ON THE DIOPTRICS OF VISION.

By DR. DUDGEON.

WHATEVER can facilitate the comprehension of the optical apparatus of the eye must be acceptable to the physiological student. It is not every one who can master the algebraical formulæ and abstruse calculations to be met with in works such as that of Donders, *On the Accommodation and Refraction of the Eye*, and it is not quite clear that, even to Donders himself, his calculations are altogether so satisfactory as we might expect, for, at page 62, after many pages of elaborate calculations, he makes this somewhat remarkable confession:—"Our knowledge of the crystalline lens, however, leaves much to be desired. Probably the index of refraction is somewhat too great, and thus the focal distance as assumed is too small." This result must be rather discouraging to the student who has devoted himself to mastering the previous pages of figures. Without affecting to criticise the calculations of Donders, I venture to think that a demonstration of the dioptrical mechanism of vision which rests more on experiments than on calculations will be more easily comprehended, and, therefore, more acceptable to the student than any demonstration founded on calculations only. I, therefore, offer the following explanation of the optical apparatus of the eye in the hope that it will render the subject as plain as it has hitherto seemed recondite. I cannot vouch for the strict accuracy of my measurements, as to secure such exactitude would require instruments of greater delicacy than I possess, but I am confident that they will be found to be a sufficient approximation to the truth for all practical purposes. As every one can repeat the experiments for himself, there will be no difficulty in verifying my statements and checking my conclusions.

What first led me to seek a simpler explanation of the optical apparatus of the eye was my desire to see perfectly beneath the water. Being very fond of swimming, when at

the sea-side last August I attained considerable proficiency in diving. I used to swim out until I got to water of considerable depth, probably from twelve to fifteen feet, and then dive to the bottom, as I had seen done by the Neapolitan divers, and bring up from it whatever I could find. But, as all swimmers know, vision under water is very indistinct. The water may be as clear as crystal, and from the shore or over the boat's edge we may be able to see everything lying at the bottom with perfect distinctness, but as soon as our head is beneath the water our vision is reduced to the mere perception of shapeless patches of colour. I set myself to discover how this lost vision could be restored beneath the water. It was evidently the contact of water with the eye that destroyed the vision, for I found that with a pair of well-fitting goggles with plain glass in front sight was perfectly retained under water as long as the glasses kept clear, which was not long, however, as they very soon became obscured by condensed vapour. The explanation of vision under such circumstances was obvious. The eyes still remained in the same condition as when above the water; they were in contact with the air contained in the goggles. But the difficulty of making such goggles water-tight and the impossibility of keeping the glasses clear were fatal objections to their practical utility. I must find some means of restoring the vision lost by the contact of the water whilst the eyes still remained in that condition.

On comparing the eyes of fishes, whose vision beneath water must be perfect, with those of terrestrial vertebrates we find the chief differences, as regards their dioptrical apparatus, to be these:—Fishes have a flattish cornea, no anterior chamber to speak of, little aqueous humour and a spherical crystalline lens immediately behind the cornea; terrestrial vertebrates have a very convex cornea, a considerable anterior chamber filled with aqueous humour, and a double convex crystalline lens, the convexities of the two surfaces varying very considerably; the anterior surface being rather flat, the posterior very rounded. Thus, fishes may be said to have only one lens to their eye, the crystalline, of a spherical shape, while terrestrial vertebrates have

two lenses, the crystalline a double convex lens, and anterior to this a water lens formed by the aqueous humour, and bounded on the one hand by the cornea, on the other by the anterior surface of the lens. As the surface formed by the cornea is more convex than that formed by the crystalline, the form of this anterior lens is that of a meniscus, as it is termed, viz. a concavo-convex lens whose surfaces, if continued, would meet. The vitreous humour has the form of a negative concavo-convex lens, in which, namely, the surfaces if continued would not meet, but as its focus is a long way behind the eye, it cannot act as a lens at all, but is only so much water in which to suspend the crystalline.

In order to understand the following experiments it will be necessary to bear in mind certain facts relating to the refractive power of different media under different circumstances.

A transparent substance forms a lens causing the rays of light to converge, provided it is of greater refractive power than the medium through which it receives these rays and is convex in form. If in similar conditions it be concave in form it causes the rays of light to diverge. The convergence or divergence bears some fixed proportion to the comparative refractive powers of the two media. If the light-transmitting medium and the lenticular medium be of the same refractive power, no alteration in the direction of the rays of light passing through the lens takes place.*

Now for the proof of these propositions. If we take two watch-glasses and place them with their concave surfaces towards one another, or with their convex surfaces towards one another, they have no refracting power in air. If when their concave surfaces are together we fill the intermediate space with water, they will act as a magnifying glass, converging the rays of light and magnifying objects in proportion to their convexity, because the more refractive medium, the water, is thus made to assume a convex form. If we put them with their convexities towards one another and unite their

* Comparative *density* of light-transmitting medium and lens does not always determine the refractive power, for though all fluids have a greater refractive power than all gases, some solids possess less refractive power than some liquids, and liquids differ among themselves in their refractive powers quite independently of their relative densities. The following experiments

rims by a ring of metal or glass and fill the space thus formed with water, we shall find that they cause the rays to diverge and act as diminishing glasses, because the more refractive

prove this, and show also the difference of the focal distances of various transparent substances in air and water.

A glass globe three quarters of an inch in diameter filled with	Focus in air.	Focus in water.
Water	0·4 inch	None
Alcohol	0·4 „	None
Sulphuric ether	0·4 „	None
Chloroform	0·3 „	2·13 inches
Syrup	0·25 „	1·8 inch
Ol. terebinth.	0·2 „	1·5 „
Glycerine.....	0·2 „	1·5 „
Ol. olivæ	0·2 „	1·5 „
Sulphuric acid	0·2 „	1·5 „
Ol. cajuputi.....	0·2 „	1·5 „
Ol. carui	0·2 „	1·5 „
Ol. caryophyl.....	0·17 „	0·9 „
Ol. cassiæ	0·12 „	0·7 „
Carbonic sulphide	0·1 „	0·62 „

The above table has no pretensions to strict accuracy ; the focus in air of alcohol may be somewhat shorter than that of water, and that of ether somewhat longer ; and there is, probably, some difference among the six substances whose focus I have marked as the same, but to give the minute fractional differences among these substances, which would amount only to 100ths or 1000ths of an inch, would have required other instruments than those at my disposal, and would not have been of any practical use.

With the above table may be compared the following, showing the relative focal length of five solid lenses of various power in air and water.

	Focus in air.	Focus in water.
1. Double convex lens	0·7 inch	2·8 inches
2. Double convex lens	0·3 „	1·25 inch
3. Glass sphere $\frac{7}{8}$ in. diameter	0·18 „	0·85 „
4. Crystalline of ox	0·3 „	1·4 „
5. Crystalline of cod	0·12 „	0·6 „

It would appear from the above tables as if the focus of fluid lenses were lengthened about seven, and that of solid lenses between four and five times, by immersion in water. Probably this is an illusion, owing, not altogether to my imperfect measurements, but to the undue shortening of the focus of the fluids in air by the thickness of the glass globe enclosing them, the effect of which would be much less observable in water,

medium, the water, is thus made to assume a concave shape. Now, if we put either of these water lenses into water they will not refract the light one way or another; the light passes through them unrefracted, for both media are now identical.

But let us take the watch-glasses placed concavities towards one another, but filled with air, and bring them under the water. We shall now find that we thus have a lens that causes the rays of light to diverge and diminishes objects seen through it, because the more refractive medium, the water, is thus forced to assume a concave form. Then let us take the glasses placed with their convexities towards one another, and connected round their rims by a water-tight metal ring so as to enclose a portion of air. We shall now find that we have a lens that causes the rays of light to converge and acts as a magnifier, because the more refractive medium, the water, is thus found to assume a convex shape.

Let us now take a glass lens that has a focus of an inch in the air. On plunging it into water which has so much higher refractive power than air, but which is still greatly inferior in that property to glass, we shall find that the focus is increased to four inches or more, and the magnifying power diminished in the same proportion.

Again, in the case of a double convex glass lens of unequal convexities, supposing only one surface is immersed in water, the focus will be longer if the surface of the greatest convexity be immersed, not so long if the lesser convexity be immersed.

It should also be borne in mind that the focus of a convex lens is shortened by placing another convex lens over it, the shortening of the focus being proportioned to the refracting power of the added lens. Thus the focal distance of a glass lens of 0·7 inch focus is shortened to 0·4 inch by the addition of a lens of 1·3 inch focus, while a lens of 6 inches focus only shortens it to 0·6 inch.

With these data in our mind, we may now proceed to the examination of our eyes. The spherical crystalline lens of a cod has in air a focus of about 0·12 inch, in water its focus is 0·6 inch, and this we find to be the precise distance

of the posterior surface of the crystalline from the retina in this fish's eye. Hence no further lens is required in order to picture objects on the cod's retina in their proper focus.* The refractive power of the lens of a fish seems to be nearly the same as that of a glass lens.

Now let us examine the eye of a terrestrial vertebrate. I have made most of my experiments with bullocks' eyes, as they are so large that I could make the requisite measurements with the rude instruments in my possession, without fear of vitiating the results by a lack of delicacy. From an average of eyes examined, I may say that the interior antero-posterior diameter is 1.5 inch. Thickness of lens 0.5 inch, distance from posterior surface of lens to retina 0.85 inch, leaving 0.15 for the anterior chamber. The focus of the crystalline is in air 0.3 inch, in water 1.4 inch. This would throw the focus to a distance of 0.55 beyond the retina, were not this corrected by the anterior lens formed by the aqueous humour, which brings the focus again precisely to the retina. Now, in order to find what is the focus of the anterior lens, we take a glass lens with a focus of 1.4 inch, and see what strength of lens will be required to shorten its focus to 0.85 inch. This we find will be done by a lens of about 3 inches focus, which must therefore be very nearly, if not quite, the focal distance of the lens formed by the aqueous humour of the ox.

The human eye resembles that of an ox in its dioptrical apparatus, differing from it only in size. When we go beneath the water our anterior lens formed by the aqueous humour, being of the same refractive power as the water, ceases to refract the rays of light convergently, as is required for perfect vision. We are therefore reduced to our crystalline lens for refractive purposes. But as the focus of this by

* I can see no reason for supposing that a fish does not see quite as well in the air as in the water. The want of a lachrymal apparatus and eyelids will prevent its seeing well for any considerable time in the air, as its cornea must soon become obscured and hazy, but its vision when first taken out of the water may be perfectly good. Seals we know see equally well in air and water, and the dioptrical apparatus of their eyes is very similar to that of fishes. Their aqueous humour, though insignificant in quantity, is sufficient together with the vitreous humour to keep the crystalline lens bathed in a watery medium.

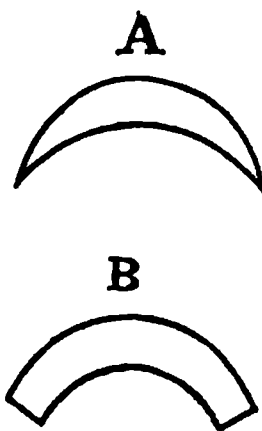
itself is beyond the eye, our vision is of the most indistinct. In order to supply the loss of our anterior lens, we must place before the eye a lens of equal focus. As I was utterly ignorant of the focal length of the anterior lens lost by immersion when I first commenced my experiments, it was only after repeated trials that I at length discovered that a glass lens of half an inch focus in air completely restored perfect vision under water. But as above pointed out, a glass lens has in water a focus four times as long, *i. e.* its refractive power is only one fourth as great as it is in air, therefore a glass lens of half an inch focus in air makes a lens of 2 inches focus in water. Hence the focus of our anterior lens is just about 2 inches. The focus of the crystalline lens will be about 0·2 inch in air and 0·8 in water. The anterior lens of 2 inches focus will reduce this focus in the eye to 0·5 in., the actual distance between the back of the lens and the retina. I have not had an opportunity of verifying these figures by the examination of a recent human eye, but I have no doubt they are a tolerable approximation to the truth. And here I should remark that while spherical aberration of the rays of light is obviated by the covering up of most of the lens by the iris, and further, by the combination of two lenses—a meniscus and a double convex lens—a contrivance well known to the constructors of telescopes, achromatism is secured by the different refractive powers of these two lenses. This in opposition to the statement of Sir D. Brewster, who says in his *Treatise on Optics*, p. 289, “No provision is made in the human eye for the correction of colour, because the deviation of the differently coloured rays is too small to produce indistinctness.”

Having thus ascertained that we required a lens of 2 inches focus in water to replace the aqueous lens we lost by immersion, I found that spectacles constructed with eye-glasses of such lenses, however clear they rendered vision below water, were of course utterly destructive of vision in the air. They were subject to this inconvenience, that they had to be adjusted to the eyes when we went below the water, and removed on coming to the surface. Besides, it seemed a clumsy expedient to have to use glasses of such great refractive

power (half an inch focus in air) in order to supply the loss of lenses of such a small degree of refractive power as the aqueous humour (2 inches focus). I therefore sought to construct glasses which should supply the required magnifying or refractive power in water, but should present no obstacle to vision in air. In accordance with the principles above laid down, this must be done by means of sections of a hollow sphere of glass arranged with their convexities towards one another, and their concavities outwards. After several trials I found that the requisite refractive power in water was obtained by means of two such sections cut from a hollow globe of $2\frac{1}{2}$ inches diameter. These placed with their concavities outwards, and united at their edges by means of a metal rim quite water-tight, formed glasses that replaced the lost anterior lens of the eye, and restored perfect vision under water. They likewise offered no obstruction to vision above water. It seemed to me the perfection of simplicity to compel the water which had taken away the vision to restore it by a new arrangement of itself. In this case the media which in the normal condition caused perfect vision, air and water, were again employed, but in an inverted manner. Various combinations of differently curved glasses besides those I have mentioned may be employed for the same purpose. Thus the concavities may be very different for both glasses, or only one concave glass may be used, the other glass being plane. Or two glasses might be so arranged that one which is deeply concave and the other slightly convex, are exposed to the water. Further experience will teach which is the best of these forms for subaqueous spectacles.

I have shown that the refractive power of the anterior lens formed by the aqueous humour of the human eye is equal to a lens of 2 inches focus. This lens is of meniscus shape, formed by the outer surface of the cornea as its anterior convex surface, the anterior surface of the crystalline forming its posterior concave surface. According to Donders the radius of curvature of the cornea is and remains 8 mm., whilst the radius of curvature of the anterior surface of the lens when accommodated for distance is 10

mm. These figures would give a meniscus of the subjoined form A. When the eye is accommodated for near objects, according to the same authority, the cornea retains its radius of curvature 8 mm., while the crystalline acquires a more convex form, its radius of curvature being then 6 mm. This would give a negative concavo-convex lens of the subjoined form B, which would be incapable of refracting the rays of light convergently, in fact, would refract them divergently according to the well-known properties of the negative concavo-convex lens. But in that case the crystalline lens would be the only agent left for producing convergence, and we have seen above that the focus of this lens suspended in an aqueous medium is considerably beyond the retina. Moreover, if these figures are correct we should be able to accommodate our eye with facility to near objects below the water, which, however, we cannot do, as I have experimentally found that we need a lens of 2 inches focus in water for near as well as distant sight below the water. There must therefore be some error in Donders'* figures, and I shall endeavour to show the probable source of that error.

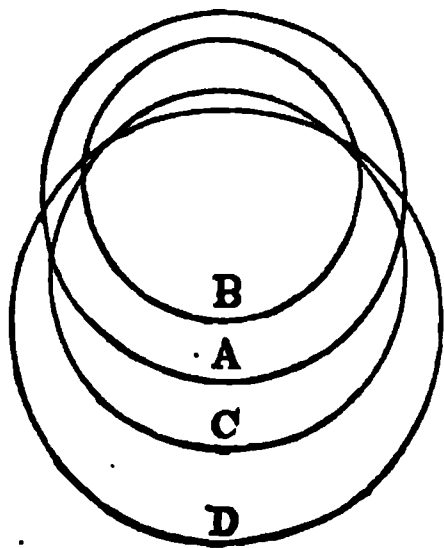


I believe there is first of all an error in the radius of curvature ascribed to the anterior surface of the lens when accommodated for distance, that is to say, in a state of rest.

As I had it not in my power to investigate this point on the eyes of human subjects, I had to content myself with bullocks' eyes, of which I could procure any number, and which from their size were better adapted for my experiments than smaller eyes, as I have no instruments that would enable me to take very minute measurements. I took care to detach the lens enclosed in its capsule, for the outer

* I take Donders to be the exponent of the most generally received opinions respecting the refractive apparatus of the eye. I find a great difference in the calculations of older authors on the subject. Brewster, Lardner, and Mackenzie all differ from Donders and from each other, so I suspect the measurements even of Donders, although he wishes them (p. 27) to be received as "exact science," may still be open to correction.

layers of the lens are easily broken and bruised though handled ever so gently, whereas the shape is well preserved if the capsule remains entire. I was at once struck by the shape of the anterior surface of the crystalline when viewed in profile. It is evidently not a section of any possible sphere, for it is much flatter towards the centre than towards the sides. By means of plaster of Paris casts of the various parts, I was enabled to make measurements of their different radii of curvature. I found that the radius of curvature of the cornea was 12·5 mm.,* but I should say that the actual convexity of the living cornea must be greater than this, for in all my eyes the cornea was very lax and was sunk in as it always is in the dead animal, and especially in the eye removed from the body, whether owing to absorption of aqueous humour, or the loss of pressure on the ball by the muscles of the eye I am unable to say, probably from both causes combined. I should therefore be inclined to put down the radius of curvature of the living bullock's eye at about 11·5 mm., though in the figure (A) I have drawn it as 12 mm. The radius of cur-



vature of the *posterior* surface of the lens I found to be 9 mm. (B). As to the *anterior* surface of the lens, I found that the convexity of the two external fourths had a radius of 11·5 mm. (C), while the two centre fourths of its surface had a radius of somewhat more than 14 mm. (D). As it is the central portion of the crystalline that

is alone concerned with the refraction of light in vision, the outer portion being covered by the opaque iris, it is the degree of convexity of the former that alone concerns us.

The fact, which my casts and measurements enable me to prove, that the anterior surface of the lens† has not the

* I preferred making my measurements in millimetres, as besides that the millimetre is a more minute subdivision of space than the tenth of an inch, it is the measure employed by Donders, with which mine may therefore be easily compared.

† My measurements were, to be sure, made on bullocks' eyes, and it may be

form of a segment of a sphere, but that its external portion has a smaller radius of curvature than its centre, is probably the reason why authors differ so much among themselves as to the actual convexity of this surface. Thus Brewster (*Optics*, p. 288) says the radius of curvature is 8 mm. (for uniformity's sake I give his measurements in millimetres), while Lardner (*Museum of Science*, v, p. 53) says it varies from 8 to 10 mm. Donders, we have seen, puts it at 10 mm. for the unaccommodated eye. All these authors agree in giving the posterior surface a convexity of 5·5 to 6 mm. radius.*

And now I come to the change said by Cramer, Helmholtz, Donders, and others, to occur in the convexity of the anterior surface of the crystalline in accommodation for near objects. I have shown above that the increase of convexity cannot be as much as stated by the authors, viz., to a radius of curvature of 6 mm., for in that case our anterior lens would be changed from a meniscus to a negative concavo-convex lens dispersing in place of converging the rays of light; and the condition of vision beneath water shows this not to be so.

The very wood-cuts given by Donders at p. 13 prove that the convexity of the anterior surface of the crystalline lens accommodated for near objects is less than that of the cornea. The image reflected by convex surfaces is larger in the inverse proportion of their convexity. In the figure A there given the image of the candle reflected by the unaccommodated anterior lens is twice the breadth of the same image reflected by the cornea. And in figure B which

objected that the human crystalline may be differently constructed. But I believe that no difference will be found in this respect except as regards size, and the carefully drawn figures of Donders bear me out in this view.

* Mackenzie (*Diseases of the Eye*, p. xxi) makes the radius of curvature of the cornea 9 mm., that of the anterior surface of the crystalline 9·5 mm., and that of the posterior surface of the crystalline 6 mm. By the way, Mackenzie talks great nonsense about the shape of the aqueous humour when he says that in its anterior chamber, i.e. before the iris, it is a plano-convex lens, while in its posterior chamber it is plano-concave. He does not seem to be aware that its boundaries as a lens are, in front, the cornea, and behind, that portion of the anterior surface of the crystalline lens that lies within the pupil.

represents the eye accommodated for near objects, the image reflected by the anterior lens is still larger than that from the cornea by one third.

It is true that the image reflected from the anterior surface of the crystalline, being seen through the aqueous humour (a magnifying lens) is slightly increased in size. But besides that this increase is insignificant, it would not be increased at all, but, on the contrary, diminished, were the convexity of the cornea less than that of the anterior surface of the crystalline. This I proved as follows :—

I took a segment of a solid glass globe with a radius of curvature of about three inches. Over this I placed a watch-glass with a somewhat smaller radius of curvature. The image of a candle was reflected from both these surfaces with equal brilliancy, that reflected from the more convex surface, the watch-glass, being the smaller. I now filled the space between watch-glass and solid sphere with water, thus forming an aqueous meniscus on the solid crystal globe in the same way as is done in the eye. I found that the image reflected from the solid globe not only lost in brilliancy, but was perceptibly larger than before, that from the watch-glass remaining unaltered in size. I next laid over the segment of the glass sphere a watch-glass of smaller convexity than itself; in this case, both images being brilliant, the larger image was the outer, the smaller the inner. I then, by means of a rim round the outside of the watch-glass, filled the space between it and the sphere with water, and found that the deeper image, besides being dimmer, was actually perceptibly smaller than before. I should observe, also, that the less the convexity of the surface the greater is the mobility of the reflected image when either the lenses themselves are moved or the light is moved.

It follows from these experiments that if the reflected image of the candle from the anterior surface of the crystalline, even when accommodated for near vision, still remains distinctly larger than the image reflected by the cornea, the convexity of the former must still remain considerably less than that of the cornea. This, I think, requires no further demonstration. But if I might hazard a speculation

as to why Donders and others have fallen into the mistake of assigning a greater convexity to the anterior surface of the lens than it can possibly have, I would say that, 1st, they have been deceived in their measurements of that surface, by looking at it as a segment of a sphere, whereas I have found that it is flatter in the middle than at the sides; 2nd, they have lost sight of the fact that in the reflection of an image from convex surfaces, the greater the convexity the smaller will be the image; and 3rdly, they have failed to remark that the mobility of the image reflected is great in the inverse proportion of the convexity of the reflecting surface, so that a very small change of inclination on such a flat surface as the anterior surface of the crystalline within the pupil is, will produce a great movement of the reflected image, and the superposition of a water meniscus, like the aqueous humour, increases the mobility.*

I do not at all deny that the accommodation for near objects is effected, as Donders says, by the changes in the convexity of the crystalline lens, and especially of its anterior surface. But I think I have proved that this change is not anything so great as Donders alleges, and that the anterior surface of the crystalline never attains to the convexity of the cornea. But it is not clearly made out to me that the increased convexity of the anterior surface of the crystalline is the only possible agent in effecting accommodation; for though Donders says that he has satisfied himself that "when the crystalline lens is absent, not the slightest trace of accommodating power remains" (p. 16), there is the testimony, on the other hand, of Sir Everard Home (quoted by Mayo in his *Physiology*), who gave an

* On moving the flame of a candle, held before the eye in the ordinary way when examining for cataract, horizontally to and fro, we shall observe the image reflected from the anterior surface of the crystalline lens to be the most mobile, that from its posterior surface the least mobile, and that from the cornea to be intermediate in mobility between the two, but decidedly less so than that from the anterior surface of the crystalline. The sizes of the images also will be seen to vary in the inverse ratio of the convexity of the surfaces they are reflected from. The reflection from the posterior surface of the crystalline is the smallest, that from the cornea next in size, and that from the anterior surface of the crystalline is the largest.

account in the *Philosophical Transactions* of a man whose crystalline had been extracted, and who yet possessed the perfect power of adjusting his eyes to near or distant objects. This adjustment is conceivable by pressure of some of the muscles of the eye causing increased convexity of the cornea. But we have, as yet, no data for deciding this point one way or another.

After the operation for cataract by extraction vision is restored by the employment of a glass lens with a focus from $2\frac{1}{2}$ to $4\frac{1}{2}$ inches. But, as we have seen above, the crystalline lens that has been taken from the eye has a focus of only 0·2 inch in air and only 0·8 in the watery medium in which it is suspended in the eye. How, then, are we to account for a lens of such low refractive power as $2\frac{1}{2}$ to $4\frac{1}{2}$ inches focus being able to replace a lens of such a high refractive power as 0·8 inch focus? The explanation I conceive to be the following. By the extraction of the crystalline the dioptrical conditions of the eye are completely altered. The eye is no longer a combination of lenses of different densities. It has become a spherical water lens, composed of the united aqueous and vitreous humours, whose densities are identical, bulging in front into a more convex shape by means of the cornea, which will have the effect of a meniscus of about 2 inches focus placed in front of a sphere of water. Now, a spherical lens made of a thin glass globe of 1 inch diameter filled with water, which will roughly represent the human eye deprived of its crystalline, has a focus of about half an inch. If we place over this a lens of 2 inches focus, to represent the bulging cornea, we shall find the focus shortened to less than a quarter of an inch, and it will only require another lens of from 3 to 4 inches focus to bring the focus quite up to the surface of the spherical water lens. This experiment will explain what takes place in the eye deprived of its crystalline lens by operation.

It would be interesting to ascertain what form of lens would be required to restore vision below water in a person whose crystalline lens had been extracted. As the water would at once destroy all the refractive power of the denuded eye, an artificial lens of about half an inch focus

in water would probably be required. This would be equivalent to a glass lens of one eighth inch focus in air, quite a microscopic object-glass, or an air lens such as I have described with two concavities, having each a very small radius of curvature.

MERCURIC METHIDE AND CHLORAL AS NEW MENTAL MEDICINES.

By EDWARD T. BLAKE, M.B.

Not long since, men of science were startled by the fact, that there existed, in the laboratory of the chemist, a volatile agent, so potent in its disease-producing power, so deadly in its inherent properties, that its mere inhalation had proved fatal; and that, not by simple suspension of the vital processes, but by a lingering, yet terribly certain, wearing-out of the powers of life, attended by the sad train of phenomena which constitute insanity, so that the hideous history of the celebrated *Aqua Tofana* is nearly eclipsed by this modern horror.

On the 3rd day of February, and the 25th day of March respectively, there were borne into the clinical wards of St. Bartholomew's Hospital, two unfortunate victims of the researches of modern science. Neither of these men left the wards alive; the former was carried to the dead-house on the 14th day of February, only eleven days after his admission; the existence of the latter was miserably prolonged through another tedious year, when death came as a relief to close a wretched existence, for through all this time this unfortunate man was completely fatuous, not even recognising his nearest relations. Both these men had been exposed to the fumes of Mercuric methide; the former had been for three months directly engaged in its preparation. Let us hope that these poor fellows have not been immolated in vain.

In the first volume of the *Bartholomew's Hospital Reports*, article viii, p. 141, there is an exceedingly interesting *résumé* of these remarkable cases. For the sake of those

who are unable to consult the original source, I present them in a still more condensed form, and to make the whole intelligible at a glance I will, at the sacrifice of their true pathological sequence, throw the phenomena into three groups.

The 1st group consisting of those symptoms, &c., which appeared in both instances.

The 2nd group, symptoms of first or fatal case.

The 3rd group, symptoms of second case.

GROUP 1.—*Symptoms common to both.**

Hydrargyrosis; debility; hebetude; enfeebled circulation; sensation and muscular sense modified; impairment of speech, and of special senses, particularly that of *hearing*; dull or vacant expression; sight impaired; pupils dilated; numb hands and cold feet; clonic spasm; dementia.

GROUP 2.—C. U—, æt. 30.

Syphilitic and Epileptic.

Deafness; speech indistinct; answers "yes" to all questions; tongue yellow.

Numbness of hands, with

GROUP 3.—T. S—, æt. 23.

Strumous.

Emaciation; slight jaundice; skin sometimes cold on one side and warm on the other; cheeks alternately flushed and pale; dark ring round eyes; pain and redness of conjunctivæ; impairment of special senses, *particularly hearing*; tongue numb, white fur, fissures in centre; thirst at first, then brassy taste; dysphagia; giddiness; nausea; green and watery vomiting; faint for twenty minutes, with snoring; tenderness in right hypochondrium;

* [Dr. Odling tells me that one of these men was *not* actually engaged in the manufacture of the drug, but merely stood in the same room. Dr. Odling was, himself, frequently in the laboratory, occupied in arranging materials for the assistants, but he never experienced any ill effects.—E. T. B.]

C. U.

supervening motor paralysis; feet cold *subjectively*.

Movement of limbs impeded, but sensation usually unimpaired.

Kidneys.—Renal epithelium, granules; albuminuria; urine passed under him.

Restless at night; dull, deaf and drowsy by day.

When this patient had been six days in the wards, he became so restless and noisy, during the night, that his hands had to be strapped to the bedstead.

Feb. 9th.—This morning his countenance is pale and sunken; lips covered with sordes, and breath very foetid; tongue dry and covered with a yellowish fur; bowels not open since the 7th; urine passed in bed and very offensive. He lies muttering incoherently, refuses to take his nourishment, and struggles and becomes furious upon any attempt being made to force him to drink.

10th.—Was again very noisy during the night, and this morning lies for a time quiet in a semi-comatose condition, but at intervals raises himself suddenly and utters incoherent howls. He appears to move well and

T. S.

numbness of hands and feet; feet cold *objectively*; great toes slightly retracted; can move limbs perfectly, but slowly; grasps imperfectly; drags legs in walking.

Impaired muscular correlation; progressive motor paralysis.

Kidneys.—Triple phosphate; urine alkaline, passed under him.

When the patient had been in the hospital a month, having grown progressively weaker, passing alkaline urine unconsciously, the report was as follows:

April 27th.—For the last day or two he has been much more restless, at times even violent, shrieking out, making loud incoherent noises, or laughing or crying in an idiotic manner. Occasionally makes efforts to get out of bed. He then becomes quiet, and lies still on the bed, generally with his legs drawn up. When his feet are touched the violent movements of the limbs immediately commence. He is as deaf as ever, but appears quite conscious, making frequent efforts to speak. *His movements resemble in a marked manner those observed in the former patient.* Skin warm and natural generally, cold over the feet; breath still

C. U.

to have no paralysis of either side; his expression is very vacant, and pupils dilated; the odour of his breath and body is still very offensive.

In consequence of his noise, which disturbed the other patients, he was removed to a separate ward.

11th.—Slept at intervals during the night; still very restless, and is strapped to the bed, but is, perhaps, rather more quiet on the whole, although he attempts to start up in bed and utters incoherent noises at frequent intervals. His countenance is more sunken, pale, and somewhat dusky; respiration is peculiar, his breathings sometimes appearing to cease altogether for a few seconds, and then to become quick and stertorous; pupils at one time contracted, at another dilated, act equally; breath very offensive; pulse 80, small; lips and teeth dry, covered with sordes; bowels still not open; urine passed beneath him; when he attempts to sit up he stares vacantly about him, and now does not move the left side. The left wrist is rather rigid, and the left knee almost completely so; he never bends it voluntarily, but it can be slightly bent by using considerable force.

T. S.

very offensive; tongue slightly coated, moist, and more easily protruded; pulse sometimes very feeble, at others of fair volume, about 100, irregular bowels more freely open; motions and urine passed beneath him; occasionally refuses food, and swallows with some difficulty.

May 12th.—He has been gradually becoming weaker and thinner since last report; swallows with greater difficulty, and refuses his food. His expression is quite idiotic, although he appears sometimes to recognise persons around him. The violent movements of his limbs continue, and are much increased on tickling his feet. He is still very deaf, makes constant efforts to speak, but only utters incoherent noises. For the last few days he has had some inflammation of both conjunctivæ, with a muco-purulent discharge; breath is still offensive; gums slightly spongy and swollen; passes all his evacuations beneath him. The skin over the sacrum is red but not broken.

June 4th.—His symptoms have very little, if at all, altered for some time past. He is getting weaker and thinner, and does not know any one now. Is, perhaps, rather less

C. U.

12th.—Slept very little during the night; countenance pale and sunken; eyes suffused; pupils dilated; lips and teeth covered with sordes; lies with eyes and mouth half open; is evidently weaker; struggles much less, but still moans at intervals. There is no sensation or motion in the left leg, which is extended, rigid at the knee, with the foot turned slightly inwards; sensation is also somewhat impaired in the left hand; breath continues very foetid; pulse 90, very feeble; bowels still not open.

13th.—Had again a little sleep during the night; countenance flushed; expression a little more natural; pupils more widely dilated; breathing stertorous. There is some reflex movement of the left leg upon irritation.

14th.—Passed a restless night; continued in the same state of complete insensibility till is death, at 11.30 a.m. to-day.

T. S.

violent, although he frequently throws his limbs about violently, laughing, crying, or howling incoherently. The conjunctival inflammation has somewhat diminished. Skin natural; pulse 84, fair volume, but slightly irregular; tongue not protruded; bowels generally confined, have been acted upon several times by injections; takes nourishment only with great difficulty.

July 4th.—Since last note was taken he has continued much in the same state, except that he now takes his food well, and also takes his oil, and has gained flesh slightly. He is, however, quite idiotic, recognises no one, is deaf, and unable to speak any words, but still mutters, cries out, or laughs, and is frequently violent, and his limbs move convulsively; sometimes sleeps well, but is often restless at night; his back is not now sore; skin natural; pulse 84, feeble; bowels still generally confined; motions and urine always passed involuntarily. He remained in nearly the same condition until within a few days of his death, which did not take place until April 7th, 1866. During this long interval he occasionally gained flesh slightly for a time, but soon again relapsed. He

T. S.

was quite idiotic, and recognised no one. The cause of death was pleuro-pneumonia of the left lung. The autopsy did not account in any way for the symptoms observed during his protracted illness.*

Post-mortem.—Brain healthy except congestion of the grey matter; kidneys and liver congested.

Thanks to expectancy, there was here very little heroic drugging; the symptoms are but slightly obscured, so that we have two excellent provings of a drug that bids fair to be of priceless value in the treatment of certain forms of insanity.

The two cases coincide too closely to permit us to doubt the relation between the morbid condition and the inhalation of the deleterious vapour to which these men had been exposed. Added to this, symptoms of the same character, although less in degree, were experienced by almost all those working in the laboratory at the time.

Then, again, the production of insanity by means of mercurials is by no means a new idea. Hughes says,† “*Mercury* has more *neurotic* influence than it is commonly credited with. The mercurial tremor‡ is as characteristic an action of this drug as its eczema and salivation, and I cannot understand this as anything but an affection of the motor nerves. The mental condition described by Dr. Wood as accompanying mercurialization is probably secondary,§ and

* The latter part of the history of T. S— will not be found in vol. i of the *St. Bartholomew's Hospital Reports*, as that was published prior to his death, but may be seen in the *Biennial Retrospect of the Syd. Soc.* for 1865–6, at p. 439.

† *Pharmacodynamics*, 1st ed., pp. 391—401.

‡ See a case of *tremor manûs* of more than ten years' standing treated successfully by *Merc. sol.* 6x, reported by me in the *M. H. Review*, vol. xiii, p. 347. [E. T. B.]

§ “The most prominent nervous phenomenon of mercurialization is an increased susceptibility to impressions, slight causes producing a disturbance of the mental equanimity, and unpleasant influences of all kinds having more than their ordinary effect. A fretful, peevish state of mind and irritable condition of temper are not uncommon, and restlessness, wakefulness, and general uneasiness are frequently added to the other sufferings.”—*Mat. Med.*, vol. ii, p. 236.

so, perhaps, is the wakefulness by night, and sleepiness by day complained of by most of the provers of *Cinnabar*. But the workers in *Mercury* get cerebral symptoms as idiopathic as is the mercurial tremor.* Sleeplessness, loss of memory, delirium, and apathy come on; the sufferers may die comatose and hemiplegic, and the cerebral hemispheres, one or both, are found after death in a softened condition, with effusion into the ventricles.†”

On the other hand, it must be borne in mind that the above are not merely provings of *Mercury*, although that metal forms nearly 90 per cent. of the methide.‡

The question naturally arises, to what species of insanity should the two preceding cases be properly relegated?

Professor Guy, of King's College, appears to consider them as instances of CHRONIC DEMENTIA,§ whilst Dr. Edwards, the author of the paper in *The Reports*, thinks that the *post-mortem* results point in the direction of ACUTE MANIA. I do not think that either of these views is quite correct; the symptoms present rather the leading features of ACUTE

* Romberg in his *Diseases of the Nervous System* relates that the German mirror-makers are subject to tremor resembling paralysis agitans. Dr. Richard Hughes suggests the use of *Mercury* for paralysis agitans in the *British Journal of Homœopathy*, Vol. XXVII, p. 23, in the most classic and complete paper we possess on the subject of paralysis.

† Jahr gives—“*Great anguish, restlessness, and agitation, with fear of losing the reason, or with excessive internal torment, principally in the evening, or in bed at night, as if conscious of having committed some crime. *Moral dejection, with great listlessness, discouragement, dread of labour, and disgust of life. Apprehensions. Ill-humour, disposition to be angry, and to fly into a passion, great susceptibility, humour quarrelsome, mistrustful, and suspicious. *Moroseness, and repugnance to conversation. *Groans. Excitement, and great irritability, with a tendency to be easily frightened. Distraction, inattention, difficulty of conception. *Entire unfitness for meditation, and tendency to make mistakes while speaking. Weakness of memory. Instability of ideas, which constantly drive away each other. Raving. MANIA or DEMENTIA, with disposition to shed tears. Loss of consciousness and of speech. Fury, with dread of liquids.”—*Mercurius*, vol. i.

‡ Mercury itself has been administered to patients who have been supposed to be suffering from “peri-encephalitis,” whatever that may mean, and in cases of syphilitic paralysis, with mental symptoms similar to those of “general paralysis of the insane.”

§ Vide *Forensic Medicine*, last edition, p. 182.

DEMENTIA, *associated with mercurial paralysis.* In any case, our business, as physicians, guided in our practice by the great truth expounded by the immortal Hahnemann, is not so much to decide what name may be attached to such and such a group of symptoms, but with eagerness to gather and store up such pathogenetic treasures as the above, knowing of a certainty that, what time the true pathological counterpart shall present itself in our practice, we shall be forearmed with a weapon, potent for good.

No cases illustrative of the clinical value of the methide can be given, because, in spite of many efforts, I have been hitherto unsuccessful in my endeavour to obtain a specimen of it; when I succeed in doing so, I shall have ample opportunities of seeing it tested.

Dr. Odling informs me that M. Cahours, jun., is at the present time prosecuting researches on the therapeutic value of this substance at Paris; when that city shall cease to be in a state of siege we may, perhaps, hear something of the results of his labours.

Dr. Odling gives the following account of the chemistry of this compound:—“*Mercuric Methide* and *Ethide* may be regarded as varieties of corrosive sublimate, HgCl_2 , in which both atoms of chlorine are replaced by the alcohol radicles methyl and ethyl, as shown by their formulæ, HgMe_2 , and HgEt_2 , respectively.

“Intermediate compounds are also known in which only one of the atoms of chlorine is replaced, namely, mercuric chloromethide, HgClMe , and mercuric chlorethide, HgClEt . The methide and ethide were discovered by Buckton, but are usually made by the process of Frankland and Duppa, which consists in acting upon sodium amalgam with the iodides of methyl and ethyl under special conditions, whereby the iodine combines with the sodium, and the ethyl or methyl with the mercury. They are both colourless, heavy, inflammable liquids of a mawkish ethereal smell and taste. The smell of their vapours, when diffused into the atmosphere, is, however, extremely nauseous. Mercuric methide

contains 87 per cent. of weight of metallic mercury, and is so heavy that a piece of lead glass floats on it readily. Its sp. gr. is 3.07.

"It boils at 93° , or seven degrees Centigrade below the boiling-point of water. Mercuric ethide has a sp. gr. of 2.46, and boils at 159° C."

Watts gives a very good account of the substance and its preparation :

" $\text{Hhg}(\text{CH}^3)^2$ is the formula of *Mercuric Methide*, *Mercuric Methyl*, *Mercuromethyl*, or *Hydrargomethyl*.

"Discovered by Buckton (*Proc. Roy. Soc.*, ix, 91), who obtained it by distilling mercuric iodomethide with cyanide of potassium, hydrate of potassium, or lime. The best product is obtained with the cyanide. A mixture of this salt with mercuric iodomethide, after being well triturated in a mortar, is distilled by small portions over a lamp. Gaseous products are then evolved, iodide of potassium is formed, cyanogen is set free and remains in the form of paracyanogen, and mercuric methide passes over in the form of a heavy liquid, which may be purified by washing with water and rectification over chloride of calcium. The essential part of the reaction is represented by the equation—



"Frankland and Duppa prepare mercuric methide by the action of sodium-amalgam on iodide of methyl, in presence of acetic ether. The mode of preparation is exactly the same as for the preparation of mercuric ethide, a small Liebig's condenser being, however, attached to the neck of the flask, to arrest the vapour of iodide of methyl, which would otherwise be carried away by the escaping gas. At the end of the operation the residues in the flasks are mixed with water and distilled in an oil-bath, the temperature of which need not be raised above 110° , and the ethereal distillate, after separation from the water which accompanies it, is agitated with alcoholic potash to remove acetic ether, and finally purified by washing with water.

"Mercuric methide is a colourless, strongly refracting

liquid, having a faint and somewhat mawkish taste. Specific gravity 3.069; boiling-point between 93° and 96°; vapour density, obs. = 8.29, calc. = 7.97. It is insoluble in *water*, very soluble in *alcohol* and in *ether*, dissolves *phosphorus*, *caoutchouc*, and *resins* easily, *sulphur* in small quantity.

“Mercuric methide is very inflammable, and burns with a bright flame, giving off vapour of mercury. It does not unite directly with *oxygen*, *chlorine*, or other electro-negative elements, but is completely decomposed by them. *Iodine* and *bromine* act upon it very energetically, eliminating methyl gas, and forming mercuric-, iodo-, or bromomethide:



“It also forms mercuric iodomethide when heated with *mercuric iodide*.

“With *stannic chloride* it forms a crystalline compound, which is decomposed by water, with formation of mercuric chloromethide and a soluble tin salt. With strong *sulphuric* or *hydrochloric acid* it acts like mercuric ethide, giving off marsh gas, and forming crystals of mercuric chloromethide or sulphatomethide. With *dilute acids* and metallic *zinc* it yields metallic mercury and gaseous products; its salts (mercuric iodomethide, &c.) are decomposed in like manner (Buckton). Heated with metallic *zinc* alone, it yields zinc-ethyl and zinc-amalgam (Frankland and Duppa). With *trichloride of phosphorus* it forms mercuric chloromethide.

“*Mercuric Nitromethide*, $\text{Hhg}'' \left\{ \begin{array}{l} \text{CH}^3 \\ \text{NH}^3 \end{array} \right.$ Nitrate of Hydrargomethyl.—Obtained by treating an alcoholic solution of mercuric iodomethide with nitrate of silver, and evaporating the filtrate *in vacuo*. Crystalline in nacreous laminæ, containing $2\text{Hhg}(\text{CH}^3)\text{NO}^3\text{H}'\text{O}$, very soluble in *water*, sparingly in *alcohol*. At 100° it melts to a colourless liquid, which solidifies in a crystalline mass on cooling. Its solution is not precipitated by *potash* or *baryta-water*. *Hydrochloric acid* and soluble *chlorides* precipitate mercuric chloromethide in nacreous laminæ (Strecker, *Ann. Ch. Pharm.*, xcii, 79).

“*Mercuric Oxymethide* or *Oxide of Hydrargomethyl* is produced by the action of the ammonia on the fixed alkalies or mercuric iodomethide. It is dissolved by an excess of either of these reagents, and the solutions yield with sulphide of ammonium a flocculent precipitate of the corresponding *sulphide*, having a faint yellowish colour and a peculiar and most intolerable odour (Frankland).”—Watts’ *Dict. of Chem.*, vol. iii, p. 297 ; Longman, 1865.

It seems probable that in CHLORAL, another of the more recently discovered chemical compounds, we have a remedy for some of the manifestations of lunacy, but as yet our knowledge of the specific effects of this drug is extremely limited.

APHASIA.

By Dr. C. B. KER.

THE two following cases illustrate a disease or the symptoms of a disease, which, both in this country and on the Continent, has attracted much attention, has become a subject of great speculative interest, and, like every such subject, one of controversial contest in academies, college theses, and in the pages of the medical press. I give them not because they prove the view of the question which I believe to be the right one. They cannot be said to prove much, not only because they are only two cases, and no more, but still more because in neither did post-mortem examination verify the symptoms or the opinions founded upon them, in the one case because, happily, the patient still lives, in the other case because no examination was allowed after death. I give them for the purpose of making some remarks on the disease itself, and on the history and present state of the controversy to which it has given rise. That controversy, it will be seen, has closed no question connected with the subject. The fierce debates in the

French Academy of Medicine have resulted in drawn battles, and the medical journals of this country have arrived at no absolute conclusion, or, if a journal or an author has attained to a conclusion satisfactory to itself or himself, another author or journal soon has proved the so-called facts and reasonings brought forward to be unworthy of any credit whatever.

On the 12th of December, 1866, I was sent for to see Miss C—, a lady 56 years of age, and who had been my patient for about five years. About two years before she came under my care she had suffered from a severe attack of rheumatic fever. The symptoms she complained of during the five years generally had reference to the head, such as fulness and weight there, especially on lying down, stiffness at the nape, and occasional deafness.

I found her suffering from an attack of right hemiplegia with aphasia. The power of the right leg and arm was gone, and though she could protrude her tongue easily and straight out, and could utter some words, those words were unintelligible. She made efforts to speak, could speak, but failed to give to her words any meaning whatever, which evidently distressed her greatly. She knew what she had to say but could not say it. There was quick pulse and a flushed face. The intelligence and the sight were both good, and the urine pale. I found that she had slept well, but had complained of giddiness on the previous day. *Aconite* 3 was prescribed, and, when I saw her seven hours later, I found that a slight difficulty of swallowing which she manifested in the morning had disappeared, that the pulse was still fast and the face flushed, the urine higher coloured, the loss of power of leg and arm more decided, and the utterance even less distinct. There was some excitement, a moist skin, thirst, and sinking.

On the next day there was no change for the better in any of her symptoms.

On the 13th there was slight dysphagia again, and the hemiplegia and aphasia unchanged. Sleep was good, appetite fair, and skin moist; the urine cloudy and high coloured; the pulse quieter, but there was still some

excitement; the bowels moved naturally, and the stool dark in colour and copious. She was alternately cheerful and depressed, and complained of no pain. The *Aconite* was continued.

On the 14th the utterance was less confused, but in other respects there was no change, nor for four days more was there any change to register. *Nux vomica* 3 was substituted for *Aconite*, and on the 18th vertigo was complained of and a good deal of irritability; the tongue was furred, and protruded slightly to the right, and the right angle of the mouth was depressed a little; the bowels were costive and the urine pale and abundant; speaks more easily and some words which she wishes to express.

Three days later my notes show that she was expressing herself intelligibly, that the power of the right leg was returning, but not of the right arm. The medicine was changed to *Zincum*.

With slight fluctuations the case went on to the middle of January, when she walked fairly, dragging, however, the right leg a good deal. The utterance was much improved and she could read, though blundering over every tenth or twelfth word, and speak so as to be understood by those addressed. From that time to the present she has enjoyed pretty good health, and when I saw her a few weeks ago I found that she had no difficulty whatever in speaking or reading aloud, but that she still dragged her right leg, and still was almost wholly deprived of the use of her right hand and arm.

The second case is that of Mr. B—, a gentleman, aged 65, I was sent for to see at nine o'clock in the evening of the 15th of December, 1869. He had been my patient for more than two years and a half for gouty symptoms. For three or four months previously to the above date I had attended him pretty frequently for cardiac symptoms of a distressing character—very sharp pain extending to the left shoulder and down the arm, tumultuous palpitation of the heart, faintness, dyspnoea and vertigo. He scarcely ever was free from pains in the knees or feet or hands, and most of the joints of the extremities were swollen and distorted.

I found him suffering from a slight attack of right hemiplegia with aphasia. He found difficulty in moving the right arm and leg. He was quite conscious, and his pulse was regular for the first time for three months, full, and not fast. He in vain endeavours to speak, but succeeds only in making unintelligible sounds. There is no expression of distress or of want of intelligence in the face, and he had been unusually well during the day, and had eaten his meal heartily. I prescribed *Nux vomica* 3.

On the following morning I found that he had slept a little. He swallows easily, moves his tongue in his mouth without any difficulty, but does not protrude it readily. The pulse is still regular, and natural otherwise. The urine is very acid, strongly smelling, and deposits lateritious sediment. He managed with a great struggle to utter the word *Yes*, and, having proved to himself that he was not speechless, he pronounced it again, and in answer to every question addressed to him, whether it was the fitting answer or not.

On the third day, the 17th, he was able to protrude his tongue easily, and all signs of hemiplegia had disappeared. He has had, however, a fit of weeping this morning. He shows no sign of distress and eats well. He has added the word *No* to his vocabulary, but his *Yes* and *No* are not always given in their proper places when questions are asked him. When he is wrong, however, he shows himself to be aware that he is so by shaking his head and looking provoked. When slightly excited to-day he hummed an air and even whistled a little, which he did well and correctly as far as the tune was concerned. He tried to express his wishes by means of ivory letters, but failed to do so, the combinations of letters he put together being quite without meaning; that they were so to himself as well as to me he showed by pushing them away from him with a gesture of impatience.

On the next day, the 18th, he showed unmistakeably by his manner and by signs that his *Yes* and *No*, which were still the only words he could utter, did not always convey his meaning, though he persisted in still using them.

Appeared happy when he was understood. He impressed those around with the idea that he knew perfectly well what to say but still could not say it. That he could speak was manifest enough. There was no aphonia; but he appeared to be like a child who makes ineffectual attempts to express its wish from ignorance of the proper language to make use of. That he knew his own mind was evident from the fact that when something other than he meant was suggested he shook his head in a most decided fashion, and indicated assent as decidedly when what he meant was found out.

On the 19th, though, in answer to questions put, he could say nothing more than *Yes* and *No*, he was able to repeat a line of a familiar psalm. On this and a few of the following days there was slight hæmoptysis with cough, but the general health steadily improved. He ate and slept well, was cheerful, had regular and natural evacuations from the bowels and bladder, and daily gained in strength.

On the 22nd he saluted me with "Good morning," and I then asked him to tell me his name; but he in vain tried to do so. He succeeded in giving me the first three letters of seven which constituted that name. It fretted him greatly that he failed in this.

On the 23rd I put a pencil in his hand and asked him to copy out a verse from the New Testament: he succeeded in doing so only partially. On the next day he was chilly, had some cough and cardiac pain, and quicker pulse and a loaded tongue; but he soon rallied from those symptoms.

On the 25th he read the Lord's Prayer to me with tolerable ease and correctness, but a muffled utterance: the letter *S* he found some difficulty in pronouncing. Next day, for the first time since the seizure, the pulse got irregular again. From this date there was not much increase of strength, and the tongue remained loaded. He still ate and slept well, however, and he gained some new words nearly every day, though he never got so far as to speak out a whole sentence: he often read one without blundering, however.

On the 29th he read the Lord's Prayer with a clear

utterance and without a mistake till he came to *For Thine is the kingdom*: there he broke down and read no more. On this day he wrote his own name and mine correctly on a piece of paper. He sometimes fails in a first attempt to read or say something, but succeeds in a second or third, and happy he is when his perseverance is so rewarded. His sight has been good from the first and the eyes normal, and no headache complained of. There have been signs of gravel in the urine once or twice, and intercostal pains on the right side have been complained of.

With little change of symptoms he went on to January 7th, on which day my notes show that his pulse was steadier and more regular. I asked him to count up to twenty, but he broke down at ten, and to repeat the alphabet, when he got no further than F. On the next day, however, he counted up to thirteen. He does not read better than he did. When he trusts to his memory he does not say the Lord's prayer so well as when he looks at the book, but in neither case can he get through the prayer without a mistake. He still speaks doubtfully, like a child, and has only the child's look of intelligence when spoken to, and does not attempt to answer. Was disposed to tears to-day for the second time, a matter to be taken notice of by his family who never saw tears in his eyes before. In hemiplegia tears are common enough, but not in aphasia uncomplicated.

On the 18th there was tumultuous action of the heart, which I thought a bad sign. There were other symptoms of unfavorable omen; diarrhoea, nausea, and vomiting, pain in the hypogastrium, flatulence, and other symptoms of a disordered digestion, a train of evils the exciting cause of which was error in diet, but for which there was but too much predisposing cause in the irritable heart and dyspeptic stomach of a twenty years' gouty habit. These symptoms, however, which did not increase the aphasia in any way, disappeared in a day or two, so that I was able to report on the 21st, good sleep, pulse regular though rather quick, appetite good: on this day too, he read the fourth commandment nearly without a mistake, and spoke better and

more correctly than usual. The tongue, however, remained furred as it had been nearly from the beginning, and the urine was darker in colour than usual.

My entry on the following morning was to a very different effect. I was sent for early, and told, when I entered the house, that my patient had been sleeping soundly for six hours. On going to the bedside I saw the unmistakeable evidences of apoplexy: blowing respiration, fixed, contracted pupils, the impossibility of being roused except by speaking in his ear and then only for a moment, urine passed in bed, the limbs that are raised fall like those of a dead man, weak, irregular pulse, clenched teeth, no sign of intelligence further than a slight sign of recognition of a question put to him—but no attempt to reply, heart's action feeble and irregular, lies with eyes open. He lived for four days, during which there were some fluctuations of symptoms. He responded by signs to questions on the second day, showing that he understood. His eyes followed persons about the room. He regained his power over both legs and arms, and moved them about the bed. He swallowed with difficulty a few tea-spoonfuls of milk, though the teeth remained clenched. The pulse and heart's action became fuller and more regular. But, on the third and fourth day those more favorable gave place to unfavorable symptoms, complete unconsciousness, hurried and loud respiration with chest râles, quicker pulse and tumultuous action of heart, involuntary stools as well as incontinence of urine, unequal temperature on the two sides of the body, the right being the warmer of the two, and death terminated the scene in the night between the 25th and 26th.

In these two cases of aphasia with hemiplegia I have said little or nothing of treatment, not only because treatment did very little, that is to say drug-treatment, but because my object in reporting them is not to show the efficacy or non-efficacy of any therapeutic plan, but simply to illustrate the disease in question.

The first case, that of Miss C—, does not bear out Trousseau's opinion of the incurability of aphasia when it is associated with hemiplegia. She reads and speaks well,

though the hemiplegia, as far as the right hand and arm are concerned, is as decided as ever. But the case of Mr. B—, does bear out Trousseau's observation that the combination of the two states is frequently followed by rapidly fatal apoplexy. What the same author says of treatment is also borne out by one of the cases at all events. He says that we are powerless to meet such, and that if a cure is effected it is one of nature's doing, and then only a very partial one, for that, from that time, the patient suffers from both physical and mental lameness. I flattered myself that one or two medicines I gave to Mr. B— improved his speech, and was beginning to cherish hope that his power of expressing his thought would return to him. But closer observation convinced me that the slight changes on which I built my hopes were not caused by the medicines. Those medicines were *Causticum* and *Conium*. The *Materia Medica* shows that under the heading "Sensorium" the following symptom is given among the *Causticum* ones; "he frequently pronounces words wrong, and confounds letters and syllables, for instance, cluent foryza instead of fluent coryza." *Conium* has under the same heading: "excessive difficulty to recollect things; when talking he is at a loss how to express himself; he frequently uses wrong expressions in speaking." *Calcareæ carbonica*, *Opium*, *Natrum*, and *Plumbum* have also symptoms somewhat resembling those of aphasia, but I had no time to try more than the two I have mentioned.

Both of the cases strengthen the opinion which is now so generally, almost universally, held that aphasia is to be found in connection with right hemiplegia only, and indicates therefore lesion in some part of the left side of the brain. Whether there was lesion there in those cases I cannot say. But the history of aphasia and of the controversy to which it has given rise force me to the conclusion that, as a cause of the symptoms observed in both, there was lesion in the left cerebral hemisphere.

Perhaps the most interesting question in connection with aphasia is the phrenological theory of the localization of the faculty of language. In 1808 Gall was led to the conclusion, by a series of close and long-continued observations,

that "bull-eyed" individuals, as he called them, were always distinguished for their attainments in classics, and in languages generally, and for their powers of recitation and oratory. This led him to the study of the anatomy of the brain, and to his finally localising the faculty of language in that part of the anterior lobes which rests on the posterior half of the orbital plates. In 1825, M. Bouillaud came to the conclusion that Gall was right. He collected together 850 cases of disease of the brain. In 116 of these cases the symptom of aphasia existed, and in all of those there was found lesion in the frontal lobes. In 1836, Dr. M. Dax limited still more the organ of language. Agreeing with Gall and Bouillaud in believing the anterior part of the brain to be the seat of language, he confined the locality to the left hemisphere only. He collected 40 cases of aphasia and he found lesion in the left hemisphere in all, and all too had been accompanied by right hemiplegia. His son Dr. G. Dax limited the seat of the organ of language further still. He placed it in the left frontal hemisphere, but close to the insula of Reil, in the anterior and outer portion of the middle lobe. In 1861, M. Broca went further than any who had preceded him in this investigation, and established the seat of the speech faculty in the third frontal convolution of the left hemisphere.

It is not to be supposed that these attempts to establish the phrenological doctrine were allowed to pass unchallenged. On the contrary, they were met constantly by counter statements and counter statistics. Trousseau, though he grants that aphasia is almost always, if not invariably, accompanied by right hemiplegia, refuses to believe in an organ of language. In 34 cases he examined 18 corroborated Bouillaud's position, but 16 did not; but in 155 cases of M. Baillarger there were only 10 in which the hemiplegia was not on the right side. Debates have been carried on in the French Academy of Medicine more than once on this interesting question, with, however, no result as yet. Statistics have been met with statistics, assertions by assertions, pathological and physiological facts by others bearing a totally opposite construction. In this country, also, much

has been said and written on both sides of the question, the chief of those taking part in the discussion being Drs. Bateman, Hughlings Jackson, Sanders, Maudsley, Moxon, Robertson, Browne-Séguard, Osborne, Ogle, Bastian, and Mr. Dunn.

The following are some of the main arguments advanced in favour of the localization of the faculty of language, in addition to those already given, in the anterior lobes of the brain, but more especially in the anterior portion of the left hemisphere.

Niemeyer states that in both hemispheres conduction exists between the organs of psychical action and the nerves and muscles necessary to the act of speaking, but, as most men use their right hands more readily and easily than their left, so, when a child learns to speak, it appears to be the rule that the tracts of the left side should be more used than those of the right. The left side gets, therefore, educated almost to the exclusion of the right. This view is that of Dr. Moxon, who contributed an able paper to the *British and Foreign Medico-Chirurgical Review* a few years ago in support of it. In that paper he says, "As education is based on the memory of former acts, and as memory resides where attention was in action—memory being only the mark left in the brain by former acts of attention,—education of similar simultaneous notions will grow up on one side, that on which the attention was operating; no education will grow on the side which habitually follows, attention never having been there, and therefore, of course, leaving no mark to constitute education." "The organ of so much of the power of speech as is above mere mechanical motion is developed by education on the left side of the brain, while on the right side corresponding parts remain in an undeveloped condition." He goes on to say that aphasia, therefore, does not follow from disease on the right or undeveloped side, but on the educated or left side. He maintains, also, that the more educated brains are the least symmetrical, and that, on the other hand, the brains of the lower animals are more symmetrical than those of human beings. In connection with this, which, if a fact,

is one telling in favour of the localization theory, Carl Vogt asserts that in apes there is no third convolution in the left cerebral hemisphere, the convolution in which M. Broca plants the organ of language. Again, M. Gratiolet has said that the left frontal convolutions are developed more quickly than the right, which, if true, strengthens the education theory of Dr. Moxon, as that education will be more readily accomplished on the side (the left) of more rapid development. Dr. R. Boyd has found the left hemisphere the eighth of an ounce heavier than the right, and M. Baillarger the left frontal convolutions larger. Dr. Ogle localizes language in the third left frontal convolution, and meets the symmetry objection,—that nature never could intend the two halves of a symmetrical organ to have different parts to play, by saying that there is not exact symmetry in the arrangement of the convolutions of the two sides, and that we educate the left hemisphere as the organ of language to the exclusion of the right. M. Trousseau never met with a case of intercostal neuralgia except on the left side, which he calls an anomaly analogous to that of one-sided speech. Dr. Ogle agrees with M. Gratiolet that there is an earlier and more rapid development of the left than the right hemisphere, and gives as a probable cause the more direct course of the left carotid than the right. Dr. Ogle strengthens this argument by quoting a case in which aphasia followed ligature of the left carotid. Corroborative also of the education of one rather than of two hemispheres or portions of hemisphere is Professor Bain's view of the action on the brain of thought and feeling. "It must be considered as almost beyond doubt that the renewed feeling occupies the very same parts, and in the same manner, as the original feeling, and in no other parts nor in any other manner that can be assigned For every act of memory, every exercise of bodily aptitude, every habit, train of ideas, recollection, there is a specific grouping or co-ordination of sensations or movements, by virtue of specific growths in the cell-junctions."

Trousseau mentions one case only in which, in connec-

tion with aphasia, hemiplegia was found to be on the left side. In this case we may, with good reason, argue that the cerebral disease was on the right and not the left side. But we ought to know, in a case of this exceptional kind, for exceptional Trousseau admits it to be, whether the patient was left-handed. For as there are left-handed people may not there be right-brained people, who make use of the right instead of the left convolutions? In another of Trousseau's cases of aphasia no lesion was discovered in the left convolutions; but, nevertheless, close examination showed a pinkish blush and a red patch there, proving, of course, an abnormal condition of that part of the brain. Considering the large amount of evidence in favour of the view that aphasia is connected with lesion in the left hemisphere, and in a circumscribed part of that hemisphere, a very close, even a chemical and microscopic examination should be made before coming to the conclusion that in a special case there is no disease to account for the aphasia. M. Baillarger and Mr. Lockhart Clarke have shown us that the brain convolutions have each of them six layers; and not only so, but that each of those layers is capable of subdivision into seven more, and that every one of those layers may have a separate function; such facts may well make us cautious in concluding that no disease exists where none can be found. The microscope itself may fail us in the presence of a structure so minute and complicated. In some cases no disease has been found in the left frontal convolution but in other parts of the hemisphere having relationship with it. On the other hand, as shown by Dr. Parrot and by Messrs. Fernet and Charcot, the anterior lobe of the right side may be extensively diseased without aphasia. Trousseau, whose evidence on this side of the question is valuable, seeing that he refuses to allow of an organ of language, examined 134 cases of aphasia with the result of finding lesion in the left hemisphere in 124. In four cases, at least, where the hemiplegia was on the left side, two of them reported by Dr. Ogle and Dr. Hughlings Jackson, it was ascertained that the patients were left-handed.

Dr. Sanders, in the *Medical Times and Gazette* of April 7th, 1866, was the first in this country to give the particulars of a post-mortem examination in a case of aphasia. The patient was Margaret M—, a servant, aged 43. She had deficient memory and speech and showed distress at her deficiency. She misnamed words and could not write, and had right hemiplegia. The examination after death showed disease nowhere but in the posterior portion of the external or inferior left frontal convolution and in the left parietal lobe. Dr. Bateman quotes a case of M. Lesur's of a child whose frontal bone was fractured by the kick of a horse. She was trepanned about an inch and a quarter above the left orbit. It was found that pressure on the portion of exposed brain suspended the power of speech, which returned, however, on that pressure being removed. A somewhat similar case was reported by Dr. Copeman to the Norwich Pathological Society. There was the rupture of a blood-vessel in the left orbit and aphasia in consequence, which continued to a greater or lesser degree as long as the pressure of the effused blood on the eyeball continued. With diminishing pressure there was a return of speech, and a complete recovery took place. Schroeder van der Kolk gives still another case of the same kind where there was loss of speech in consequence of the pressure of a bony splinter on the left anterior hemisphere. The bone was removed by trephining and a cure followed. Dr. Hughlings Jackson gives the particulars of thirty-four cases of aphasia, in all of which, with three exceptions, there was right hemiplegia. Here Dr. Hughlings Jackson gives involuntary testimony in favour of a doctrine he repudiates, the existence of an organ of language. In the *Medical Times and Gazette* of July 9th, 1864, Dr. Stewart gives a remarkable case, and one, it appears to me, powerful on the affirmative side of this question, in which there was at first left hemiplegia and no lesion of speech, but in which, a week afterwards, right hemiplegia occurred with aphasia.

Aphasia would not stand alone as a pure and uncomplicated symptom if the organ of language had its seat in

the brain as a whole, as some maintain. There are such uncomplicated cases of aphasia to be occasionally met with, cases which have been treated with a few leeches or by venesection or not treated at all, and have ended in the perfect recovery of speech. Such cases have had, probably, as their cause some temporary congestion with consequent pressure upon the cerebral matter at the seat of the organ of language.

Dr. Broadbent has found in his physiological investigations that the third frontal convolution of the left side is larger than other convolutions, and receives fibres from a greater variety of sources.

M. Bouillaud pierced with a gimlet the frontal lobes of a dog. The animal survived the operation, but lost the power of barking. Schroeder van der Kolk is an advocate for the localization of the faculty of language, but he placed it in the olivary bodies, which, he says, are to be found only in the mammalia, and smaller in the lower animals, in the ape, for instance, than in man. He illustrates his position by cases in which disease was found in those bodies when aphasia had been a symptom.

Such are some of the chief arguments in favour of the localization of the organ of language in one part of the brain, in its left hemisphere, and in a circumscribed portion of that hemisphere. I shall now proceed to give the arguments on the other side, that of those who hold that there is not and cannot be an organ of language.

Dr. Maudsley objects that if there is a speech-centre or seat in the brain there must be a motor centre also, which is contrary to our present knowledge. All motor centres, he says, must be subordinate to the supreme hemispherical ganglia, and lesion of the corpora striata, not of the third left frontal convolution, or of some part of the communications between the motor and the supreme centres, must exist in aphasia. To Dr. Moxon's education theory he objects that if the motor nuclei act simultaneously the education of one side must be the education of the other. Dr. Anstie says: "it is possible to suppose some anatomical differences in the brain structure or in the disposition of the blood-vessels

which might account for the frequent coincidence of aphasia with right hemiplegia without the necessity of supposing a localization of the faculty of speech in one side of the brain. There must be some such difference to account for right hemiplegia being more common than left." Dr. Hughlings Jackson grants that aphasia may be caused by lesion near the corpus striatum, but denies that there is any faculty of speech localized there or anywhere but in the whole brain or body. The lesion there results in the breaking up of parts which help to make symbols; it is the way out from the hemisphere towards organs which the will can set in motion. If there is lesion in the third left frontal convolution there must also be lesion in the surroundings of the corpora striata—the most important meeting-places of the fibres of the brain.

Cases have been reported in which, where aphasia existed, disease was found in the right and not in the left frontal lobes, and also cases in which, disease having been found in the left frontal convolution, no aphasia had existed. Dr. Crichton Brown has collected together six cases in which there was *left* hemiplegia accompanying aphasia. Dr. Robertson does not believe in the existence of an organ of language though he grants that aphasia may be caused by lesion of different fibres in the left hemisphere passing between the convolutions and the great coordinating centres, the voluntary motor impulses for the articulation of language being thus incapable of being transmitted. The morbid change is thus, he says, a motor and not a mental one, being simply a defect in transmission.

M. Baillarger maintains that both hemispheres are educated but that the left takes the lead in operations where both are concerned; the words lie on the right side but their internal reproduction on the left. The absence of aphasia in lesion of the right hemisphere may be explained by the escape of parts usually implicated on the left side owing to some slight anatomical difference such as exists in the case of other bilateral organs. Trousseau gives the case of a woman who had right hemiplegia and in whom the lesion was found in the left insula of Reil, and in the third

right frontal convolution ; and Velpeau gives one of a man in whom post-mortem examination showed a complete destruction by scirrhus tumour of the right anterior lobe of the brain and of a portion of the left ; the man having been during life, an “extremely tiresome talker,” and not having shown up to the last any defect of utterance. M. Bernard reported, in 1843, to the Anatomical Society of Paris, a case in which both anterior cerebral lobes were destroyed by the explosion of a mine, “and in their stead was a mixture of blood, of bony splinters, and of brain substance.” This man lived for twenty-four hours, but was able, before his death, to give an account of the way in which the accident happened. Cruveilhier is said to have published some interesting cases of speech defect in which lesion was found not in the anterior, but in some other part of the brain. M. Andral has examined carefully into the subject, and his conclusion is that the faculty of speech “is under the special guidance of some definite part within the cranium.” But he denies that we can, in the present state of our knowledge, state what that spot is.

Dr. Forbes Winslow found disease in the anterior lobes in 54 cases which he examined and where no aphasia had existed. Dr. Maty, quoted by Dr. Bateman, gives a case of aphasia of four years’ standing where the only lesion found was hypertrophy of the medulla oblongata with denseness of structure and very tough membranes, and hard cervical portion of the spinal cord.

In the *Medical Times and Gazette* of December 21st, 1867, Dr. Simpson gives a case of disease of the posterior part of the third or inferior frontal convolution of the left side in which there had been no aphasia. This case stands almost alone on the negative side of this controversy. An argument has been brought against an organ of speech having its existence in the brain, founded on the fact that venesection and the action of a purgative medicine have sometimes cured aphasia. It is forgotten that temporary congestion with its consequent mechanical pressure will sometimes produce many of the symptoms of formidable disease, and that congestion will sometimes give way under

the influence of bloodletting, or a purgative, or nothing at all. Dr. Bateman, who has perhaps made more a specific study of this subject than any one else in this country, says: "Is there a centre of speech at all? May not loss of speech be owing to some altered state of the cerebral tissue not appreciable to eye or microscope? May not the defect be that of an altered electrical condition, or of a thermal change, the result of chemical action?"

Is there a centre of speech at all, and, if there is, where is it to be found? That is the question which these authorities whom I have quoted have set themselves to answer. If "language is our Rubicon and no brute will dare pass it," as Max Müller says it is, no one can be surprised that the aphasia and phrenological questions should be eagerly discussed. The surprise rather is that the phrenological one should have been so long in abeyance, and only now galvanized into fresh interest by the aphasic one. That I believe those to have the best case who support the affirmative side of this argument will have been gathered by such as have read what has gone before. It is difficult to explain away such facts as the following: aphasia is found almost invariably in connection with right hemiplegia, a fact recognised by both parties; cases are reported by Dr. Ogle and Dr. Hughlings Jackson of left hemiplegia with aphasia in which the subjects were left-handed: in the case of symmetrical organs it is observed that one part or organ is developed and used in a much greater degree than the other, and that even in the most apparently symmetrical organs, the microscope, if not the unassisted eye, detects differences which are not obvious to cursory examination; in such cases one side remains uneducated and undeveloped, and lesion of it may not affect the function which both were intended to subserve; whereas, lesion of the educated or developed side is followed by loss of that function: strongly corroborative of this view of symmetrical organs as far as the brain is concerned, is the fact that the left hemisphere is earlier and more rapidly developed, and larger and heavier than the right, and another fact that the lower animals have more symmetrical brains than man: the ape

has no third convolution in the left hemisphere ; the ligature of the left carotid has been followed by aphasia ; I must here allow, however, that Mr. Nunneley, of Leeds, questions the truth of this statement, and gives a case in which ligature of that artery was not followed by aphasia : many cases are on record of extensive disease of the right frontal lobes without aphasia. M. Bouillaud's dog-case, in which the power of barking was lost from injury to the frontal lobes ; the case of M. Lesur, where a portion of the left frontal lobe was exposed by the operation of trephining and aphasia caused by pressure on that portion, the aphasia ceasing on the removal of the pressure ; Dr. Copeman's case of hæmorrhage in the left orbit, the aphasia being caused by the pressure of the effused blood, and diminishing with the lessening amount of hæmorrhage, and ceasing when the hæmorrhage ceased ; Dr. Stewart's remarkable case in which two paralytic seizures happened within a week, the first one being of left hemiplegia without aphasia, and the second of right hemiplegia with aphasia ; the uncomplicated or pure cases of aphasia of only a few hours' or days' duration, which argue a circumscribed locality and a temporary cause, for if speech resided in the whole brain as some assert, such cases could scarcely occur. Finally, the non-detection of disease in the left frontal lobe need not be considered proof positive that no disease exists, for our means of detecting disease are not so unerring as to enable us to say, when examining a portion of brain or muscle or other tissue, that there is no lesion in it ; in the case of a recent paralysis of brain matter, for instance—and we do not know that such a lesion may not exist—what means have we of diagnosing such a disease or recognising it in a post-mortem examination ?

The arguments on the other side of the question are, most of them, more negative than positive. One of these is that, in the case of a symmetrical organ, nature could never have intended that two sides should have different functions, or rather that the function should reside on one side and not on the other, and that the education of one side must necessarily be the education of the other also. Facts, however, are against this view of the case. Symmetrical organs

and parts are not identical as to shape, and size, and power. It is a question of degree; the difference is more or less, and it should not surprise us more that the left frontal hemisphere is developed in a higher degree, and more used than the right, than that our left hands should be so inferior to our right ones in every respect in which the hands are agents of our purposes. It is said by some who unwillingly acknowledge that aphasia accompanies right and not left hemiplegia, that the explanation may be some anatomical difference in brain-structure, or in the distribution of the blood-vessels of the two sides of the brain; but such explanation is not made out, and, even if it were made out, would scarcely argue against the existence of an organ of language. That the faculty of language resides in the whole body or brain, and not in a particular part, is simply an assertion which cannot very easily be proved or disproved, but, at all events, it cannot be allowed to have weight in the present controversy. That when disease exists in the third left frontal convolution, there must be disease also in the corpus striatum is an argument against an organ of language which does not appear to me to have much weight. Even if the fact were so, how can it affect the argument further than that the corpus striatum may have as good a claim to be considered the organ of language as the third left frontal convolution. The strongest arguments against the localization of language in a certain small portion of the left hemisphere are urged by those who quote cases of aphasia where brain lesion was found on the right side, and other cases where disease in the left hemisphere was not followed by aphasia. The details of some of those cases are not given so minutely as must be considered necessary when a question such as the present is under consideration. For instance, in Velpeau's case, which I have given above, and where speech was not lost, it is said that the right anterior lobe of the brain was completely destroyed by a scirrhus tumour, and that the left frontal lobe, also, had been encroached upon, and was, to a great extent, destroyed. It is not said that the left frontal lobe was completely destroyed, but only that it had been encroached upon, and, to a great

extent, destroyed. Notwithstanding this serious lesion, the third convolution of that left hemisphere may have been left intact. In M. Berard's case, too, which is considered so conclusive against the localization advocates, there is an important flaw in the argument. There is no record of a post-mortem examination. It is said that the brain was exposed in consequence of the destruction of the anterior part of the skull, that both anterior lobes were completely destroyed, and that in their stead was a mixture of blood, of bony splinters, and of brain substance; and yet that, notwithstanding all this, the patient was able to speak. It appears to me that it would not be possible to tell, in this case, whether the anterior lobes were *completely* destroyed or not. That there was great injury to those lobes is quite clear, but that their destruction was complete could only have been decided after death. The man died, of course, but there is no report of the post-mortem examination, if one was held. The other class of cases in which aphasia existed in connection with left hemiplegia, and disease in the right hemisphere, are wanting in some particulars which ought to have been given to make the argument complete. With only two or three exceptions it is not mentioned whether there was or was not left-handedness, nor is it mentioned whether a careful examination, by chemical agents and the microscope, had been made of the brain tissue of the third left frontal convolution. Without such investigation the conclusion that there was no disease cannot be arrived at. Dr. Simpson's case is, perhaps, the most positive argument against the existence of an organ of language. He found disease in the very convolution of the left hemisphere in which M. Broca and others have planted speech, and in a case in which there had been no aphasia. In the first place, this case only refers to disease in a particular convolution, so, even if it was conclusive, it would only be so against seating language in that particular locality. It would not be an argument against an organ of speech; but would it be conclusive even against the third left convolution? It appears to me that it would not be so. We know that there is disease of the ear which does not imply deafness,

and of the eye which is not accompanied by blindness. Disease, therefore, may exist in the organ of the faculty of language, which may not have, as its consequence, the loss of speech. One disease of that organ may involve the loss of speech, but another may not.

The conclusion from this summary of the arguments for and against the localization of speech in the left cerebral hemisphere is, that a good case is made out on the affirmative side of the question. It would be going too far to say that there is proof positive that there is such an organ, and having its seat in that part of the brain. But the weight of evidence, up to this time, appears to me to be in favour of the existence of such an organ, and of the site of that organ, if not in the third left frontal convolution, at all events in the left hemisphere.

There are many questions in connection with aphasia quite as interesting as that of the phrenological one, and upon which there is quite as much difference of opinion. Authorities cannot even agree as to its definition. Trousseau calls it "a loss of the faculty of expressing one's thought by speech, and, in most cases, also, by writing and by gestures." Brown-Séquard's definition is, "A paralysis of the organ of the expression of ideas," a definition which it appears to me is the best that has been given. Todd calls it a want of relation between the centre of volition and that of intellectual action necessary to give expression in words to thought. Baillarger defines it to be a lesion in the left hemisphere of afferent fibres passing between the convolutions and the corpus striatum, so that voluntary motor impulses for the articulation of language cannot be transmitted. Dr. Sanders says that the motor impulse to speech cannot be conveyed to the articulating muscles or to the co-ordinating centre of articulation by reason of injury to the voluntary initiating or connecting apparatus. Many define it to be simply a want of memory—M. Dax, for instance. But there are many objections to such a definition. If Max Müller, Dugald Stewart, and others are correct in saying that thought can only be accomplished by the agency of words, and if there is proved to be thinking power in aphasics,

which it is not difficult to do, then it must be granted that in aphasia there is no want of memory. For, not to allude to the fact that the power of writing sometimes remains even when that of speaking is gone, a proof, of course, that the memory of words is not lost, there is abundant evidence offered by most aphasics that thought and intelligence exist in the animated gestures and signs they exhibit, in the evident approbation or disapprobation manifested when their wishes are complied with or thwarted, and in the enjoyment (by some) of entertaining books. The word amnesia should, therefore, not be used when speaking of this question. The words are all there, the mind is charged with them, but they cannot be uttered. The defect must be considered a motor, not a mental one. Between the word thought and the word uttered there is a channel of communication. That channel does not hold, there is some flaw in it, and so the thought is interrupted in its road downwards towards the tongue. But the thought is there which shows that memory is there.

In connection with this memory definition there is another explanation to the effect that, though there is not a loss of memory of words, there is a forgetfulness of those motor actions which are necessary to set the muscles of the vocal organs in motion. It would be difficult to prove that there is such a memory. It is just as likely that the lung's and heart's actions depend upon the existence and normal condition of such a memory.

Dr. Ogle describes aphasia to be a want of co-ordinating power over the muscles of articulation; an ataxic defect, in fact. Dr. Maudsley thinks it involves a lesion of some part of the motor centres or of their communications with the supreme centres. Mr. Dunn is contented with saying that it is a lesion in more than one part of the brain, and not of the left side only. Dr. Bastian defines it to be a lesion of the afferent fibres along which the motor stimuli pass that primarily incite those combined muscular contractions necessary for speech and writing; the afferent fibres not being damaged there is the intelligent perception of what is said by another, and, there being no lesion of the

cortical or grey matter, thought can be carried on but not translated into articulate speech, and this owing to the lesion of fibres running between the grey matter and the corpus striatum, or in the latter itself. Dr. Wilks says, "Between the surface of the brain where ideation is carried on, and the origin of the nerves of the vocal apparatus there is a part of the brain where the ideas are put into form, into the form of speech, and here, as it were, the strings are pulled which set the machinery in motion. If this is damaged, the power of speech has gone, and we have what is called aphasia." I have said and quoted enough to show the contrariety of opinion on this interesting subject. But it cannot be otherwise as long as the physiological and pathological questions connected with language and its defects remain unsolved or unsettled.

The exciting causes of aphasia are numerous. In the cases I have reported at the beginning of this paper the cause in both was probably the same, obstruction to the course of the blood through a cerebral artery by an embolus. In Mr. B—'s case there was both gout and rheumatism to account for the valvular disease of the heart, which unquestionably existed, and which had existed for a long time. Warty vegetations in all probability had formed on the valves, and accounted for the irregular, and unequal, and tumultuous pulsations of the heart and radial artery. The attack of hemiplegia with aphasia was most likely caused by the detachment of one of those vegetations, and its conveyance to the left hemisphere in the current of the circulation to a blood-vessel too small to admit of its passage, where it, therefore, stuck and constituted the condition called embolism. His improvement after a certain interval of time had elapsed might have been caused by the restoration of the current of circulation, either by the passage onwards of the clogging embolus or by the function of the part of the brain affected being restored by the increase in size of neighbouring blood-vessels. The second attack may have been caused by a still further clogging of the arteries, and by the consequent arrest of function of a larger portion of brain. A fact which bears out this explanation is that, just

before both seizures, the heart's action was unusually irregular and tumultuous, and perfectly regular immediately afterwards, as if the cause of the heart's disturbance had been carried off. Might not warty vegetations, blocking the heart's orifices and impeding the action of the semilunar valves, have explained that disturbance, and their detachment and passage onward, the calm afterwards? The pulse remained quiet and regular till within about a week of the second attack, when it and the heart's action became irregular again, as if fresh vegetations were again forming. On stethoscopic examination, at that time, there was no bruit, but only muffled, blunt sounds, the first and second being quite indistinguishable. Again the pulse got calm and regular after the second seizure, and remained so to the end.

In Miss C—'s case the embolus, if embolus it was that proved the exciting cause of the attack, was not followed by another. The pressure on the brain was gradually taken off, the function of the injured part was gradually restored, till she attained that measure of health which she still enjoys.

DR. ACWORTH AND COUNT MATTEI'S MEDICINES.

WE have received the following letter from Dr. Acworth respecting his article in our last number and our comments thereupon.

To the Editors of the 'British Journal of Homœopathy.'

GENTLEMEN,—I must ask you as an act not of courtesy but justice to let me say a few words in your Journal in reference to my article on Count Mattei's medicines and your note appended thereto.

By that note it seems to me you are placed in this dilemma:—Either my article is worthless or it is not. If worthless—if only deserving of contempt—it is very strange respect for a colleague

that could be the motive for its publication, while it has not withheld its being published with additions of such a very condemnatory kind, as condemn not merely the article itself, but those who would inflict it on their readers. They, if not I, have certainly cause for grave complaint against you. If, on the other hand, the substance of my paper be not so indubitably worthless and absurd as to make it undeserving of fair consideration, you have proved yourselves to have learned all too quickly the vices that characterize an established corporation—the intolerance that marks the lovers of a system rather than lovers of truth. That homœopathy, scarcely yet emerged from a storm of universal and uninquiring scorn, should so soon become a shut-eyed scorner is sad—is sad indeed. Suppose it even came to pass that you had to burn your diplomas and your books! What then! The question really before you as *men*—bound to relieve the sufferings of your fellow-men, and, *à fortiori*, as *physicians* whose *raison d'être* is the existence of disease, which they are especially called upon to combat—is not the fate of your diplomas or your books, but the curative power of Count Mattei's medicines? If you say that there is no question that what has been put forward is self-evidently folly—then, again, I cannot help repeating that you have wronged your readers, if not me.

But if a question there really be, is it not a shame to you as students, and much more a shame to you as homœopathic students, that, while taking the field against allopathy, you inscribe upon your banners as your cognisance or motto, "*In certis unitas, in dubiis libertas, in omnibus charitas?*" no sooner, to give your words a meaning, does "*in dubiis libertas*," freedom of inquiry ask "*in certis unitas*" a certain British Journal, with its oneness of assurance made trebly sure that "*in omnibus charitas*," the charitable world may become acquainted with a new claimant for its favour, than you raise the yell of the old bigotry again, re-echo the cry of charlatan and quack, which even yet your enemies have not ceased to fling at you, and "trust that no one in our body will respond to the invitation" to take upon himself to try whether medicines claiming to be the mightiest that have ever yet been offered to the world are really so or not.

And now as to your use of the words charlatan and quack. Will you kindly tell us what they really mean, or rather are meant to mean? Are they mere compendiums of cheap censure which they would convey if they only could, but could not, being

properly defined? What is a charlatan? A bad pathologist? If so, alas! for our profession. Is he one whose therapeutics are at fault? If so, I say again, alas! for our profession. Or is he one, as I understand the word, who imposes on credulity and suffering, and solely with a view of base advantage to himself? And, if so, how does this apply to Count Mattei? For these twelve years past he has charged himself with maintaining a hospital at his sole expense, and administered to rich and poor alike his advice and medicines gratuitously. At all events, he cures—and he cures for nothing! “If,” as he says in a letter to me, “he who cures cancers and aneurisms as you have seen vouched for by Professor Pascucci, director of a hospital in Rome, be called a charlatan, so be it. All the worse for him who is called a physician, and knows not how to cure effectually. Charlatan has hitherto signified a cheat. Cheats will fast prove those physicians to be who, possessed of certain sight and bad faith, persist in their obstinate unwillingness to see, and try to strangle the truth with a word of derision.”

So much for Count Mattei's quackery. And now for mine. In what does it consist? In extolling the curative power of certain medicines as being greater than of others better known. There surely is no quackery in this, even if it prove to be all a mistake. The essence of quackery lies in the intention—in cheating others wittingly, and not in cheating unwittingly oneself. All my quackery lies in using secret medicines which I have an interest in keeping secret still, instead of making their virtues known that others beside myself might profit therewithal. This is the very head and front of my offending. Now, I no more approve of secrecy in medicine than those who objurgate it and me. But when the choice lies between the use of secret medicines and letting a patient orthodoxly die, my mind is easily made up. And if on this account I am called a quack, I shall try to find satisfaction in the name.

The question was propounded some time ago by one of the Editors of your Journal, Am I an Homœopath or a Physician? When this has received a satisfactory solution there is another I would like to ask—whether I call myself one or the other (and for me I merge the former in the latter), what is the duty as a medical practitioner that I am called upon to do? My answer is to cure disease, not *secundum artem Homœopathicam*, but *quocunque modo* that I can. I adopt the homœopathic plan of treat-

ment—and why? For the sake of curing it *cito, tuto, et jucunde*. But if I knew any 'mode of' curing it *citius, tutius, et jucundius* I would not fail to make use thereof, even though it should be *secundum artem caninam*; for Count Mattei took a dog for his preceptor who, not having physic thrown to him, which he might cynically deem a work of human supererogation, got rid of his mange by taking his own physic, instead of Dog-Latin prescriptions, not his own, and which he used to find in his botanical excursions when he set out in quest of certain healing plants which this four-footed herbalist dosed himself withal. I would not, any more than Count Mattei, disdain to profit by such cynic teaching, seeing, by-the-bye, that it is something better than that of most of the cynics I have known.

So, if asked what I am, I say a physician who practises medicine by homœopathic law, but when finding that fail would not scruple to do so according to the doctors of the "cynic-cur," if their school of medicine promised more success. *Cure* is my *first* consideration—the *mode* of cure my *second*. Modes of treatment are but means to an end. Systems are nothing but as they lead to this. The end is everything, and the best means to secure it are the means to be preferred, be they made use of by whatever school they may. A physician has to overcome disease by the best means in his power, and so, if by all known remedies he cannot, by unknown if he can. If any medical code forbid their use he will find there is a higher, a moral code, that peremptorily demands it. To save life surely should be something more to him than to save himself from the charge of quackery. The duties which he owes to humanity and God are paramount to those which he owes to his profession, if those of his profession were incompatible with these. But they are not. They cannot be. The idea is too absurd. His great business is to cure. It is the thing he came into the world to do, in spite of Sydney Smith's assertion that the sixth commandment is suspended in his favour. It is "doing his duty in that state of life into which it has pleased God to call him," and showing that his catechism has not been learned in vain.

But all this is medical heresy, it seems, and not to be tolerated by heretics themselves, for in orthodox style you express the hope that "the universal condemnation my paper must elicit will lead me to reconsider my views and not desert a cause I have hitherto adorned for such gross empiricism." With regard to the "uni-

versal condemnation" it is, at most, a thing to *come*. And, meanwhile, I could almost wish that it might have saved me from the many letters I receive which, strange to say, condemn their writers and not me. As to the "reconsideration of my views" I hope that I shall never be wanting in enough of wisdom and humility for this. But if by reconsideration you mean renunciation of what I hold true, or the retractation of a statement of facts (for *facts* are what I have given, not *views*, though I have criticised those of Count Mattei as to his medicines being strictly homœopathic, which they yet may prove to be if *really proved*). I must beg to remind you that had your great master but acted in the spirit you recommend to me, there would be no *British Journal of Homœopathy* to illustrate now in allopathic fashion the virtues that its outside blazons forth of "unity in things certain, liberty in things doubtful, and charity in all."

Thus much has it seemed to me right to say on behalf of my profession rather than myself. Thus much too has it seemed well to say as between the Editors of a journal and one who has been a contributor to it of articles which, if appraised beyond their worth, might yet have disposed those who so appraise them, to "allay with some cold drops of modesty, the skipping spirit" brought to bear upon his last. Thus much as between the Reviewers and Reviewed. Of how far you have transgressed the courtesy that gentlemen are apt to think their due, and the kindness that should pass between men of our high calling, and the something more that perhaps might be expected between those whose intercourse had hitherto been friendly, I have not a single syllable to say, as I am not one who is wont to waste his words.

I am, Gentlemen,

Your obedient Servant,

E. ACWORTH.

9, MONTPELLIER TERRACE, BRIGHTON;

November 12th, 1870.

We have allowed our colleague to speak for himself; and now we would have our own say upon his *gravamina* against us, and upon the general questions he has raised.

And, first, upon our action in inserting his article and appending thereto our condemnatory note. Dr. Acworth argues that if the latter was warranted, we did a wrong to our readers, if not to himself, in allowing the former to find

place in our pages. Not necessarily so. In suffering our regard for the author to outweigh our objection to his doctrine, we thought for our readers, as likely to be influenced by similar motives. We acted, whether rightly or wrongly, as the representatives of their wishes; and gave Dr. Acworth a hearing, while protesting against any favouring of his views. We now, indeed, think we should have been wiser had we refused the article altogether. But the fact was that we had little time for consideration. The MS. was sent to us only at the last moment; and, owing to a misunderstanding, was actually read by none of the editors until the printed sheets came before us for correction. In the haste of the moment, with no time for intercommunication, we decided on the course we followed. If we have erred, we crave our readers' forgiveness; and with this leave the present branch of the subject for another of more importance.

Dr. Acworth seems to think that we are inconsistent with ourselves, and copyists of the old bigotry, because we refuse to listen to Count Mattei. But we entirely deny the analogy between the attitude of the old school towards us and ours towards him. It is not against novelty we are protesting, but against secresy. We have no thought, as we have used no word, of such things as orthodoxy and heresy in medicine. Our objections are moral, not intellectual merely. We cannot admit that, because we have embraced homœopathy, we are disabled from calling anything quackery. We defend our own system from the charge, not as denying the existence of the thing, but as showing that the name is inapplicable to what we profess and practise. So far, therefore, from being shy of condemning real quackery, we are bound to cherish especial jealousy against it. Hitherto our hands have been clean: we have even been able to say to the old school, "The Morisons and Holloways, the true quacks, are yours, not ours." But if we were to follow Dr. Acworth's guidance, this vantage ground would be lost; and the homœopathic principle would be saddled, in addition to the already burdensome weight of infinitesimals, with the crushing load of secresy in its remedies. The one we must bear as best

we may ; but Dr. Acworth must not be surprised that we are forward to repudiate the addition of the other.

In truth, the question is not one of homœopathy at all. We do not know, nor does Dr. Acworth himself, whether these remedies act upon the principle of similarity. The only appearance of homœopathicity they present is their form of globules—an accidental and by no means admirable feature of our system. So that the “*in dubiis libertas*” of our motto has no application to them. It is a “*British Journal of Homœopathy*” which is conducted upon the basis of such liberty ; and the “*certa*” and the “*dubia*” alike are understood to be homœopathic in nature. It would have become us, therefore, if we had chosen to refuse Dr. Acworth's paper ; it became us equally, since we thought well to admit it, to accompany it with our disclaimer and reprobation.

But let us go a little deeper into the matter. Dr. Acworth asks us what the word “*quackery*” is “*meant to mean*” by us. We will tell him. It is “*meant to mean*” the use in a liberal art, which Medicine is, of the practices of *trade*. In Medicine we are dealing with human life, and with what is almost more precious than life itself, human health. To practise rightly such an art, every precaution should be taken for full qualification on the one hand, and for perfect simplicity and openness on the other. Hence it is generally agreed that in Medicine certain practices are “*unprofessional*.” Of one class are those which violate the necessity of due qualification—such as practising without a license, or using a title not legitimately obtained. Of a second class are all means of pushing oneself forward other than the exhibition of knowledge and skill. Hence we may not advertise ourselves, or obtain puffing paragraphs in newspapers. There are many shades of such unprofessional conduct. As they grow darker, we want a more positive term to characterise them. And so, perceiving how they assimilate their practisers to the nostrum-vendor of the fairs and markets, we call such persons by his name of “*quack*.” We ridicule him by it ; but we stigmatise them, because their pretensions are of a higher order. And we use the oppro-

bricious term in its fullest meaning, when any professor of the art of healing copies Dulcamara's ugliest feature, and keeps his remedies secret. His motive may be different; he may really believe in the virtues of his panaceas; but his conduct is the same. Though he be not a charlatan, his practice is charlatanry. And the consensus of the whole profession, and the instinct of all who have looked at the subject, is that any one who makes a secret of the remedial means he uses is guilty of quackery.

Dr. Acworth, of course, does not deny that the use of hidden nostrums is ordinarily unjustifiable. But he endeavours to prove that Count Mattei's proceedings form an exception to the rule. He tells us that the Count takes no fee for his prescriptions, and gives his medicines away. Well, if he be a rich nobleman, money is naturally no object to him. But it remains to be proved that there are not other things besides money that a man may wish for, and that may be obtained by the means Count Mattei has adopted. There is such a thing as the love of notoriety and applause. But even granting that he is influenced by no motive but pure humanity, he is not therefore to be excused when he imitates the charlatan in his mode of proceeding. No reason of any adequacy has been assigned for keeping these medicines secret. If there be any good in them, in the name of humanity itself let them be known far and wide,—not as “Count Mattei's Marvellous Medicines,” not as “anti-canceroso,” “anti-scrofuloso,” and such like misleading titles, but by their own recognised names. He who keeps a remedy secret is morally responsible for the deaths of all who would have recovered had the remedy been revealed. Dr. Acworth would have rendered much truer service to the suffering subjects of our art if he had joined his English brethren in refusing to prescribe one of these remedies until their composition was revealed. Now he has just confirmed the Italian amateur in his ill-advised course.

And when our colleague goes on to ask us wherein does his own quackery consist, we wish that we could reply that we do not accuse him, as we have not hitherto accused him,

of personal guilt in the matter. But we fear that our judgment must bar our feelings here. Would Dr. Acworth, we ask him, announce to his professional brethren, that he had discovered seven remedies capable of curing all internal diseases, which he called "anti-this" and "anti-that," but whose name and nature he would not disclose till we had one and all admitted their superiority to our ordinary medicines? Would he promulgate the virtues of a liquid external application, falsely named by the catch-penny title of "vegetable electricity," and only to be obtained (to check the rapacity of chemists) at its weight in gold? We are certain that he would scorn such unworthy conduct. How then can he reconcile it to his conscience to father it when practised by another, and himself to profit by it? We know Dr. Acworth to be an honorable man: but he must be aware that every appearance of mystery is attractive to the vulgar, and that the more *hocus pocus* there is about a man's practice the larger is his clientèle. "All my quackery," says Dr. Acworth, "lies in using secret medicines which I have an interest in keeping secret still, instead of making their virtues known that others besides myself might profit therewithal. This is the very head and front of my offending." Of course Dr. Acworth means by this—though the words convey but obscurely his meaning—that he is absolved of the charge of quackery in using secret medicines by the open avowal of their use and the indication of the source where others may procure them. This, indeed, would remove the actual stigma of quackery at first hand, but it leaves him as the abettor of a quack, not, one would think, a very comfortable position for a high-minded physician—which we know our colleague to be—to occupy. If Dr. Acworth would scorn to be the promulgator of anti-scrofulosos, anti-cancerosos, and vegetable electricities, at first hand, as we know he would, how can he reconcile it to his professional conscience to use and advocate the use of them merely because a foreign nobleman and not himself is the author of the quackery?

We do not think that Dr. Acworth's case is improved when he takes his stand on the ground of success, and of

the end justifying the means. The narratives he gives bring no conviction to our minds that the results obtained are better than those which have often followed the use of our known remedies. And the 20,000 cases said to have been cured at Count Mattei's Hospital in two years and four months, certified though they are by Professor Pascucci (who may be a great man, but of whom we never heard), produce upon us precisely the same impression as Dr. Barry's 60,000 cures by means of his *Revalenta Arabica*, certified by all the European celebrities from His Holiness Pope Pius IX downwards; or Holloway's 100,000 cures by pills and ointment, vouched for by all classes of patients with all manner of diseases, from the Earl of Aldborough with his liver complaint down to Maria Jolly with her bad leg of thirty years' standing (by the way, the leg could not have been so very bad if it stood so long, and it was highly creditable to Maria to remain Jolly under such an affliction). They impress us as much, and no more, as does the colossal granite lion in front of the "British College of Health," said to have been erected to the memory of Morison the Hygieist by the pennies of grateful patients cured by his wonderful pills. If the system whose value they purport to evidence were otherwise plausible, or even unobjectionable, they might just incite us to inquiry. But they are wholly insufficient to plead in exculpation of a mode of practice in no respect better favoured than those to which we have alluded.

But our objection cuts deeper than this. It is only one side of the truth that in medicine the end is all-important, and the mode of reaching it a thing indifferent. Another side is, that when we are in possession of what promises to be a universal method of cure, we do more wisely in seeking to perfect the application of such method, than in trying short cuts in special cases. The allopath is consistent in being empirical (we are using the word in its etymological sense): he is free because he is lawless. But we are entrusted with the working out of a law of cure, bequeathed to us by one of the discoverers from time to time vouchsafed to our race. We are far from having yet

exhausted its capabilities. Until we have done so, it seems to us that the resort to mere empirical treatment is a dereliction of duty. Once in a way it may be pardoned, but to adopt it as our habitual line of action is to make the extreme medicine of the State its daily food, and must end in the starving out of all method in our practice.

This, however, is by the way. Were we to marshal all our objections to Count Mattei's system, we could fill many more pages. We could argue how improbable it is that seven vegetable medicines should cure the whole tribe of diseases, and that one amateur should have discovered them all. We could show how ignorant is the pathology, and how crude the nosology, upon which the application of the remedies is founded. Above all, we could demonstrate the utter incompatibility of the whole theory and practice with homœopathy, which Dr. Acworth with us professes to hold as truth in medicine. But all these are subsequent considerations. They may become our duty when the mask of secrecy is dropped, but till then we believe ourselves bound to have no art nor part in the matter. The risk of losing a good thing by refusing to use unknown remedies is of small weight compared with the countenance we give to the fraudulent quack by adopting them. And the risk itself need not exist. Let all true physicians resolutely stand against even the trial of secret nostrums, and then the sincere enthusiast, which we gladly believe Count Mattei to be, will have no alternative but to introduce any discovery he may think he has made in a legitimate and scientific manner.

NOTES ON HOMŒOPATHIC PHARMACY.

By MR. THOMPSON.

(Read before the Homœopathic Society of Liverpool.)

To no section of the medical faculty is it so absolutely necessary as to homœopathic practitioners that the curative agents they employ should be prepared with the greatest

care, and in their most thorough purity, and in these few remarks it shall be our endeavour to point out some of the special features of homœopathic pharmacy, especially as they affect physicians' prescriptions.

The want of a thoroughly reliable homœopathic pharmacopœia has for long been felt : those of Drs. Quin, Jahr and Grüner, each in their day and generation aided the onward march of the cause, but it has not until the recent appearance of the admirable pharmacopœia edited by Dr. Madden, that our system of pharmacy has been founded on an undeviating and fixed basis.

The *British Pharmacopœia* of 1867 was a firm foundation to build upon, so far as it supplied *bond fide* tests for identifying and ascertaining the purity of many substances used in both systems of medicine, and, as Dr. Madden aptly remarks, it would have been a work of supererogation to again go over the ground which had already been worked so well, and at so much cost of time and labour. It would perhaps be well, therefore, to dwell for a few moments on some of the distinguishing features of our new pharmacopœia.

First and foremost it differs from those previously in use, that whereas formerly a large group of medicines was promiscuously prepared with the same medium as a vehicle for its reception used indiscriminately to each, now we have each medium separately classified with the exact strength and quality of menstruum necessary to it. This desirable result has been attained through the practical preparation and testing of every medicine by the pharmaceutical sub-committee appointed for that purpose. Thus to each plant is used such a strength of spirit as menstruum, according to the solubility of the active principles to be abstracted from it, and to continue this necessary provision it is directed that the first three decimal dilutions shall be made with precisely the same strength of spirit as was used for the mother tincture. By this means the most valuable properties of each plant are extracted and not again decomposed as might previously sometimes be the case, through using menstrea of an antagonistic character. But I shall have occasion to refer again to this presently.

An important feature in the new pharmacopœia in connection with that just alluded to as regards fresh plants consists in a number of carefully prepared tables, by use of which the pharmacist after ascertaining the percentage of water in a given quantity of moist magma, is able to find out the exact quantity of rectified spirit to be added to the whole mass in order to form with the expressed juice contained therein an alcoholic preparation of the required strength to bring out its own special virtues. This forms the matrix or mother tincture, and as a nearly undeviating rule this mother tincture is arranged to contain one grain in *ten* of the crude substance.

The pharmacopœia committee were for a time undecided whether or not to call this by its *true* name 1^x, but after considerable discussion, the term matrix was again introduced as a foundation from which to prepare the dilutions, and I think very wisely, considering that, prepared under the above process, it must be of a constant uniform strength in each case.

The adoption of the decimal scale was warmly advocated by many, but gave way to the centesimal before several high authorities. Thus, No. 1 is the 1st centesimal, but as an intermediate strength between ϕ and 1 and between 1 and 2 is frequently required, the terms A and B are introduced as correct pharmacopœia preparations, and in order to ensure a more thorough dynamisation in the lower dilutions and as a basis for the higher, we prepare No. 1 from A,—No. 2 from B one in ten, and then proceed on the centesimal scale.

The new process for making tinctures as sketched above certainly requires much more careful manipulation than the method hitherto adopted for the preparation of our vegetable medicines, but being so vastly superior, it is confidently trusted that all true homœopathic chemists will follow it out strictly.

The use of the percolator is also strongly recommended, and from personal experience in its use whilst preparing specimens for the pharmacopœia committee the result has proved highly satisfactory.

With regard to triturations, microscopic experiment has

shown that intense admixture and dynamisation are more thoroughly accomplished by the hand mortar than by any of the machines hitherto in use for the purpose, so the latter are for the most part discarded. The time also for the preparation of a trituration is altered from one hour to forty minutes, this being found sufficient, as they are made from the decimals, and not the centesimals, as was formerly the case. Before dismissing the subject of triturations I would add a word respecting the lowest attenuations of tinctures of the insoluble minerals and earths. Many chemists, some by order of physicians, are in the constant and most objectionable habit of preparing No. 3 tinct. of such medicines as *Merc. vivus* or even *Ferrum* from the 2nd centesimal or on a lower scale still, and thus giving rise to what we cannot doubt both from all experience and microscopic investigation to be a most inert preparation, and from this the pilules and tinctures of the higher dilutions are worked up. It is a point on which we have often had to suffer, but firmly believe it is for the good of homœopathy that such insecure preparations should not be countenanced, and therefore, with scarcely any exception, make the 4th cent. of all minerals, alkaloids, and earths, with dilute alcohol, and No. 5 again with proof spirit, and we would respectfully ask the medical profession to bear us out in this particular.

The question of the selection of drugs whereby their true medicinal virtues may be best obtained is one that should primarily engage the attention of the pharmacist. It must be his duty to select medicines in as complete a state of purity as possible, and in the case of vegetables to see that their medicinal properties are unimpaired.

On this account we have declined to prepare from dried or cultivated specimens plants whose native habitat is not this country, but in all such cases consider it necessary to import the tincture ready made from the fresh plant on its own soil.

Considering this, and that we again have to furnish our native medical plants to other countries, it is of great importance that the same modes of exhaustion, for the sake

of reliable uniformity and for correctness in proving, should be followed out by homœopathic chemists all the world over, and we earnestly trust that our brethren both in America and on the Continent will seriously take our excellent pharmacopœia into consideration with a view to its universal adoption.

I have been requested to say a few words as to the best method of dispensing certain medicines, and perhaps this is the more necessary in consequence of the change in the making of mother tinctures, and the increased quantity of water in place of alcohol therein. Our attention has been particularly called to this subject on account of a prescription for *Conium* ϕ dispensed by us a short time ago. The powders after being kept for a little time were found by the patient to have become quite mouldy (whether from spontaneous generation or no I will not pretend to surmise), but if we may venture to make a suggestion on the subject, it would be that such preparations as the most aqueous mother tinctures and the first dilutions of the same medicines might be advantageously prescribed in the form of mixtures, with the addition of a little alcohol if thought well to keep it good, but probably the latter precaution is unnecessary, except in cases where the medicine is but seldom taken or in hot weather. The same reason, viz., the aqueous nature of the mother tincture, would in cases where practicable, render the form of a mixture superior to that of pilules. The stronger attenuations of acids also, as well as those of the deliquescent salts, *Iodine*, &c., are, we think, more properly dispensed in the form of a mixture than in any other manner.

Perhaps it might not be out of place here to take a hasty run through our pharmacopœia, merely noticing those medicines about the dispensing or composition of which there is anything specially worthy of remark.

Firstly the *Acids*. Those of vegetable extraction, as benzoic and oxalic, make good preparations either as triturations or as tinctures with rectified spirit, the rest, as before noted, can only be reliably dispensed in the stronger dilutions in the form of mixtures. The pure acids as described

in the *British Pharmacopœia* we count as mother tinctures with the exception of *Phosphoric acid*, and this being the hydro-phosphoric we call it 1st dec.

Aconite.—We import two tinctures from Germany, prepared one from the whole plant, and the other from the root.

Agaricus musc.—We prepare with proof spirit from the fresh fungus also a liniment, 1 part of the mother tincture to three parts of soap liniment.

Ambra grisea,
Ammoniacum,
Anacardium,
Copaiva bals. &c. } and others of that class, being of a very resinous nature and readily soluble in ether if not in rectified spirit, we as a rule prefer that method to trituration, but keep the three former in both forms.

Ammonium carb. has been hitherto recommended in trituration, but from its great volatility, we have always found it best dispensed as a mixture.

Tart. emet. 1^x must be trit., and the other low dilutions are preferable in that form, but 2 and upwards can be dispensed as tincture or pilules.

Arg. nit. is best dispensed as a mixture, as the trituration is liable to decomposition, and the dilutions up to 3 are entirely aqueous.

Arnica,
Rhus tox., &c. } We import the mother tincture from Germany prepared from the fresh plant. But for external purposes we prepare tinctures from the imported dry plants.

Bromine can be reliably dispensed as a mixture only.

Cantharis, under the rule of the new pharmacopœia, is, like the rest, one in ten with proof spirit; the result is a very strong tincture eight times the strength of that of the *British Pharmacopœia*.

China officinalis of the *Yellow Bark* is the one most usually preferred, and this we now adopt exclusively, although, probably, the physiological difference between the three kinds is not very marked.

Chininum sulph., unless with the addition of a little *Acid sulph.*, does not form a good tincture lower than the 3rd

cent., it is often prescribed as a mixture in the above form, but more frequently as a trituration in powders.

Cicuta,
Conium,
Æthusa, } and others of the umbelliferous class, being very succulent, are treated with proof spirit, and their early attenuations likewise. The great similarity in the general appearance of these plants renders care in their selection particularly necessary, and we are fortunate in possessing good habitats for them near Liverpool, where we personally gather them. Dr. Christison records a rather curious fact with reference to *Cicuta* and *Enanthe* to the effect that, gathered in their *native* soils near Edinburgh, they appear to possess none of the poisonous properties possessed by specimens native in England.

Crotalus,
Lachesis,
 and *Naja*. } The snake poisons have been so seldom extracted and then attended with so much danger to life that the lower attenuations are necessarily almost extinct. Of *Crotalus* and *Lachesis* we have with difficulty procured a small quantity thoroughly reliable of the 3rd cent., and the 3rd dec. of *Naja*; but a recent communication from our American agents gives hope that a further supply of the two first may be procured before very long.

Hyoscyamus we have found to be considerably influenced by its situation and soil. Some years ago we discovered a very good habitat for it near Parkgate, and have since annually procured an abundant supply from there, growing in a very favoured situation. Like those plants just mentioned the *Hyoscyamus* yields a large quantity of juice, and is prepared in the same manner.

Kali bichrom. we usually prepare as a trituration, but the early attenuations made with water are thoroughly reliable if kept from the light. With the addition of a little glycerine in a mixture, *Kali bichrom.* forms a good gargle.

Kali brom.,
Kali carb.,
Kali hydriod. } can all be prepared in the first cent. with Aq. destill., but we think the form of a mixture is as a rule the most satisfactory way of prescribing any of these. The carbonate especially is too deliquescent to keep long as a trituration.

Lycopodium we prepare both as a trituration and tincture

from the pollen of the *Clavatum*, but think the former way very much superior to the latter.

With the exception of *Corrosive sublimate* the numerous *Mercury* preparations are very sparingly soluble, and below 5 cannot be satisfactorily dispensed except as triturations.

Nux vomica } both prepared as mother tinctures from
and *Ignatia*, } their rasped hard beans with 20 over-proof spirit 1 in 10, also in the form of trituration. It will be seen, therefore, that No. 1 trituration is exactly ten times the strength of No. 1 tincture, and the same rule applies to all medicines dispensed in both forms, except as

Petroleum, when both are prepared from a liquid.

Two salts of strychnia termed

Strychnic nitrate } have been added to the pharma-
and } copœia by Dr. Kidd. We would
Strychnic phosphate } suggest that when these are pre-
scribed the required strength should be clearly expressed unless strict rule is observed, as they are merely classed in the pharmacopœia as $\frac{1}{800}$ th of *Strychnia*, and we propose thus to know them as half the strength of No. 1 instead of styling them mother tinctures, but are quite willing to alter the designation if thought desirable.

Oleum animale, a most difficult substance to preserve; can be dispensed with ether or strong alcohol in the lower attenuations.

Phosphorus treated with either ether or absolute alcohol forms a most reliable tincture styled 'mother,' each of which is about the strength of 1 %. The pharmacopœia directs a trituration, but as the soluble properties of phosphorus are so good we think it would be unwise to dispense it by means of trituration. It is best to leave pieces of phosphorus in the stock solution.

Podophyllum peltatum.—We have usually procured the mother tincture ready prepared from America, as also all the American remedies, none of which we have alluded to on that account, but from the present large importation of *Podophyllum* in the root form, in consequence of the position it now occupies in the pharmaceutical world, we have lately selected good samples and prepared the tincture ourselves,

and with very good results. Our pharmacopœia leaves the question of fresh or dry root in this case an open one, and from examination we are inclined to believe our own tincture to be superior to that which we have procured from America, and for the reason that that also is apparently from the dry root, and does not appear to be so thoroughly and advantageously exhausted as by our improved system.

From *Keith's Podophyllin*, like the other active principles, a mother tincture also is occasionally prescribed, but for the most part these are solely used as triturations.

Sulphur ϕ , as it is styled, is similar in its preparation to alcoholic phosphorus, and from this the *lower* dilutions are prepared. The triturations, however, from the washed flowers, and the tincture and pilules therefrom, are the forms of sulphur most usually prescribed.

This ends the small list of drugs I have selected as types of classes in their mode of preparation. Many others might have been mentioned did time permit.

The future of homœopathy must greatly depend upon the care and attention exercised by its pharmacutists, for upon their conscientiousness the skill of the physician is frequently at stake.

As a science homœopathic pharmacy is, we believe, making sure strides, if not rapid ones, and we hope for the day when each town shall be able to support a purely homœopathic chemist in place of leaving our delicate attenuations to the merciless hands of ordinary drug dealers. The latter practice we believe to be most injurious to homœopathy, as to our knowledge it is a frequent temptation to fraud, often yielded to, and we trust to the kindly help of the medical faculty to warn their patients in cases where *genuine* medicines are procurable in the district.

We are frequently applied to by chemists for tubes, corks, labels, and unmedicated pilules, but *without medicines*; and although we refuse to supply the unmedicated pilules, confectionery houses are now manufacturing them and selling them to chemists on a large scale. To one of these chemists we rather suspected, we applied for *Lachesis* 2 and were at once supplied, proving its nonintegrity.

But this is an unpleasant subject to dwell upon, yet one that, if not discouraged, *must* form a deepening quicksand to homœopathy, and especially debar young men from commencing business as *bond fide* homœopathic chemists in new localities.

In conclusion I would thank you for your kindness and courtesy in inviting me to read a paper before you, and if it may have thrown an additional light upon any point in pharmacy, it has been an instructive pleasure to do so.

A FEW NOTES ON FEAR AND FRIGHT, AND
THE DISEASES THEY CAUSE AND CURE;
ALSO ON THE MEANS OF PREVENTING
AND CURING THE EFFECTS OF THESE
EMOTIONS.*

By Dr. M. ROTH.

1. *Definitions.*

Fear is the dread or apprehension of any real or imaginary object or event.

Fear is a depressing mental impression, caused by a real or imaginary danger by which we are, or believe ourselves to be, threatened, and which we feel unable or not strong enough to bear, combined with an involuntary endeavour to escape the threatening danger.

Fright, terror, is a mental shock based on fear caused by a sudden irruption of a real or imaginary danger for which we are perfectly unprepared; the confusion and concussion of the mind caused by this shock can scarcely be compared with any other sensation. A sudden and powerful

* SYNONYMS.—*Engl.*—Fear, dread, fright, terror. *Lat.*—Metus, timor, pavor, formido, horror, terror. *French*—crainte, peur, terreur. *Ital.*—Timore, paura, spavento, terrore. *German*—Furcht, Angst, Schreck, Schrecken.

impression on our senses, most frequently of sight and of hearing, by a vision or real event, by an unexpected intelligence regarding losses of dear relations and friends, of property, &c., by a threatening danger, precedes the actual shock of the mind, the effect of which has been described as similar to that of a concussion of the brain by an external mechanical force.

Dread is a minor species of fright or terror, but of a more enduring nature.

Cicero considers fear, *Metus*, as the genus, and defines *Timor est metus mali appropinquantis. Pavor est metus mentem loco movens. Formido est metus permanens.*

2. There is also a mental shock caused by unexpected excessive joy, which is similar to that of terror, but its instantaneous, almost paralysing effects are counterbalanced and overcome by the immediately subsequent agreeable sensation of joy.

3. *Pathogenetic symptoms of fear.*

Fear, according to its intensity and duration, causes the following symptoms:—

Skin is pale, yellowish, cold, shivering, contracted like goose skin, covered with cold perspiration.

Brain.—Sensation of anxiety and of pressure; the regular functions are interrupted, loss of memory and recollection ceases.

Head.—There is congestion to the head and face, bleaching and loss of hair, tremor of lips and chin, loss of speech, difficulty of swallowing; the countenance pallid, startled, staring, flickering.

Lungs.—Congestion to the lungs, oppression of the chest, the breathing irregular, interrupted, very deep, heavy, anxious, moaning.

Heart.—Pulse irregular, interrupted, palpitation, pain, even organic lesions of this organ.

Abdomen.—Bitter, acid taste, nausea, eructations, flatulency, vomiting, diarrhoea, colics.

Sphincters.—Relaxed, involuntary excretion of urine and fæces.

Secretions.—Sudden suppression of milk secretion, of lochia and catamenia, of suppuration.

Extremities.—Weak, heavy, shaking, collapsed, powerless, and paralytic.

Temperature.—Martine has observed a decrease of temperature of one or two degrees. Currie's observations prove that the moral condition determines the capability of maintaining the normal temperature. He found a decrease of three degrees under influence of cold, while the same individual under the influence of fear has shown a decrease of five degrees in the temperature of the skin.

Muscles.—Haller mentions that all parts of muscles contract, and the power of bodily action is diminished. *Est affectus quo agendi potentia corporis minuitur vel coercitur.* *Spinal* nervous action is diminished, and the hypostasis of venous blood causes organic lesions; thus cancer and scirrhus are believed to be produced (Lobstein).

The vital turgor of the tissues due to the influence of nervous action is suspended (Lobstein).

General Symptoms.—Rigidity, chronic or tonic spasms, convulsions or cataleptic or epileptic fits, various forms of insanity, morbus hypochondriacus, melancholia, chorea, stoppage of all organic functions, death.

Fear, even of imaginary evils, acts injuriously by a constant nervous tension on some organ, and thus causes danger in its nutrition.

Fear retards the cure of many diseases, the influence of the nerves on the other functions is weakened, appetite and sleep diminish, the digestion is interfered with, the pulse is irregular, slow, and small; the heart, not being sufficiently stimulated by the blood, weakens, the lungs overcharged by the blood are relieved by sighing, the stomach becomes atonic (Devay).

4. *Effect of fright or terror.*

The primary symptoms are a succession of recurring periods of unconsciousness, alternating very rapidly with intervals of consciousness; a *smaller degree* of fright causes quickly passing loss of consciousness, whilst the highest

degree of fright causes total deprivation of consciousness, and even death. As long as the primary effect of great terror lasts, there is almost a perfect cessation of the function of the will, and a momentary general paralysis of all action.

The first or paralysing effects of terror do not depend upon its magnitude, but on the suddenness of the real or imaginary danger or events; they are usually only instantaneous, but will last for a shorter or longer period, according as the terrified individual is able in a calmer moment to consider the event as less terrible.

5. The mania of fear, panphobia, is characterised by involuntary, irresistible, blind terror, which arises and continues without an adequate cause, and is not influenced by reason or religion, not even the removal of the supposed object of alarm. The patients shrink from sympathy, though horror-stricken by gloom; they hide in corners, they run away, they shriek in desperation, they climb trees, and apparently inaccessible places, encounter real danger in order to elude fancied ones, or they are motionless or paralysed; they fear and flee from enemies, police, demons, death, punishment, indescribable agonies themselves.

The primary symptoms of terror are more intense than those of fear, and show themselves in the circulation of the blood; the pulsation of the heart is interrupted by one or several attacks, then several small and frequent movements precede the return of very strong and energetic movements amounting to palpitation of the heart. The face is in the first instance very pale, then reddens, the muscles twitch and contract—the muscles are prominently affected, the chest contracts and sinks in, the sensation of paralysis—the paralysis of fright—is general and in the highest degree lasting, and thus causing even death.

The function of the liver is disturbed, and the secretion of bile either profuse or stopped, and thus fright is the occasional cause of several chronic complaints. The coldness and the pallor of the face, the throbbing of the head, there is a tearing and pressing frontal headache, and profuse cold perspiration on the face and head, eructation and

vomiting of acrid and acid mucus, palpitation of the heart, oppression of the chest, and shaking of the body and limbs, difficulty of breathing, with sensation of heaviness and cold in the abdomen.

Fright diminishes considerably the roundness and size of the body in twenty-four hours (Devay). Organic lesions of all internal organs, especially of the heart and brain, are caused by fear (Devay).

Fear and terror cause irregular pulsation of the brain, the blood is sent with increased force to the brain, and the pulsation becomes frequent and violent. Indeed, the mischiefs that occasionally result from violent mental emotion are more frequently exerted upon the nervous function itself than upon the vessels distributed to the nervous centres; the heart beats irregularly, convulsively as it were, and at times ceases at once to act, so that the individual dies suddenly in fatal syncope. Although the action of the heart is but indirectly under the nervous influence, we know it is powerfully acted upon by the emotions, and in sudden perversions of fear or terror its irritability is at times irretrievably exhausted. The individual does not in such cases die of apoplexy, which begins in the brain, but of syncope beginning in the heart; this last organ, in other words, is the first to die (Dungkinson).

6. As a young physician, I was requested in 1839, in Vienna, by an old man who had already several times paid the debts of his son, to visit his son in the debtor's prison at Clichy, which at that time was one of the sights of Paris, and to send him a report on his son's health. Shortly afterwards I was informed of the sudden death of the old man, and requested by other members of the family to communicate to the prisoner at Clichy the news of his father's death; after having had a preliminary conversation with the prisoner, in which I mentioned that I called at his father's request, the young man spoke in the most bitter and violent terms against his father because he objected to pay a third or fourth time the debts of a profligate son; under these circumstances I did not think that the news of the father's death would have much or any

effect on this heartless son, but I was painfully surprised that, when I calmly communicated the sad message with which I was intrusted, to see him suddenly terror-struck, fall powerless down on the floor. The sudden transition from the state of the greatest rage towards his father into that of perfect weakness of a syncope, was very remarkable, and even now, after the lapse of so many years, the scene is brought back to my mind in consequence of the previous note.

7. Another instance of syncope, caused by fright, which I witnessed, is that of a mother, usually very strong and healthy, who had travelled a long distance to visit her son prostrated by rheumatic fever. She was so much frightened when she saw her son's inflamed and suppurating arms, on which several abscesses had been opened, that she immediately fell into a swoon, although she had never previously had such an attack, or any predisposition to fainting.

8. *Severe paralytic stroke in consequence of fright, caused by the unexpected news of the sudden death of a beloved sister.*—Several months ago I was told by a lady belonging to the highest rank in society, who had just recovered from a most dangerous hemiplegia, with the exception of the flexors of the instep, that she was in perfect health, when she heard that her sister, who was very ill, was improving; two hours later she had a telegram that the sister was dead; this caused the shock to which she attributed her almost immediate seizure. This lady has, undoubtedly, the so-called habitus apoplectic, and was very much attached to her sister.

9. Fear, anxiety, grief, and sorrow, belong to the same class of mental emotions, and make the greatest depredation on the functions and structure of the central organs of circulation (James Johnson).

Fear and sorrow shake the heart, make it flutter and pine away with great pain; and the black blood drains from the spleen, and diffused under the ribs of the left side, makes those perilous hypochondriacal flatulencies which happen to those that are troubled with sorrow (Melanchthon).

Where the heart breaks metaphorically by fear or fright, the main morbid impression is made on the nervous system,

and especially on the great nervous centres. Owing to the concentrated action on these, watchfulness is induced, and the different functions carried on under the presidency of the system, especially the functions of nutrition, become impaired, so that the health declines, the organs are insufficiently nourished, and atrophy gradually dries up the fountain of life (Dungkinson).

10. The influence of fright on the arrest of development of the foetus has been admitted by some physiologists, denied by others, because there is no direct communication between mother and foetus, neither by nerves or blood-vessels, and consequently there is no mode of transmitting the impressions made on the mind of the mother, from her to the foetus. The advocates of the transmission theory say that, although the senses are merely the means of conveying fearful, dreadful, frightful, horrible, and terrible impressions to the brain or spinal chord, these are connected by the nerves with the various organs, therefore it is easily explained why fear and terror cause shortness of breath, irregularity of pulsation, palpitation of the heart, derangements of the circulation, and thus indirectly arrest the development of the foetus, or change its form—facts which have been frequently observed.

11. *Conditions predisposing to fear.*

These are either general or individual, as far as they affect a larger number of persons simultaneously, or only one individual at the time, and they may be divided in psychical and physical ones.

Weakly constituted, sickly, ailing, highly imaginative persons, those who have been shaken by a railway or other accident, convalescents after severe illness, or a surgical operation; those weakened by repeated and great loss of blood, general sickness, or suffering from cephalalgia, giddiness, constant ringing in the ears, irregular circulation in the sensory ganglia, asthma, chlorosis, cardiac affections, disturbance in the circulation of the larger vessels by repletion or plethora; women during the periods of catamenia, pregnancy, confinement, of secretion of milk, and

excretion of lochia; children, adolescents, and adults whose physical education has been neglected, whose mental education has been conducted on false principles, and as children have been living under the threatening influence of punishment, of being frequently frightened in dark rooms, by black men, by bad spirits and spectres, by reading terrible and horrible tales, by a certain unavoidable punishment in future life; those whose mental irritability is increased by mental or bodily stimuli, persons addicted to drink, those whose bodily and mental vigour is weakened by onanism, masturbation, and other sexual excesses, those who are already mentally depressed through any lowering emotions or bodily condition, by misery, sadness, sympathy with others, during great calamities, of epidemic and infectious disease, of war, of fire, inundations, commercial panics, are predisposed to fear, and to suffer more intensely from its effects. The mental anxiety of professional men while conducting an important lawsuit, while attending patients in dangerous illnesses, of speculators and commercial men engaged in affairs exposing them to the danger of bankruptcy, anxious parents nursing their beloved children, and in general students and others who overtax their mental powers, may be added to the long list already given.

Darkness, solitude, silence, sleeplessness, contribute as external causes to the development of fear in those who are predisposed. Threatened or actual loss of property accumulated during a laborious life is one of the frequent causes of the consequent fear of an unprovided future.

In some cases the hereditary disposition to habitual fear has been traced in three successive generations.

Epidemically habitual fear has not only occurred during the general calamities already named, but a singular affection, called *Timoria*, marked by debility, tremor, and terror, has been ascribed to the effects of damp and unhealthy regions in Sardinia and Sicily, the only countries in which this epidemic has been hitherto noticed.

I have fully entered into these various predisposing causes in order to refer to them when the preventive treatment is mentioned.

The uncontrollable pain of James I at the sight of a sword is ascribed to the fright of his mother, Queen Mary, of Scotland, caused by Rizzio's murder committed in her presence.

Murat, "the bravest of the brave," was subject to attacks of fear which for a time unmanned him.

12. *General effect of fear in disease.*

Fear aggravates all diseases, disturbs their course, and causes new symptoms, weakens considerably the patient; children and old people suffer most; cataract, mania, and trembling which lasted twenty years; suppressed secretions and violent spasms are caused by a high degree of fear (Zimmermann).

13. In the Russian campaign of 1812, and in Kabylia (Algiers) in 1852, the French soldiers under the influence of fear suffered from chills and frozen limbs, while those supported by their courage escaped the effects of the cold winter.

14. Fear increases the number of patients in the first period of all epidemic diseases more than the infection and contagion; at a later period, when people are more accustomed to the epidemic, fear loses to a great extent its predisposing effects.

15. *Suppression of suppuration after a surgical operation, and death caused by fright.*

In 1813, two brothers severely wounded, the one in the chest, the other in the leg, were placed purposely in two different wards, in the Hospital of Madrid, in order to prevent any emotion which might have been caused mutually by their dangerous conditions. One of the brothers, whose thigh was amputated, progressed favorably, the suppuration began, and a good result was reasonably expected, when he was suddenly *frightened* by a stupid nurse, who told him that he must hurry if he wished to see his brother still alive. The patient was immediately seized with violent

oppression of the chest, and palpitation of the heart, and shed some tears while calling for his brother. When his wound was to be dressed next morning, the suppuration had ceased, the bandages were dry, the wound was pale, and some blackish spots were visible on the surface, the patient was extremely depressed, and died the following day (Ribes).

16. *Bad effects of fear before and after surgical operations.*

Fear preceding surgical operations prevents the necessary reaction. Civiale quotes the case of a patient who died two hours after lithotomy in consequence of fear. Dufresne mentions the death by fear after the operation of the frænum penis, and Dupuytren after the prick of the skin with a pin (Ribes). Surgical operations on mutilated soldiers and other patients who fear their future distress and misery in consequence of incapability of working and want of means, are usually less successful. A highly sensitive patient had to undergo a surgical operation, which was successfully performed, but she was much afraid of dying in case she should be feverish. Ten days after the operation the wound was in a very good condition, the suppuration sufficient and normal; by chance a surgeon paying her a visit touches the pulse, and says she is slightly feverish; these few words frighten her, the wound dries up, and the next morning she is found dead (Ribes).

17. A man, 63 years old, had his third finger and the corresponding metacarpal bone smashed between two stones. When brought to the hospital there was neither hæmorrhage nor fever, and the patient's mind was calm.

The surgeon proposed the amputation of the wrist as the only means of a cure; the patient objected, and preferred to die rather than lose his hand; the operation was consequently not performed; notwithstanding this the wound appeared in a satisfactory state, the gangrenous parts separated from the adjoining ones without surgical interference, and the patient had scarcely any fever; on the evening of the fourth

day a medical student, who wished to court the favour of the surgeon-in-chief, visits the ward, and says in a brutal manner to the obstinate old man, "You have not permitted the operation—you are lost." Instantly the patient is very frightened, his face is tinged with a straw yellow colour, the skin becomes dry, the pulse small, intermittent, irregular; the next morning at 2 a.m. he was dead. Three days after at the post-mortem *no internal lesion was found* (Ribes).

18. One of Boerhave's pupils was so impressed by the fear of diseases, that he suffered from all the complaints described by his teacher; thus, he went through inflammation and fever during the winter course, and had all kinds of neurotic affections during the summer course, and was finally obliged to cease his medical studies.

19. When I attended the lectures on diseases of the eye I suffered from several nervous symptoms: I had constantly *mouches volantes*, and other symptoms of congestion and amaurosis before my eyes.

20. *Return of gouty attacks caused by fear.*

Werlhof relates a case of a gouty patient who had repeated attacks of gout in consequence of fear and dread.

Morgagni mentions a cardinal who, during an attack of gout, was frightened by the news of the sudden loss of his brother; the consequence was a metastasis to the chest, which caused soon afterwards his death.

21. *Effect of fright caused by the sight of scars or open wounds.*

A young and pretty woman was so frightened by seeing a scar on her bosom after the operation of a cancer of the breast, that she fell in a swoon which threatened to be fatal (Bonnesfoy).

A patient with a wounded hand was frightened by the

sight of his exposed tendons, and died suddenly (E. L. Petit).

22. Effect of fear during pregnancy and after confinement.

A husband was so imprudent as to mention to his young wife expecting her first confinement that he would never love a little girl as much as a little boy ; this preyed on the mind of the wife, and she was in constant fear of giving birth to a girl and dying from grief. Her first inquiry after the birth of the child was regarding its sex ; unhappily she was informed that it was a girl ; immediately a grievous sigh escaped her, and the next morning she was dead (Ribes).

23. Paræsis.

At present I have under my care a child of eleven years, who, when born, had not the use of her limbs ; only when three years old she began to stand and to walk, and at present she has a very considerable weakness of all the limbs and spine. The mother, whose other children are hale and strong, attributes the weakness of this child to a fright she had a short time before her confinement, in consequence of a serious accident to her husband.

24. Fright of the mother alleged as the cause of epilepsy of the baby.

A clergyman whom I know had an accident, by firing a small gun at a country festival ; the gun exploded, and the pieces injured his legs severely ; one had to be amputated, and the other was saved. The wife of the patient was nursing at the time a healthy baby, which soon afterwards was seized by epileptic fits ; these the mother attributed to the shock she had in consequence of the husband's accident, and her subsequent anxiety regarding his recovery after the operation. She continued nursing while attending to her husband.

A young female a few months gone with child visited a brother in one of the hospitals of London, who was wounded in the side ; his condition affected her extremely ; her child

was born with a deep pit in the same part that was wounded in the brother.

Thousands of similar cases have been recorded which occurred to different individuals (Dunlinson).

25. Chronic tremor caused by fright.

I know an artist who, while painting in one of the temples in India, was seized by a crowd of natives, who believed he was committing a sacrilege, and furiously threatened him with a cruel death; he was instantly attacked with a considerable tremor of his arms and hands, which he has never since lost. Happily it has not prevented him from using his brush: it is remarkable that the attention required in handling the brush diminishes momentarily the tremor.

26. Diarrhœa and enuresis are frequently caused by fear.

One of my patients whose moral education was conducted on the principle of constantly exciting his fears of terrible events and future punishments, is, even after the lapse of many years, and after perfect recovery of his physical powers, which had been undermined by the constantly injurious influence of fear in childhood, still liable to diarrhœa and enuresis, when there is even but an imaginary cause for fear.

27. Paralysis caused by fright.

I had a paralysed man under my care, who, being pre-disposed to abdominal complaints, lost the use of his leg in consequence of being frightened by the news of having been robbed of a case which contained his papers and whole fortune.

28. Fear of smallpox.

A smallpox patient, being much afraid of the disease, was not told from what she suffered, but when told she had recovered from smallpox, she died (Ribes).

29. Fright causes insanity.

Fright, which is so dangerous to the female organisation,

may immediately give rise to an insanity which for years may present the physiological effects of fear, that half convulsive, half paralytic state of torpor, of thought, of will ; we may call to mind the similar sudden effect of shock in the production of epileptic attacks. More frequently the insanity originates indirectly by the fright causing deviations from the normal organic processes in other parts, from which then the cerebral proceeds as a secondary result. The effects of fright are explicable, through the influence of the nervous centres on the whole economy. And it is easily comprehensible that these consequences of the emotion of fright are most frequent and most dangerous in the period of life in which the organisation is subjected to the greatest expenditure of force, in order to supply its proper development and growth, and in which it is most capable of disease, viz., at the period of puberty, pregnancy, childbirth, the climacteric period.

Insanity from fright most commonly presents the character of melancholia with stupor, with or without consequent mania (Griesinger).

30. Fear of loss of fortune, and actual loss,

causes suicidal mania either by hanging, drowning, shooting, or, according to various trades and occupations of the patients, by inflicting injuries with the various instruments used by the frightened bankrupts. Many instances of this kind are reported by Esquirol.

31. The loss of fortune, or the fear and anxiety of losing it, caused 150 in 2012 cases of insanity, according to statistics of the Bicêtre and Salpêtrière. (Andanel and Thore.)

32. Theodoric, alarmed by fear of the future, was once frightened by the aspect of a fish which was served at his table, and called out that he saw the irritated face of Symmacus ; he left immediately for his castle at Ravenna, where he died within three days (Gibbon's History).

33. Frightened by the execution of Essex, Elizabeth fell

into a state of depression which led to her death. Melancholy is caused by fear (Hippocrates).

Fear of not being able to survive a false accusation is the cause of our having lost lately an eminent professor of chemistry, who, in consequence of this fear, poisoned himself. He was partially paralysed since infancy, and induced by a high sense of scientific ambition he overworked his brain ; in the evening he returned for several weeks before his death mentally and bodily exhausted from his labours in the laboratory ; the fear was only the occasional cause of the sudden development of the suicidal mania.

34. Fright cause of insanity.

A patient of mine, a young widow, whose two aunts had been insane, attended while pregnant her husband, who during a severe illness had a suicidal mania, and in an unguarded moment escaped to the closet, where he cut his throat ; the fright caused by suddenly seeing the corpse covered with blood caused a state of insanity, during which she was two months later confined ; the mother was in such a state of mental absence, that she knew only a few months later that she had given birth to a child ; she appeared to have almost entirely recovered, and married a second time. During the subsequent pregnancy she was strange, irritable, disliked her husband, but recovered after the confinement. The first three children of the second marriage were girls. After the fifth confinement, being that of a second boy, the insanity again developed under the form of melancholia, with obstinate refusal to take food ; finally she was fed by force, and after six months, again some signs of improvement appear.

In this case the coincidence is to be observed of the return of the mental disease, after the birth of a second boy. While during the previous three confinements and birth of girls, the symptoms threatening mental disease have disappeared.

35. Misers and gamblers living under the influence of

fear of losing what they possess, are more liable to *abdominal derangements* (engorgements) and aneurismal affections (Devay).

36. Corvisart has called the attention of the profession to the fact that during the French Revolution, when all classes were kept in a state of constant agitation by fear and terror, diseases of the heart were very common.

37. A broken heart in its *literal* sense is also caused by fright. Philip the Fifth of Spain died suddenly by the fright caused through the news of the disastrous rout of his army near Piacenza; Zimmermann states that on opening the body the heart was found burst.

38. *Ague cured by fright, and fright the cause of death.*

One of the Dukes of Ferrara suffered for a long time from ague, which his physicians could not cure; his court jester having heard that patients suddenly immersed in cold water have been cured by fright, threw the Duke into the river, from which he was immediately rescued. Although the remedy proved successful, the Duke, wishing to punish the jester for his impudence, ordered him to be executed; the Duke, grateful for his cure, had arranged privately that the execution should be only a sham, and when the head of the culprit was bent to receive the fatal blow of the sword, he was struck only with a towel dipped in cold water; after this operation the witnesses ordered the jester to rise, but were disappointed to see him retain his position, which they attributed to a joke, and were grieved to see that the fear had really killed him.

39. *Fear as a cause of fever.*

Baglivi mentions the effects of fear and fright caused by an earthquake on the 14th of January, 1703, at 2 a.m., in Rome; although nobody was killed, and although the majority of buildings were but slightly injured, the fear which took hold of the Romans caused fever, of which several persons died the same month; others who were

ill at the time suffered from aggravation of their symptoms.

40. Fright caused by fire takes the form of *maddened instinct of self-preservation*, and is the cruellest slaughterer of human creatures in places where large assemblies congregate ; instances of this kind, as in Santiago, and lately in a theatre at Manchester, are but too frequent.

41. *Death of a soldier by fright.*

An American surgeon, Raub (*Medical Times*, 1864), reports the death of a soldier by fright. As soon as the first gun was fired he became uncontrollable, and begged and prayed to be sent to the rear ; his captain threatened to tie him to the cannon if he did not go to his post ; this only made him worse, and it was feared he was becoming insane ; when sent to the hospital he had slight diarrhœa. The most remarkable feature was a *tremor* of his whole body ; his chin and lips trembled so that he was unable to give an answer to any question ; he continued worse in spite of all they could do for him ; he would jump up from his bed, exclaiming that he did not wish to fight, that he could shoot no one, and that he desired to go home ; as the nurses were dressing another man he jumped up and started to get out of the ward. One of the nurses caught him and laid him back on his bed, and ten minutes after he was a corpse.

42. *Fire marks caused by fright of the mother before her confinement.*

The fright caused by fire, by a threatened accident to a beloved child, by the sight of cripples exposing their deformed and diseased limbs in the street, or by other causes, is often mentioned by medical writers as the cause of the so-called fire marks, which cover a smaller or larger surface of the face, and are a kind of angioectasy ; the retarded development, or deformities, or absence of single parts of the limbs, or the presence of a sixth finger or toe, has been frequently attributed to fright during pregnancy.

43. *Deformities of all the limbs caused by fright of the mother.*

The following case came lately under my observation.

Mrs. —, 21 years old, fair, healthy-looking, was married seven months ago, and was pregnant since the first week of her marriage; has been always in good health. She complained of a pain in the right side, similar to pleurodynia, caused, as she thinks, by some unkind and quarrelsome treatment of her landlady; being a foreigner, and unable to speak with an English accoucheur, she called to ask my advice regarding the pain, and partly regarding the approaching confinement. She was soon relieved, but in a short time later she felt one morning some water escape from the womb. An accoucheur was immediately called in, who examined and advised her to be quiet, and held the hope out that she would not be confined before several weeks; notwithstanding this opinion the water continued to escape, and a week later she felt labour pains, and before the accoucheur was in the house she was confined prematurely of a very little girl, with a nice but small well-developed head and body, while all the extremities were more or less deformed, as seen in the accompanying engravings, made from drawings when the child was ten days old. Having been requested to examine the child, I tried to find out the possible cause of these deformities. Both the relations of the father and mother are healthy, there is neither a hereditary nor acquired complaint or deformity in either family; but during the first months of their marriage the newly-married couple had been in Paris in the Jardin des Plantes (Zoological Gardens), and notwithstanding the husband's caution not to look at the serpents the young wife answered that she had often seen serpents, and was not afraid to look at them; they were shown the compartment of a small but very venomous serpent, with a wire grating inside the thick glass, which formed the front of the cage. They could in the first instant not see the serpent, but immediately afterwards the serpent suddenly raised its head, and with a noisy clash on the grating, showed itself almost opposite the faces of the spectators, who, forgetting at the moment the thick glass of the cage, were much frightened,

believing themselves to be attacked by the serpent, of whose dangerous venom they had just heard. The young wife

FIG. 1.—Right hand, dorsal aspect.

FIG. 2.—Left hand, dorsal aspect.

FIG. 3.—Right foot, dorsal and partial
plantar aspect.

FIG. 4.—Left foot, dorsal and partial
plantar aspect.

thought only that and the following days several times of the
fright caused by the serpent, but had entirely forgotten the

accident, which she did not mention during the few visits she had paid me. Only the day after her confinement, when the unhappy state of the baby could not be concealed from her, she remembered the fright she had. I am inclined to attribute the arrest of development of the fore and middle finger of the right hand and the deformities of the other limbs to the effects of the terror which took place at the period when the germ of the extremities begin to develope. The baby, being tongue-tied, could suck neither the mother's bosom nor a feeding bottle, and I was told that she was considerably thinner than immediately after birth. As the baby had jaundice, and could not be fed, it is not probable that she will live more than a few days longer.*

In case she should grow up the left hand can be made perfectly useful by dividing the connecting membrane between fore and middle finger, and between ring and little finger ; the right hand can be partially useful by dividing the connecting membrane between the ring and the little finger, which, with the thumb, could be very well used for grasping, holding, picking up, sewing, and many other occupations ; the arrested fore and middle finger of the right hand might serve as stumps and supports for artificial fingers ; the right foot permits at present the reposition into its natural form by slight manipulation, consequently the deformity of this foot can be cured without the use of the knife, only the left foot would require tenotomy of one or two tendons, in order to replace it as far as possible with the assistance of a spring apparatus in its normal position.

44. Fear of a growth on the nose and of consequent ugliness.

About thirteen years ago I was asked in Brighton to see a girl of about 17 or 18 years old, who looked the picture of health and beauty ; she was an Italian ballet-dancer, had been for some time depressed and suffering, thus unable to fulfil her engagements, and was sent by the manager of one of the London opera houses to the sea side for a change of air, and to be placed under necessary medical attendance ; her aunt told me that it was her first season in London,

* The baby died a few days after I had seen her.

she had never before left Italy, and that for some time she had failed in her appetite, was depressed in spirits, but did not complain of any bodily ailment. Being able to speak with me in her own language, as she did not know any other, had a cheering effect upon her, and I found that the principal cause of her despondency and failing spirits and want of appetite was the fear of a growth on the tip of her nose, and of the subsequent deformity and ugliness of her face. Although this appeared in the first instance very ludicrous I found that this fear had assumed a very serious form; my placing her before the looking-glass, the touching of her nose with her own fingers, my reasoning with her,—all this was of no avail. She persisted in her seeing the growth on the nose and in her fear of having a deformed ugly face.

Finally I found a tendency to constipation and masturbation as the only physical causes of this strange mental derangement. Change of diet, a course of mineral waters, moderate exercise in the open air, her being watched by her aunt, not to indulge in the abnormal excitation of the sexual organ, and my threatening her with greater ugliness in case she should persist in her bad habits, have been the means employed which, within six weeks, completely restored her and removed all fear.

The following two cases are also taken from my own practice, and show the various kinds of fear:

45. Fear of a spinal curvature.

Miss —, about 35 years old, consulted me about a curvature of the spine from which, according to her statement, she suffered. After a minute examination I did not find any trace of curvature, but only a very slight weakness, and, notwithstanding my assurance to the contrary, she would not believe me that her spine was perfectly straight. After questioning her more closely, I found that she had suffered a short time before her visit from copious uterine hæmorrhage, and that one of her aunts had a serious spinal curvature; thus, I conclude that, having been weakened by

the great loss of blood, she was more nervous, and was in constant fear of a spinal curvature. In consequence of her repeated request, I permitted her to go through a course of hygienic gymnastics, and in the course of a fortnight even the previous slight weakness in the spine had disappeared ; but, notwithstanding this, she still continued in her fear of a spinal curvature.

Although she lived in a remote country, she came several times to town within intervals of several months with the principal object of having the spine examined, as her fear still continued, and she still pointed to the same spot as being curved. Neither the double looking-glass nor my assertion that I would willingly begin a treatment if it were necessary would persuade or convince her that she was perfectly straight ; only at her last visit I found her more calm in this respect, but she still insisted upon my looking at her spine. I may add that her general health was very good, and she had no complaint whatever.

46. Fear of curvature.

I remember another case of a female patient, with a hysteric tendency, and fear of a spinal curvature, who has worn an orthopædic spinal support at least two pounds in weight for several years, although she had no trace of a curvature. In this case the patient was unhappily confirmed in her imaginary complaint by a surgeon of one of the orthopædic hospitals, who has prescribed the steel apparatus. In this case I had less trouble of convincing the patient of the absence of the curvature, not only because I had her sister's assistance, who was present when I was searching for the curvature, but also because the fear of the imaginary evil was not as deep rooted as in the previous case. Having for years been in the habit of leaning on the crutches of the orthopædic apparatus, the slight inconvenience caused by the removal of the support under the armpits was soon neutralised by the use of a few of the most simple active gymnastic exercises which the lady could soon carry out for herself, and which increased her general state of strength.

47. *Effect of a shock caused by excessive joy.*

Having mentioned the similarity of the shock by excessive joy to that caused by fright when excessive, the effects of this emotion might prove of interest. "Death by excessive joy is not unusual; the dangerous effects of the shock by joy are more frequent than those of a grievous, painful emotion, because the impression on the mind is too powerful, and the sensorium cannot resist its *attack*" (Devay).

48. Fontenelle mentions that the wife of the only heir of Leibnitz's treasure was so overwhelmed with joy at the sight of such a large sum in her possession that she died suddenly.

49. Sometimes cerebral derangement is caused by the same circumstance. Richard Meade has long ago and frequently observed mental alienation, caused by the excessive joy in consequence of people coming into possession of an unexpected fortune.

50. A servant girl went to Paris in search of a place; by chance or by a mistake she missed the right door, and entered the house of a known artist, M. Foressier, who just wanted a servant. The girl continued for years in the service of this artist, and by strict economy and order contributed very considerably to the accumulation of her master's fortune, who left her at his death a fortune of 800,000 francs (£32,000 sterling).

This change of fortune caused hallucinations of fear. She fancied herself pursued by enemies, and therefore obstinately refused to leave the house. The consequence was that the heirs asked her sequestration.

Dr. Frelat, who reports this case of mental derangement, says, "There may be something as injurious as misfortune to the normal state of the intellectual functions, and that is the excess of happiness."

Devay maintains that the shock caused by excessive joy is

still more injurious to those whose mind has been previously tormented by the contrary state of fear. Thus, according to Valerius Maximus, two Roman ladies suddenly died; one while she was embracing her son, whom she found at her door, and the other at the arrival of her son, for whom both had grieved as being lost in the battle of Canæ. They had been able to overcome the effect of grief, but not that of the excess of happiness.

51. *Therapeutic effects of fear and terror.*

Fear deranges the organic functions by a violent and sudden mental impression, which changes forcibly the course of pathologic processes; thus, fear, by causing a different direction in the solids and liquids, exerts energetically a salutary reaction (Ribes).

Are we not right to believe that a power capable of causing diseases may also change their course, and that the same power which can aggravate it and make them fatal may also cure it (Devay).

Under the influence of particular emotions some of the secondary functions are acted upon, and, amongst these, those concerned in defecation and excretion; anxious dread or excessive fear has this effect in a marked manner; and it has been probably experienced by every one under such circumstances. Certain emotions may, therefore, be looked upon as *mental* cathartics, although as such not capable of being invoked in the treatment of disease (Dunghinson).

The same author mentions among the mental antispasmodics, abstraction, powerful emotions, fear, &c.

Fear is an eminently conservative function (Ribes). This may justify sometimes its application, which is to be done with much judgment, because the power of this emotion is very great and even fatal, which has been mentioned in some of the preceding cases.

52. *Intimidation*

has been successfully used in infancy and youth, because

at these periods the effects of slight fear are more quickly transitory; but in children predisposed to fear its use might be dangerous.

A similar impression on the adult should be used only with the greatest caution, and the physician must well appreciate the circumstances and condition of his patient. It is well known how even apparently the most indifferent patient studies the physiognomy of his doctor, and how satisfied he is when the latter does not betray any emotion, and his features remain quiet.

53. *Fear a remedy in hæmorrhage.*

Serre, professor at Montpellier, mentions a case of lithotomy performed by a renowned surgeon on a person of high rank. The inflammation which followed was checked by profuse bleeding. A few days later a hæmorrhage through the wound took place, and the patient, having lost already so much blood, could not be bled again, and all styptic means and remedies failed. The surgeon thinks seriously what he is to do, when the almost dying patient asks him whether he cannot try some other means of stopping the loss of blood. The surgeon very quietly and solemnly answers, "No, my dear sir, it cannot be helped; you must die." These few words frighten the patient to such an extent that he loses consciousness, falls into a syncope, but the hæmorrhage is stopped, and from this moment the recovery begins.

Similar instances of arresting hæmorrhage by painful and frightening impressions caused for the purpose by the medical man are recorded.

54. *Fear a remedy in hectic fever.*

Feuchtersleben quotes the following from Dr. Herz:—"A patient was in the last stage of hectic fever; the physician felt it his duty to support him by hope; the patient was conscious of his dangerous state; the struggle between these two sensations of hope and danger kept up and

increased the fever. Finally, the medical man resolved to take the hazardous step of saying to the patient "that he is lost." First great agitation, and then sad depression followed this announcement.

In the evening the pulse was regular, the night more quiet than many preceding ones; the fever abated from day to day, and in the course of three weeks the patient was cured.

55. *Fear a remedy in gout and rheumatism.*

Instances of gouty and rheumatic patients having thrown away their crutches, and having made use of limbs which had not been moved for days and weeks when frightened by fire, inundations, or by any great danger, are frequently quoted by medical authors. About a year ago a report in the French papers stated that a patient paralysed for years, whose wife was murdered in his presence by a servant, recovered through the fright the use of his limbs. Although this report has not been well authenticated, its possibility may be admitted.

56. Falmuth records the case of a gouty patient suffering from great pain, and who applied a poultice for his relief. Being much frightened by a large pig rushing suddenly into the room and beginning to eat the poultice, the patient lost the pain and ran away.

57. *Fear a remedy in epilepsy.*

Fourteen girls in the House of Correction at Sonnenburg, who enjoyed previously good health, were one morning seized by epileptiform spasms, probably in consequence of seeing the fits of two patients suffering from epilepsy. The physician, attributing this sudden epidemic to a psychical cause, ordered that large bundles of birch rods should be hung up in the female wards, and it was announced that each prisoner would be struck by these rods as soon as attacked by the spasms, and as long as they should continue. The fear of the punishment had the desired effect, and the spasms ceased (Zimmermann, *Medic. Zeitung*, 1838).

58. Cullen mentions the case of an epileptic child, for whose cure many remedies had been used without any benefit. This child, when sent to the hospital, was very much frightened by seeing some soldiers mounting guard, and was cured from that instant.

59. *Fear a remedy in paralysis.*

Diemerbrock quotes a paralysed patient, 40 years old, who owed his cure to the fear caused by a thunderstorm.

60. *Chorea.*

Dr. Roth, of Paris, in his *Histoire de la folie Musculaire*, quotes Boerhave, as having stopped the spread of an epidemic chorea amongst school girls through the fear caused by threatening to treat them by the application of a redhot iron along the spine.

61. *Suicidal mania.*

At Milet, a moral epidemic inducing suicide among the young girls was stopped by threatening to exhibit, quite naked, the body of the next who should kill herself (Ribes).

62. *Hysteria.*

A lady suffering from a violent hysterical suffocative fit, which could not be relieved medicinally, was cured by fear of having her throat burnt with a paper which her physician prepared in a suitable form, and then lighting approached her with the apparent intention of introducing the burning paper into her mouth (Ribes).

63. *Descent of the womb,*

unsuccessfully treated by other means, was replaced by spasms in consequence of fright, caused by a mouse which, tied by one foot, was permitted to run under the petticoat of the patient, who, quite unaware of the new proceeding, felt suddenly the movements of the animal between

her legs, jumped up and ran through the room in a state of great agitation (Fallen).

64. *Dislocations of the joints*

have also been cured by fright. Dupuytren used fear methodically as an anæsthetic to relax the action of the muscles, which permitted the easier reduction of the disabled limbs.

65. *Intermittent fever.*

Fright has been frequently used in chronic intermittent fever when the usual febrifuges did not cure. A powerful emotion has been found to produce an effect similar to that of a tonic, and has completely prevented an expected attack, and even after its inception has removed it. Accordingly fear is placed by many practical writers amongst the febrifuges employed in ague (Bingham).

Cold water is poured down the neck and spine at the beginning of the cold stage without the patient being aware of the affusion. Draughts containing spiders and other disagreeable insects are presented to the patient as a drink, who, after beginning to drink, is frightened by the aspect of the insect.

66. *Onanism, masturbation,*

and venereal excess are considerably restrained by the dread of the terrible consequences of these vices impressed in an earnest manner by the medical attendant on the patient's mind.

67. *Loss of the use of limbs.*

A paralysed woman, apparently dying, was in the Hôtel-Dieu, when fire broke out in one of the wings. When she heard the tumult caused by the fire and the cracking noise caused by the flames, she was frightened to such an extent that, without thinking of her hitherto powerless limbs, she rose suddenly, ran from the hospital to her home, a considerable distance from the former, and felt herself completely restored (Ribes).

68. *Rage, mental excitement, and their dangerous consequences.*

For years I have attended a patient who was liable to attacks of rage (*colère*) caused frequently by insignificant circumstances; he used to tremble, to flush in the face, and to be very excited. Repeatedly he was soothed by the fear caused by this statement, "You will have an apoplectic stroke, and either die suddenly or remain paralysed for life, if you continue in this state." He died suddenly abroad while in a state of a similar mental excitement, and when there was nobody near him to recall the danger to which he exposed himself through his rage, and to exert on him an alarming influence by the fear of death or paralysis.

69. *Fear a remedy in enuresis nocturna.*

In a charitable institution at Berlin a large number of children suffered from enuresis, and the servants, unable to preserve the cleanliness of bed-linen, asked the physician for advice; he (remembering Boerhaave's proceeding) placed twenty-three children, specially pointed out as the greatest sufferers, in a circle, and in their presence placed several small iron rods in the fire till they were red-hot; he then very seriously reminded them that he, being the medical man of the institution, was not there to punish them but to cure their severe ailment by slightly touching them with the red-hot iron. Notwithstanding their copious tears, three of those who suffered most were slightly touched on the bare upper arms, afterwards he told them to wait another week, and in case the complaint should not cease during that interval, he would again select three others to be operated upon. The result was that the very careful superintendent of the establishment stated in a detailed report that one of those who had been operated upon was perfectly cured, the two others had only rare returns of the enuresis, while eighteen of the spectators had been cured and two only of the spectators continued to suffer (Casper, *Med. Zeitung.*, 1834).

70. *Fear a remedy in hernia scrotalis.*

It is known how powerfully a fear or terror acts on the peristaltic motions of the intestines. A distiller of brandy suffered from incarcerated scrotal rupture; phlebotomy, warm baths, and all other known means were employed without effect. During the consultation of several medical men in presence of the patient, one of them seriously insisted upon the necessity of a surgical operation. As soon as the patient heard the word "operation" he appeared very frightened and almost instantaneously exclaimed, with the greatest joy, "My rupture has disappeared" (Teuffl, *Rust. Magazine*, 1828).

71. *Fear a remedy in aphonia, dumbness.*

The son of Croesus was dumb: when the oracle was consulted, the answer was, "It will be an unhappy day when Croesus' son will speak." So it happened after one of the wars in which Croesus was vanquished he was threatened to be killed in the presence of his dumb son, who, seeing his father's danger, exclaimed, "Don't slay the king!" This saved the king, whom the soldier who intended to slay him did not know; he then preferred to lead him as a prisoner to his chief (Herodotus).

72. *Fear a remedy in mental disease.*

In mental diseases fear judiciously employed has often good results. The hypochondriacal maniac and those with similar tendencies are restrained by the fear of the future, by fear of a refusal of a decent interment, and by the fear of a cold ablution or cold douche, or immersion in cold water.

73. *Lypemania.*

A female patient, thirty years old, suffering from lypemaniac delirium, was placed in an asylum and treated by isolation and baths of various kinds. A fortnight had passed without any improvement, when one of her children died suddenly after meningitis; with the consent of the

husband the physician wished to make use of the fright caused by the news of the sudden loss of her child for the cure of the mother. For this purpose he communicated to the patient without in any way preparing her for the sad news—the loss of her beloved child—in order to cause a sudden and deep emotion on the mother. Without obtaining immediately any result, as was expected, the means adopted proved soon successful, as the patient was better the next morning, and the third day the madness had ceased. Devay, who had watched the case, reports that the patient had no return of the mental disease.

74. Hydrophobia.

An English servant was frightened by the report of a horrible death caused by a rabid dog, and was immediately seized with hydrophobia, and cured only by the application of remedies used against this disease (Devay).

Dr. Devay mentions, also, that he has attended a young medical man attacked by hydrophobia symptoms, after attendance on another colleague who died from hydrophobia.

75. Toothache.

The fright caused by the dentist's instrument often instantaneously removes the most violent toothache and prevents the patient from submitting to the operation of the dentist. A clenched fist threatening a blow on the face has, according to popular belief, a similar effect on the relief of toothache.

Preventive and curative treatment.

Moral as well as hygienic means must be used either alone or in combination. The predisposing causes to fear must be as far as possible removed, or where this is not in our power, they are to be lessened and modified; the constitutionally weak and ailing, convalescents, and those weakened by preceding severe disease or surgical opera-

tions, must be strengthened ; all external conditions depressing them physically and mentally should be kept off ; patients under fear of the result of a surgical operation, and women afraid of death at an approaching confinement, are in a quiet, cheerful, consoling and reassuring manner, to be persuaded to be calm ; we must try our best to remove the patient's idea of a threatening danger, or their strong conviction of an inevitable death ; we must force ourselves to remain as quiet as possible, not to betray in our features any uneasiness, because the patients trust more to their eyes than to their ears, and therefore listen less to all we say, and scrutinise carefully the expression of our face. Sometimes it is even necessary to show some disappointment, and speak with more energy and slight anger in order to impress more forcibly the patient with the unreasonableness of his apprehension. It is quite understood that all those who surround the patient, relations, nurses, and others, must contribute in a similar manner to dispel the patient's fear and anxiety ; thus an irregular circulation, an oppressed respiration, spasms, an abnormal state of digestion, want of appetite, which are only concomitants in such cases of fear, will be removed by the friendly, calm, and consoling features and words of the physician and his subordinates ; only those are to be permitted to be near the sufferer who can inspire him with hope or bring him consolation. What consoling influence the quiet, kind care of the sisters of charity and of voluntary nurses have in the ambulances and hospitals on the wounded we hear daily during the present internecine war in France, where as in every war the wounded suffer much from fear, which appears partly the consequence of the shock caused by the bullet on the frame, partly of the sight of their fellow sufferers, and of their frequent death.

The following advice was given by Petit :—

“ Nevers place two patients suffering from similar complaints near each other or together, because they will watch each other and by looking at their common pains and sufferings think how much they have to suffer ; one will fear beforehand

the evils of which his neighbour suffers, and if the disease terminates by death, the survivor is struck an hundredfold with the fear of death; he sinks into a state of moral depression, loses all hopes, and reaches his end in a more painful manner.

“ Study the character of your patients, their sensitiveness and impressionability, be careful what you say, watch the talk of the visitors and friends, especially after a surgical operation.”

When girls and young women retain *marks* and *scars* after diseases and wounds, it is desirable to prepare them for the loss of their good looks, and to call their attention to other charms; because after a disease or operation when they look the first time at the looking-glass, they are sometimes frightened to such an extent that the consequences are very serious. Persuade them in a reasonable manner to pay less attention to their changed features or scarred bosom and neck (Ribes).

Everybody knows how desirable it is not to expose women in confinement to the effects of fear; for this reason even the officers of justice were prohibited by the laws of the town of Haarlem to enter those houses wherein a woman was confined, and which were marked by a signal placed outside (Van Swieten).

Prevention by consolation.—The importance of moral means in the prevention of bad effects of fear is proved by the following case reported by the surgeon-in-chief of the Hôtel-Dieu at Clermont Ferrand.

A coachbuilder cut accidentally the radial artery; the ligature was performed; he was a good paterfamilias, but his means were restricted, and he had two little children unprovided. After the ligature of the artery he was much agitated, depressed, and grieved; in fact, he was seized by fear of not being able in future to make use of his hand, that he would die of his wound, and leave his children as unprovided orphans: he had neither a friend or near relation who might have called on him in the hospital; the moral suffering was greater than the local, caused

by the wound, and he got rapidly worse. The surgeon in attendance reports,—When I came near him the first time, he said with a very tremulous voice, Sir, if you have not pity on me I shall die ; I want not only to be dressed, but my heart is oppressed, and I am indeed very, very unhappy ; grief and *ennui* will kill me if you are not kind enough to console me, and I feel that my little children will soon be without a father. The surgeon made use of all suitable means to counteract the patient's fear and sadness, and ten weeks later he left the hospital, although abscesses and erysipelas had developed in succession on the hand and the forearm.

Preventive treatment by education and hygiene.—Develop by instruction and good example the knowledge of the value of work and money ; make the acquisition of money not the only and principal aim of life, but teach its right use by habits of providence and saving, by teaching frugality, by showing the consequences of prodigality and extravagance, even of those whose station and position enable them to enjoy many luxuries of which others are deprived, and by the sad effects of incurring debts or entering speculations in the vain hope of a sudden acquisition of riches.

The paragraph on the predisposing causes to fear and fright is sufficient to prove the importance of education as a preventive of fear and its effects.

Change of air and scenery is frequently useful both as a preventive and as a curative ; a bracing, invigorating place, and much exposed to the sun, is preferable to any other when the depressing influence of fear is to be lessened and courage to be raised.

Where the assimilation is deficient, the proper diet is to be selected ; small quantities of wine and more of a stimulating animal food are to be taken ; according to circumstances tepid or cold bath in form of shower, or douches along the spine, or in form of effusions on the head, or a hip and foot bath to be applied.

The influence of mind and body being reciprocal, it is a

fact that mental emotions manifest themselves by external signs, various positions and movements of the body, change of features, &c.; in the same manner an intentional position, action, or movements of one or all parts of the body reacts on the mind: thus, the depressed will be cheered up, and the sufferer from fear will be encouraged.

Well-directed hygienic or medical gymnastic movements have been recommended by many authors for the increase of the bodily vigour, and power of will for the development of presence of mind, self-reliance, and courage.

Sufferers from fear, especially those in childhood, youth, and in whom fear assumes the character of a real ailment, derive great benefit from a gymnastic treatment, but this must be combined with a psychical one; instead of entertaining their minds by tales of misfortune and wonderful events, facts and events should be brought before them where firmness of will, presence of mind, and examples of courage are prominent.

The instantaneous or immediate treatment consists in the quick removal of the primary and more severe effects of fright and terror, especially of the syncope, swoon, and arrest of circulation by loosening the dress, and everything tight round the body and limbs; in longitudinal friction of the arms and feet, in one direction only, namely, towards the periphery, sprinkling the face, neck, and chest with small quantities of cold water, quickly and forcibly thrown towards these parts, change to a horizontal position, fanning of fresh air towards the patient's face; windows should be thrown open where the room is close, smelling salts, empyreumatic or aromatic substances kept before the nose, in fact, all is to be done that is required for revivifying the patient. Some few stimulants, as wine, brandy, cognac, &c., are to be given as soon as the patient is able to swallow, and when the respiratory function is well restored, great rest and quiet is desirable, while the patient is kept warm, without oppressing him with blankets, clothes, or coverlets.

Medicinal Treatment.—The old school continues to make use of the treatment by tonics and stimulants, interspersed

with aperients and cooling draughts. The new school selects, among the list of the following medicines, according to the various forms of fear, and according to the simultaneous circumstances in which the various symptoms occur.

Opium counteracts the primary effects of fear and terror, and prevents the dangerous consequences.

Aconite when the mental shock has taken place some time ago.

Crocus—the sensation caused by excessive joy, approaching madness, with pallor, headache, and confused sight.

Cantharis—fear of imaginary evils.

Carduus benedictus—fear, with starting at every noise and cold perspiration.

Ignatia—fear of every trifle, especially of approaching objects.

Lycopodium, Opium, and Veratrum—great fear of imaginary phantoms.

Calc. carb., Sepia—fear of consumption.

Ranunc. bulb.—electricity—fear of being alone.

Ignatia, Zincum—fear of thieves.

Calc. carb.—fear of misery.

Colchicum, Chlorine—fear of being unable to bear sufferings.

Aconitum, Calcareia, Mercurius, Causticum—fear of falling.

Ruta—fear of being captured (imprisoned).

Zincum—fear of horrible phantoms.

Carbo veg.—fear of not being able to do anything well.

Staphisagria—fear when walking quickly.

Lycop., Phos., Stram.—fear of terrifying images.

Arsenicum—fear of himself.

Cuprum—fear of vigorously walking.

Cocculus—fear of surprises.

Calc. carb., Tannin—fear of mental derangement.

Hyoscyamus, Stramon.—fear of being bitten by animals.

Natrum mur., Stram., Chlor.—fear of insanity.

Calcareia, Mercurius solubilis—fear of losing his reason.

Calc. carb., Dulc., Staphisagria—fear of the future.

Belladonna, Calc. carb., Digit., Moschus, Natrum muriatic., Nitric acid, Raphan. sativ., Squilla, Vinca, Aconit., Arsenicum—fear of death.

Arsenicum, Carbo vegetabilis, Pulsatilla, Ranunculus bulbosus, Sulphur—fear of spirits.

Pulsatilla, Ranunculus—fear of spirits in the evening.

Carbo vegetab., Sulphur—fear of spirits in the night.

Belladonna, Drosera, Hyoscyamus, Rhus—fear of being poisoned.

Dr. Richardson has lately in his address to the members of the St. Andrew's Medical Graduates' Association, called their attention to the physical study of emotional phenomena, and he mentions the following facts which partly explain the effects of fear and fright, and which suggest to the advocates of *simila similibus* the use of a new medicine to counteract these effects.

(a.) The section of the sympathetic trunks, leading to an organ, or part, causes paralysis of the vessels of the part, so that they fill with blood, and for a time give off excess of heat.

(b.) The sweet fruity-smelling fluid called nitrite of amyl, when introduced into the body, produces the same kind of paralysis (as the section of the sympathetic); so that we can, by its means, charge the blood-vessels with blood to such intensity of fulness, and so easily, that a little vapour diffused in the room would quicken the motion of every heart, and flush every cheek with crimson.

(c.) Unseen agencies which excite the emotional phenomena of blushing, palpitation of the heart, pallor, faintness, act in precisely the same physical manner, viz., by producing paralysis on the controlling sympathetic power.

(d.) Emotional phenomena are the results of physical vibrations or shocks, conveyed from the outer world through the senses to the organic nervous centres; these centres lose, under the physical impression, their controlling power over the vessels under their charge, and that all the phases

of an emotional phenomena, from flushing to paleness, from paleness to pallor, from pallor to syncope, and from syncope to death, are but grades or gradations from one physical change to another, the whole resting on the primary shock conveyed to the organic nerves.

(e.) The emotional effects produced by the nitrite of amyl can, when not overpoweringly called forth, be opposed to a certain extent by the force of the will.

(f.) Persons most easily influenced by what are called emotional causes are also most easily influenced by the nitrite of amyl; and a chemical test might be employed as an actual physical test of the organic susceptibilities of different individuals.

To those who have had the patience to peruse this paper it is my duty to apologise for offering only such a fragment on a most interesting subject, but my aim was—

1. To moot the question whether a suitable treatment might not enable us to diminish the bad effects of fear and fright during pregnancy, and indirectly on the arrest of development of the foetus.

2. To show that some of the diseases caused by some emotions are also curable by similar ones.

3. To induce those who have more talent, leisure, and experience to pursue the subject on the effects of the various emotions, and to show to what extent they can be used as curative agents.

ON THE ACCESSORIES OF HOMŒOPATHIC PRACTICE.

By R. M. THEOBALD, Esq., M.A., M.R.C.S.

(Read before the British Homœopathic Society.)

PRACTITIONERS of homœopathy may be variously classified according to the different points of view from which they are regarded. But perhaps all varieties may be included in the contrast between those who make specific homœopathic treatment secondary to the physiological treatment of disease, and those who make homœopathic treatment primary, and all accessory treatment subordinate. The difference between the style of practice in the extreme specimens of these two classes is very remarkable. The practitioner who relies almost exclusively upon the medicine which he selects according to the homœopathic law is most careful in investigating the case before him to its minutest features. He will spend half an hour over a pain or an abnormal sensation that he may most exactly ascertain its characteristics, conditions, concomitants, antecedents, and consequents. After this is done the comparison of symptoms with medicines follow, till the remedy is determined, and then the work is completed. He has few and simple directions, chiefly negative, to give about diet, the principal object of which is to prevent the intrusion of any adverse influences that may counteract or impede the action of the selected remedy. But here his directions begin and end. He has very little active treatment to suggest directed to the symptoms or external manifestations of the disease. They are not to be attacked physiologically lest the specific medicine should be counteracted by its coarser rival.

On the other hand, the practitioner who relies mainly on accessory treatment is usually less careful to investigate the minute peculiarities of the case, and is sometimes inclined to censure such microscopic attention to small facts as puerile and futile. He aims at a general accuracy of

diagnosis, according to the standard of the pathological schools; and, having attained this by a tolerably summary investigation, he has not much hesitation in deciding upon the remedy which he thinks adapted. But the medicine is often the least important element in treatment. He abounds in all sorts of resources derived from physiology, chemistry, hygiene, hydropathy, kinesipathy, mesmerism; he incorporates into his practice all the most recent therapeutic devices of the *Lancet* or the *Practitioner*; he does very much more outside the body than inside, though he does much there also; the *primæ viæ* are high roads for enfilading a most *recherché* collection of culinary curiosities and delicacies, while the whole surface of the body is a *rendezvous* of plasters, poultices, fomentations, lotions, embrocations, galvanic plates or wires, and every variety of apparatus that surgical art or hygienic ingenuity can invent. The chemistry of digestion and assimilation suggest most learned contrivances for helping or modifying the action of the stomach, which straightway becomes a laboratory where theory can be brought to the test of experiment. He has very decided opinions about the pathological changes that have been produced by the disease and the consequent physiological mark which he must hit, and he does his best to manipulate the offending organ or function into better behaviour, by treating this, which he regards as the focus or head quarters of the disease, as directly as possible. If he can bring the disease or any part of it within arm's reach he gains his finest opportunity, and he lengthens his arm by every conceivable contrivance till he can directly or indirectly touch every mucous membrane and nearly every organ or gland in the body.

These two contrasted types of treatment may more or less approach one another in many ways: the rigid devotee of specific medicines may open various loopholes for other expedients, and the ingenious performer on the complicated instrument of therapeutic physiology, with its many keys and manuals and stops, may sometimes cease his multitudinous operations while he awaits the undistracted action of a medicine in whose specific virtue he has more than usual confidence. But usually I think these two types of treat-

ment stand apart from one another, and are incapable of any very close approximation. And it must I imagine generally be so. The two classes of treatment belong to different orders of mind, different habits, different educational schools. Moreover, each requires a kind of mental absorption—that renders their combination almost impossible without some sort of compromise by which the distinctive features of the contrasted types are to a great extent lost.

It is obvious, however, that no practitioner, however much he may attempt to treat diseases exclusively by specific medications, can confine himself to this. He must exercise such a supervision over the patient, his diet, his environments, the light, heat, or atmospheric conditions in which he is placed, as shall most materially affect him, whether he adds to this treatment the more restrictively medical or not. Indeed, it is quite conceivable that this negative treatment may be so important as to decide in itself the results of treatment. This is in fact what allopathy bombastically calls *expectant* treatment, in which the doctor abdicates his proper function and becomes merely head nurse. Still, whether he adds specific treatment or not, this amount of general hygienic direction he must undertake. The difference between the two classes of practitioners is this: the exclusive practitioner only gives general physiological directions in *self defence*, in order to provide a clear stage for the action of his remedies, and prevent the admission of anything that might antidote them or prevent them from producing their full effect. The eclectic practitioner, on the other hand, uses all these accessory measures as essential parts of his treatment. He regards them not so much as subordinate but rather as *additional* methods of acting on the disease. He seeks by them not only to put the patient in a proper attitude for receiving to the best possible advantage the action of a specific remedy, but he wishes also to act on the disease itself independently by these appliances. This is, then, the question at issue between the exclusive and the eclectic practitioner—is it admissible to pursue two plans of treatment co-ordinately? can physiologic treatment of the disease itself, and not of the patient only, co-operate with

specific? or must a selection in all cases be made between the two, and one pursued without mixture with the other? The so-called Hahnemannian practically asserts that the two cannot be combined. The eclectic homœopath thinks they can.

Now, I think we are bound to recognise the theoretic right of every homœopathic practitioner to bring all the resources of general and medical therapeutics into co-operation with homœopathy, to the best of his discretion and ability. He has a right to use all the powers which exist in nature to act remedially upon disease, and I know of no reason why he should shut himself out from the use of all the knowledge and appliances which allopathy claims as exclusively its own. There is no reason in the nature of things why homœopathic treatment should be incompatible with physiological if it is scientifically administered; and, whether the two can be combined or not, it is quite reasonable to suppose that the physiological method may rest on a solid and independent basis of its own, and may exist side by side with homœopathy as a co-ordinate but distinct system of medical practice, equally legitimate, and perhaps having peculiar advantages of its own which may occasionally place it on a level, or even raise it above its rival.

Whatever homœopathy proves, it disproves nothing which has been established by other methods of treatment. It puts other methods on their defence, and challenges them to show cause for continuing to exist; but it does not condemn them unheard. It may supersede any other treatment by doing in a superior way what it attempts to do but fails. But whatever merit they have it is bound to recognise, and the homœopathic practitioner will take care not to unlearn anything he has derived from other sources till he is quite sure he has found a better substitute in homœopathy. And it is obvious that the degree to which a practitioner is independent of his allopathic traditions is determined by the skill he possesses in the application of homœopathy. It follows from this that a beginner may employ accessory or physiological treatment in cases where a veteran who has made himself master of the *materia medica* would be able

to manage without them by keeping to strict homœopathy. This, which looks like a truism, is constantly forgotten, especially by the American champions for pure Hahnemannism, who denounce and scold with angry, almost vixenish, asperity all whose homœopathy does not reach the level which they claim for their own, and who, therefore, allow the deficiencies of their homœopathic skill to be supplemented by the resources of the opposite camp. Homœopathy is not yet, I think, sufficiently developed to take such an exclusive attitude. It must admit many degrees of purity among its adherents, and must leave a large latitude for those members of the profession who have a general conviction of the truth of the homœopathic law, but are not able to follow their Hahnemannian brethren into the exceedingly rarefied atmosphere of their transcendental therapeutics.

It must be a matter for the discretion of each practitioner to decide whether he can himself do most good by a purely homœopathic treatment of any given case, or by a physiological treatment of the symptoms and manifestations of disease, or by a mixture of these two. Doubtless simplicity of treatment has an antecedent probability of being scientific and successful, and the ideal of scientific simplicity is attained when the whole disease can be mirrored, and therefore antidoted, by the appropriate specific remedy. But disease is what it professes to be, *disease, i. e.*, an abnormal condition, a state in which the laws of life and the functions of vitality are disturbed and confused beyond all calculation. It seems to me impossible to take one fixed and unvarying law as the inflexible and unchanging rule for the management of such a shifting and fluctuating thing as disease, especially when it becomes desperate and irresistible, defeating and defying all attempts at cure, and admitting only palliation. We must accept its conditions as we find them, and meet them by all the expedients that science or even crude experience can suggest; we must, if needs be, be elastic in treatment, not slaves to one rule, not *doctrinaire* or pedantic in our determination that our patient must be relieved *secundum artem* or not at all. We must vary our aim according to the peculiarities of the case before us, and

with different aims employ different instruments. If we cannot cure we must palliate or relieve. And if we cannot bring the whole case under one conception, so neither can we be simple and uncomplicated in treatment. Just as no reasonable physician would hesitate to call in the aid of the surgeon when his art becomes necessary, so I submit that no reasonable physician will hesitate to seek for assistance in physiology or chemistry, or hygiene, or any of the subordinate branches of the healing art when the case under his hand requires the relief which these can best supply. Doubtless any one who hoists the colours of homœopathy will feel bound in all cases to treat his patient, if he can, according to the homœopathic law. He will recognise it as a duty to do his best always for the credit of homœopathy as a militant and misrepresented candidate for universal recognition. He will even place this motive side by side, though on a lower rank, with his first duty as a physician to do his patient all the good he can by all available means. He will not willingly accomplish by non-homœopathic means what he can do equally well by pure homœopathy. He will be jealous for the honour of the one medical reform which is destined to bring order into that wild, disorderly jungle of empiricism which is termed, by a scarcely excusable excess of forbearance and civility, the "*Science*" of Therapeutics. But having thus done justice to homœopathy he will not, for the sake of consistency to a medical creed, leave any good undone which he has the means of doing; and neither homœopathy nor anything else can impose such an unnatural obligation upon him.

Let it, then, be assumed that a homœopath is at liberty, if he thinks he can advantageously do so, to use all other means of relieving bodily suffering as well as homœopathy, and that it is for him to decide in each case as it arises in what proportion he shall use each method, and how far he can bring them into co-operation.

It is, however, very important that he should act frankly and honestly in doing this. If he thinks it his duty to treat separate symptoms by palliative means, or to choose what are called indications for treatment instead of prescribing for

the case as a whole, let him not suppose that he is treating his case homœopathically. It is possible to use even homœopathic medicines in a non-homœopathic way, and even if this is not done, yet if the disease itself is treated only according to physiological indications, if no attempt is made to cover the case by its proper medicinal equivalent, if strongly-acting medicines are smuggled in under the disguise of food or beverage, medical waters or medical baths; or if all sorts of modifications in the state of the patient are produced by heat, cold, moisture, galvanism, and other modes of surface treatment,—all this may be the best thing the doctor can do, according to his lights, but it is not homœopathy. I do not say, and I do not think, that the homœopathic ranks should be closed against such practitioners of Homœopathy-and-something-more. But they are most assuredly no fair representatives of homœopathic practice.

The question then arises—given the *right* of every medical man to exercise his art in the best way he can, is it possible to combine homœopathy with other methods of treating disease?

Now I think it must be allowed first of all that no other *drug* treatment is admissible while a patient is being treated homœopathically. Nothing can be tolerated which is capable of giving rise to morbid conditions or symptoms of its own. Otherwise, either inextricable confusion must arise, or the treatment must be a compromise, and no longer truly homœopathic at all. In order to prescribe homœopathically the physician must know all the symptoms of the disease, which is only saying that the facts of the case must be fairly brought under his observation. And the more purely these symptoms are manifestations of disease, and of nothing else, the more exact and complete are the data he has to act upon. If, however, some of the symptoms are due to the presence of other medicines, the case is confused and its features are presented in distorted forms before him. He must if possible separate what is artificial, *i. e.*, symptoms of a disease manufactured by medicine, from what is natural, *i. e.* the proper evolution of the disorder from which the patient is suffering. We all know, from every-day experience, how

difficult it is to treat a case that has been sophisticated by a long course of drugging ; and if it is difficult to treat an organism saturated by drugs administered long ago, it is even more difficult to treat a case in which the transient and superficial effects of medicines recently given present themselves side by side, in inextricable mosaic with the essential symptoms of the disease. Even if we know what these drugs are, and so are able, by a reference to their pathogeneses, if they have been proved, to separate between the symptoms due to them from those arising from other causes, yet the confusion is only diminished, it is not removed, because it is impossible to say when the proving of any medicine is completed. Hence it is quite possible that the patient may be exhibiting some new effects of a drug—effects not yet registered in the published provings. If science means exact knowledge of facts and of the laws of their existence, it is certainly unscientific to adopt a course which brings doubt, confusion, and uncertainty into the facts, and renders it necessary to grope in the dark.

Moreover, as one of the conditions of scientific accuracy and simplicity, one of the fundamental laws of homœopathy is, that only one medicine should be given at a time, and that that medicine should cover as nearly as possible all the symptoms of the case. Now, if one medicine is given for the case as viewed generally, and another left for pain, another to open the bowels, another to produce sleep, another to relieve indigestion, another to quiet the nerves ; or if two or more medicines are given in alternation to meet the most clamorous symptoms, who does not see that the whole treatment becomes haphazard, and can only be explained by the assumption that the doctor's perceptions themselves are in an utterly bewildered condition ? Of course, the case supposed is an extreme one, but the principle is the same when only two medicines are given ; and, indeed, when any medicine is given, not for the whole state of the patient, but for some prominent indication or symptom in his case.

But if homœopathy cannot admit accessory drug treatment, there are yet many other modes left in which treatment may be helped without in any important degree

entrenching upon the province of specific treatment. By regulating the diet the various secretions may be acted upon in such a way as not only to produce no confusion in the symptoms of the case, but even to bring out the essentials of the disease in stronger relief. So long as no artificial or drug symptoms are produced by what is taken into the stomach, the variations in the symptoms produced by changes in diet, so far from being a source of confusion, are highly suggestive and valuable. These symptoms, which the attendant can evoke by his own management, and which arise from the combined action of the disease and the general treatment of the patient, are part of the physician's capital, and throw important light upon the nature and treatment of the case. They are not pure pathogenetic effects, but symptoms of the disease as modified by conditions supplied from without. This, of course, applies not only to diet, but to all other modes of affecting the patient which do not produce any derangement of their own, but only such changes as are within the range of variation consistent with health. Such changes as these, then, may be sought for, not only for their own sake, *i. e.* because they may add to the comfort of the patient, and put him in a better condition to respond to proper medical treatment, but for reasons peculiar to homœopathy itself, that the case may be studied under the new lights and shades produced by these causes.

It is no part of my design to go into details on this and similar matters, though it will be quite consistent with the general scope of the question under consideration, if such details are suggested in subsequent discussion. It may suffice on this point to observe that, by judicious management of diet, almost any region and function of the body may be influenced, and thereby treatment may be most importantly aided. Another valuable accessory is water, hot or cold, by baths of different kinds, washes, packing, fomentations, enemata, injections, &c. It is quite possible, however, to use these appliances in such a way as to produce effects resembling drug effects or artificial disease. For instance, fever may be kept very much under control by the

wet sheet pack, but if too long continued it may produce a feverish condition of its own, or lead to internal congestion, especially in the brain or lungs. Also the too frequent use of injections or enemata may act injuriously by substituting the water of an outward application for the natural lubricating secretions of the mucous surface affected, and thus both disguising its real condition and interfering mischievously with its functions. Poultices and fomentations may be regarded as naturally branching out from hydro-pathic practice. Much relief may be often given by these in pain and inflammation, especially if they are largely and liberally applied. It may, however, be questioned whether a passing relief is not sometimes gained at the expense of a permanent cure. For instance, in the treatment of boils, when they come in crops, in repeated succession, I believe I have seen the tendency to return fostered and aggravated by external treatment, and better results, and more lasting, obtained by leaving them to develop themselves and discharge their contents according to their own law of evolution, without interference by hot applications or poultices.

Indeed, returning to the more general question, it seems to me a very difficult thing to determine how far permanent cure may be either prevented or rendered incomplete by too great eagerness to snatch at a palliative relief of the most urgent symptoms. If patients are led to expect that every symptom which arises is to receive instant and special attention, and that all the resources of medical art are to be employed to give them the particular kind of relief which they have set their minds upon, they are likely to lead their medical adviser to a loose and superficial style of practice, and in the end great results are sacrificed for the sake of small ones. The art of managing patients so that they may follow rather than lead, may co-operate instead of thwart, is one of the problems of medical practice which can be scarcely solved without a good deal of tact and dexterity, as well as personal character and weight in the physician himself. It is in modifying the most distressing symptoms that the accessories of treatment may be most fitly, though also most carefully and cautiously applied, and if

the medical attendant possesses the confidence of his patient, he will generally so guide him that his expectations will be reasonable, if the patient is capable of reasoning about his own case ; or he will exercise the same healthy sway over those who are responsible for the patient, if he cannot manage himself. It is hard, indeed, to adjust fairly between what amount of endurance you may expect from the patient, and what kind and degree of relief he has a right to claim of you. If the moral duty of patient endurance has its special application to the sufferer, that is a law which the physician must take as little advantage of as possible for himself ; he cannot tolerate as a physician much that his patient is morally bound to endure patiently as a man, and he cannot expect from his patient the same kind of calculation of consequences, the same calm and passionless gaze into the future issues of the case, that is perfectly natural to himself. How are all these complications to be steered through?—obviously only by intense self-reliance in the medical attendant, who must see clearly what he is to aim at, what he can accomplish, and what he cannot, and the order in which his plans are to be carried out. And whatever good accessory treatment may do, or fail of doing, one very cogent reason for setting a high value upon it is the moral effect it is capable in most cases of producing on the mind of the patient, and of those about him. By our use of tasteless doses of medicine which produce no violent effects, we sacrifice the one great advantage attaching to active treatment, that there is no mistake about the reality of the effects which treatment produces. Something is being done, even if the first effect is to add to the patient's suffering. Those of us who have felt the benefit in our own persons of homœopathic treatment, know that the relief often comes as a sort of surprise. The disorder has vanished before we are aware of it, and it is not till the patient asks himself what has become of the pains or distresses that he was accustomed to that he realises the fact that anything has been done at all. Often the symptoms vanish with so little fuss—melting imperceptibly out of view—that it is difficult for an unreflective patient to give

to the medicine the credit to which it is entitled. He thinks the disease has "gone of its own accord," or that nature has righted itself, or the disease expended itself, and gone out like a candle that has burnt out. Especially if an allopathic luminary is at hand to bring the light of his profound medical philosophy to bear on the case, the suggestion that nature did it all wears an air of great plausibility. Of course we know that this only proves how friendly homœopathy and nature are, and what a close alliance there is between them ; but this explanation is not obvious to a mind warped by the unnatural violence of allopathic medication. Now, it is unquestionably an assistance to a homœopathic practitioner if he can by his accessory treatment either create or confirm the impression that something is really being done, and that help is being brought through other avenues than the absorbent glands and ducts of a stomach that is, perhaps, already in an extremely perverted state itself. Were there no other reason than this for accessory treatment, this would be sufficient, so important is it that the imagination and good will of the patient should co-operate with the measures adopted for his relief.

The conclusions which seem indicated by all these considerations are as follows:—That it is on many accounts exceedingly desirable to give colour and shading to the bare outline of homœopathic treatment by as much accessory treatment as can be harmlessly combined with it ; that what this is must be determined in each case by the discernment and skill of the practitioner ; that there can be no good reason for refusing the aid of any accessory measure which restricts its action to the production of such changes as in a healthy person would fall within the range of variation in kind and degree compatible with health ; that any accessory treatment beyond this is to be regarded as a departure from true homœopathic practice ; that such a departure, if frankly made, is a perfectly fair and legitimate style of practice for any one, whether a homœopath or not ; but that a homœopath should only sanction such deviations from true and pure homœopathy as are not only innocent in themselves, and approved by good and trustworthy expe-

rience, but are likely to lead to results superior to any he is capable of producing by his own practice of homœopathy. In a public hospital doubtless such deviations ought in all cases only to be resorted to after consultation with one or more additional members of the medical staff, so that the homœopathic standard may not be lowered unnecessarily, and on the undivided responsibility of one man.

If these are said to be lame and impotent conclusions which leave the matter practically where it was before, I can only, in all candour, aver that I could never dream of offering a complete solution of so vexed a question; that the maxims I have laid down, and the reasons on which they are based, do at least give an intelligible ground to stand on; that I have endeavoured to avoid the extremes of fanatic exclusiveness on the one hand, and of such laxity on the other hand, as leaves homœopathy nowhere, and throws back our reformed practice upon the trackless waste of empiricism. I will frankly confess that my own ideal of practice is one in which accessory treatment shall be reduced to a minimum, in which the greatest possible pains shall be expended on the selection of the remedy, and in the administration of it, and in which all other treatment shall be put in subordination to this. If my own practice often falls short of this standard, I attribute the fault, not to homœopathy, but either to myself or to the imperfect development of our noble art. But, in truth, one need not on this occasion indulge either in professions or confessions. Every one who places a definite goal before his mind, and constantly aims at it, is sure to meet with successes as well as failures, and will probably leave the highway of sound practice less embarrassed for his successors.

Discussion on Mr. Theobald's paper.

Dr. RANSFORD is much pleased with Mr. Theobald's paper. The discussion of all its remarks will scarcely do more (from want of time) than elicit individual opinions. Dr. Ransford advocates a careful study of the totality of the symptoms of each case, and endeavours to find the corresponding remedy. If the

case can be cured on physiological principles without drug medication of any sort, so much the more merit will be due to the physician. He does not object to such accessories as water, cold or hot poultices, or fomentations, as homœopathy is simply the law for drug-administering with which such auxiliaries as the above need not interfere; but in all *curable* cases he strongly objects to such means as opiates or purgatives. He has himself obtained the most success the more closely he has adhered to the homœopathic law, and he has noticed the comparative successes and failures in those of his colleagues who adhere closely to or deviate from it. About diet he is very careful, prohibiting whatever may be indigestible or having medicinal properties which might interfere with the operation of the remedies chosen.

Dr. HALE, in thanking Mr. Theobald for his paper, said that the description of the two classes into which Mr. Theobald had divided practitioners was, on the whole, fairly given. He contended that, however much we tried to be purists—and it is our duty to try to be so—nevertheless exceptional cases now and then occur where we are obliged to supplement the purely drug treatment by using accessory measures; *e. g.* a young officer, now under Dr. Hale's care, contracts gonorrhœa, during the acute stage of which he is exposed, sitting in an open boat at sea in wet clothes, to a severe chill, which is followed by acute pleurisy. The military surgeon appears to have only treated the gonorrhœa, and when the case came into Dr. Hale's hands, although the patient was saturated with *Copaiba*, which had produced its characteristic eruption on the skin, yet the *gonorrhœa* was uncured; and he found extensive pleuritic effusion on the right side. Now, as this patient was expecting every week to be ordered off to India, and it being obviously of great importance that he should be cured as quickly as possible of both diseases, and supposing that no medicine could be found which would cover the *totality* of the symptoms, the question arises whether, while treating the pleurisy by purely homœopathic measures, local treatment for the gonorrhœal discharge was not only justifiable, but absolutely imperative? Dr. Hale contended that, although his desire as well as his duty was to practise pure and scientific homœopathy, yet he was also a physician, and therefore at perfect liberty to treat forms of disease which were not amenable to drug medication by every means which he considered best for his patient. While, however, claiming this liberty of action, he believed there had prevailed of late years great laxity of practice amongst so-called homœopaths—a practice homœopathic only in name. One glaring instance had come under his observation where, in a case of pulmonary disease, three medicines in the lowest dilutions possible were administered in the course of twenty-four hours: *Phosphorus* for the cough, *Nitric acid* for the perspirations, and *Morphia* to procure sleep. Dr. Hale felt more than ever convinced that by patient study of our cases, by which alone we can

arrive at the true remedy, our success will be increased in the treatment of disease. Neglect of that painstaking method went a great way to account for the homœopathic cures of late years being less brilliant than they were some years ago when a pure homœopathy was practised. He trusted, however, that a more scientific practice was now to be hoped for, and he believed the paper Mr. Theobald had just read would be found a valuable contribution to that much to be desired end.

Dr. WILLIAMS fully agrees with Mr. Theobald in all main points, not excepting his remarks on palliative treatment. He thinks such treatment should only be adopted in exceptional cases, and after homœopathic treatment had been fully tried. But in any case in which cure was impossible, he should feel it his duty to relieve as much as possible. To give up such a cure because homœopathy could not cure it, when it might be relieved by palliative treatment, would be to be a homœopathist and not a physician. We have as much right to palliate the symptoms of an incurable case, or to give a night's rest to the excited and exhausted nervous system by a dose of *Opium* as any other physician, only, of course, it should not be called homœopathy. For himself he has never used opiates in his practice, but would do so if he saw a case which required them. He mentioned the case of a lady known to him who was suffering from cancer of the upper anterior part of the thigh; the tumour was very large, and the pain she suffered was excessive, so that she had scarcely any rest night or day, and was being literally worn out by extreme pain and want of sleep. Her cure was out of the question, and all that could be done was to seek to relieve her sufferings and procure sleep; and in such a case, if homœopathic medicines failed to relieve, he would certainly feel it his duty to give *Opium*.

Dr. BAYES fully agreed with Mr. Theobald that two drugs of antagonistic action should not be administered at the same time to a patient. With regard to the question of accessory treatment, he agreed with Dr. Williams that we ought to take broad views of our position as physicians. It is undoubtedly true that accessories invalidate the value of statistics; that it is difficult to say how far the drug given has cured the case when powerful auxiliaries are used. For example, in a case of swelled testicle, who would trust to internal drug-giving alone? We enjoin rest and lotions, or cold and hot water applications, as well as medicine, and it is difficult always to say how much each of these remedies have contributed to the cure. But we are not to look on the treatment of a case as if it were a simple homœopathic experiment. We must remember we are physicians bound to use every means that may conduce to cure, even though it should invalidate to some extent our knowledge as to the true curative power of the drug used. In many cases we are bound to use accessories, or to leave our patient longer unrelieved than

he would be if we used them. As to the use of more than one homœopathic drug, he could not see why we should limit ourselves to one. Few diseases can be cured with one drug, and to what time are we to be limited to one drug before we administer another? Is it to be twelve or twenty-four hours, or longer or shorter? In few acute cases do we find that any one drug exactly covers the whole symptoms of a case, and if we find that we can by two drugs cover the whole symptoms, we are bound to use the two drugs in alternation or in succession. At the recent congress at Birmingham the use of two or more drugs was considered by Dr. Black and others to be needful in the treatment of acute rheumatism—*Aconite* and *Bryonia*, for example. When we are in front of a disease we must always ask ourselves what we have to treat. Are we to treat the disease-cause, or the symptoms resulting, or both? If we are to treat both, we are sometimes driven to prescribe two medicines in alternation, or in rapid succession, to meet both. In some diseases, too, many organs are consentaneously affected. In order to restore the balance of function to these, if there is any truth in Dr. Sharp's organopathy, we ought to use more than one remedy. If the liver and the kidneys are both affected, we ought to prescribe a medicine for both these organs. In short, we are bound to do all that we can in each case to restore our patient, and the use of accessories and non-medicinal appliances is a paramount duty. Hot and cold water, external stimulants, *Rhus* or *Arnica*, or other external remedies, each and all, may greatly aid us in inducing curative results, together with the administration of the appropriate internal drug.

Dr. HAUGHTON thought that homœopathy had undergone a decided change in character during the last five years; but he observed that several speakers had spoken of being "justified" in doing this or that for the good of their patients, as if their first duty was not towards them. He himself made no apology for the use of non-homœopathic accessories, especially as it was self-evident that there were natural limits to the application of the homœopathic law of specific medication. With regard to anodynes, he felt almost inclined to agree with the old adage, "*Sine opio nolo esse medicus.*" He alluded to the effects of liberal sentiments at the late meeting of the Dialectical Society upon some allopaths present there, and upon himself when inquiring into homœopathy. He mentioned Dr. Barter's death, which he had just heard of, in connection with another great accessory which he had done so much to introduce, viz. the Turkish bath, and also declared that his confidence in hydropathic appliances was not diminished by his knowledge of homœopathy, as he found them of especial value in acute and inflammatory affections.

Dr. LEADAM observed that we certainly ought to treat our cases with as little recourse to accessories as possible. Adjuvants should not be carelessly resorted to, but the true homœopathic

remedy should be diligently sought for, as in most cases it is the want of sufficient knowledge of the *materia medica pura*, or the want of sufficient industry in seeking the true remedy, that leads the practitioner to the use of homœopathic expedients; for it is found more easy to fly to them than inquire at the fountain-head of all homœopathic truth, namely, the *materia medica pura* of Hahnemann. It would be an interesting question whether low dilutionists or high were most given to fall back upon allopathic drugs. In his opinion it is the former.

Dr. DUDGEON said that discussions of this kind made him regret that the word homœopathy had been coined, that we were called homœopaths, and made him wish for the time when we should be able to repudiate the name, and call ourselves physicians only. It was sad to hear gentlemen apologising for doing the best for their patients, and fearing they might be held to be practising under false pretences when they prescribed anything for their patients except drugs on the principle of similars. There was no such thing as a pure homœopathist. We all prescribed things for our patients that could not be referred to the homœopathic law, and some of our best cures were made without prescribing a dose of medicine of any kind. Dr. Hale was, as gentlemen knew, a great stickler for pure homœopathic treatment, but the case he had related showed the advantage to the patient in as far as shortening the duration of his disease was concerned, by a departure from his ordinary pure homœopathic treatment. He looked forward to the time when, having dropped our sectarian appellation, we should be able to regard with an unprejudiced mind all methods of treatment as equally legitimate, and those to receive the preference which enabled us to do most good to our patients. Feeling ourselves not bound to support any method through thick and thin, we should then be enabled to assign to each its proper value.

Dr. YELDHAM, echoing sentiments already expressed by Dr. Dudgeon and others, said, first and foremost, they were *physicians* commissioned to cure their patients by all legitimate means; secondly, they were homœopaths, bound to cure by homœopathy alone where they could. Auxiliaries were of various kinds, some were positive, others were negative. The former had been largely quoted. He would instance some negative ones: for example, the cure for brain-fag was rest—medicine without this was of little use. He lately was consulted by a young man, strong and hearty in other respects, who had been annoyed for years with sore throat simply. He had received much fruitless medical advice. He (Dr. Yeldham) learnt that he was an inveterate smoker. Ordering him to discontinue this, he got quite well in a month without medicine. He had cured many cases of chronic boils on the back, &c., by a course of warm soap-and-water baths. These cases often arose from uncleansed skin, in which medicines alone were of little avail. He had seen cases of severe erythematous

rash of the skin caused by wearing flannel. These got well when the dress was changed. Many cases of headache were caused by breathing impure air, especially by living in small rooms lighted by gas; by substituting oil lamps or candles the headaches got well. Some cures of indigestion owed more to the aid of the dentist, to thorough mastication of the food, and to dietetic advice, than to medicines. Much might be done by common sense alone, and yet leave plenty for medicine to do.

Mr. ENGALL said that the subject of the discussion had been so exhausted by the previous speakers that there was little left for him to say, except to advert to some remarks made by a previous speaker regarding the brilliant results obtained by Hahnemann. Of these cases very few were narrated, and he (Mr. Engall) was of opinion that the success which was so much spoken of as attending the earliest practitioners was due to the diseases they had to treat, being many of them drug diseases from the doses given by the allopathists previously. He thought that any auxiliary treatment should be of such a nature as not to act against the curative action of the homœopathic medicines employed.

Dr. MADDEN had been much interested, both in the paper and in the discussion which had followed, and he was glad to find several of the members objecting strongly to the use of *medicinal* accessories. Mr. Theobald had made a very just and important distinction between accessories which were in themselves medicinal and those which did not involve drug-giving. The latter he considered that we were free to use, while he believed that the former (drug accessories) would seldom, if ever, be required by one who really took pains to carry out a strict homœopathic practice. He would certainly unhesitatingly use such if he could not do without them; but the longer he practised homœopathically, the stronger became his conviction that, as far as drug-giving was concerned, it very rarely, indeed, required any auxiliary. Mr. Theobald also drew attention to the circumstance that some physicians, especially homœopaths, made drug-giving their chief means of treatment, while the advanced members of the dominant school well-nigh neglected drugs altogether. He (Dr. Madden) agreed with Mr. Theobald in believing that the best treatment consisted in giving homœopathically selected drugs, in conjunction with the general, non-medicinal methods followed by those who called themselves "rational" practitioners.

Dr. DRURY agreed generally with the remarks that had been made. While admitting that perfect freedom should be allowed as to doing what was best for a patient, he could not but feel that the danger of the present day arose from the careless practice that was too often adopted, and the readiness with which homœopathic remedies were laid aside and allopathic ones substituted, or remedies given nominally homœopathic, but really for the sake of their allopathic action. He agreed with Dr. Madden that, when judicious homœopathic treatment was used, the necessity for

auxiliaries was reduced to a minimum. Cases, however, arose at times when a departure from a strict rule was necessary for the patient's welfare; such cases were very rare, but when one did occur a love for pure homœopathy ought not to be allowed to act to the prejudice of the patient. He had lately attended an old gentleman, about eighty-seven years of age, in a severe illness, who during its progress had submitted without murmuring to what was being done, but as he got stronger and better, began to get fidgety about the state of his bowels, and was very anxious to be allowed to resort to a mixture of senna that had been his constant companion for many years. The craving for this grew so strong that he was becoming ill again from its being withheld, though it was by no means needed. Believing that it would not do him any harm, but that he would suffer very much from mental irritation by refusing it, he (Dr. Drury) told his patient that, while advising him not to take it, he left him free to do as he thought proper if he fancied he required it. The old gentleman immediately directed the bottle to be brought to show what it was like. On receiving it he looked lovingly at it, smelt it, and immediately put it to his lips and took a good sip of it, and evidently enjoyed it as much as if it were some good old wine. Dr. Drury was recently asked by a lady to purchase something that might be of use to the wounded soldiers in one of the German hospitals on the Rhine. Considering the circumstances of the case he thought chlorodyne would prove a valuable remedy in procuring a few poor fellow's freedom from pain and a good night's rest. Uninfluenced by those prejudices that he thinks would have operated on his mind a few years ago he got the chlorodyne, and was thankful to hear that it was highly prized. Homœopathic remedies if sent would not have been valued, whereas any one could administer the chlorodyne. He merely mentioned the circumstance to show the change that he thought had happily come over his own mind, and to illustrate the point that at times, by laying aside prejudices and exercising a legitimate freedom which differed from carelessness, much good might be done and no principle be compromised.

Mr. THEOBALD in reply said that Dr. Madden had explained with his accustomed lucidity the scope of his paper. While he thought that no drug-treatment other than homœopathic should be ever combined with homœopathy, he claimed for homœopaths the right to use drugs, if they chose, in any way best suited to the purpose they might have in view, but he thought occasions for the physiological or antipathic use of drugs ought very rarely to arise. Even the agonies of cancer can often be mitigated without opiates by using the appropriate medicine for the pain, even when cure is out of the question. He thought, however, it was a perversion of language to speak of curing when there is nothing done except to alter the bad habits (smoking, for instance, or want of cleanliness) of the patient. Curing is distinctly a work of art, not

merely a return to nature. And it should be remembered that if such a disease as boils may arise from want of cleanliness, the homœopath is bound to regard the dirt, &c., as only one factor in the causation, and to find other factors in the constitutional susceptibilities of the patient, and these are matters for homœopathic, *i. e.* art treatment. Mr. Theobald begged to express his strong resistance to the polypharmacy of those gentlemen who use two or more drugs in alternation. No disease is ever covered in its entirety by a medicine, but it is impossible to say how far a well-selected remedy will go in really covering symptoms not included in the proving, but which are related to those which are so included. It is conceivable that such symptoms would have been produced if the proving had been pushed further, or if the patient had been among the provers. This is one of the many suggestions of the key-note principle. You have only to touch the right fibre in the organism and the magic telegraphy of the homœopathic remedy finds its way to all the parts that are *en rapport* with the clue so acted upon. And even when two independent disorders coexist, as in the typical case of mixed gonorrhœa and pleurisy mentioned by Dr. Hale, we may be quite sure the disorders are not so separated in the organism as they are in our pathological notions—they reciprocally modify one another; and if we treat the whole case as it stands, by some of the marvellously comprehensive remedies which homœopathy affords, we shall find that the chasms which look so wide and deep are bridged over by the medicine which the homœopathic law points out, and that we can treat both at the same time. Every practitioner must remember cases in which patients at their second or third visits have mentioned symptoms which they had forgotten to name before, which, however, have been removed by treatment even although they were not included in the data supplied. So far-reaching and comprehensive are the medicines which homœopathy supplies. These considerations, though not belonging to the subject of the paper, naturally connect themselves with it. Referring, however, again to accessory treatment he would suggest that physiological measures for relief of pain and suffering in true surgical cases, such as the cases of those wounded in war, were on a different footing from medical cases. In these cases there is no disease to combat, but only injury, and the effects are often such that they must be met by physiological management rather than medical treatment; though even in these cases the resources of homœopathy are very great. It had been remarked that gentlemen defending the use of accessories adopted rather too apologetic a tone, and “justified” themselves so much as to give opportunity for the application of the proverb, *Qui s’excuse s’accuse*. In justice to ourselves, however, it should be remarked that our freedom and latitude in using variety of treatment is much greater than that existing in allopathy. We are committed to a principle of innovation, and are prepared to apply it to other cases. If an

allopath, on the other hand, uses hydropathic or homœopathic means, he screens himself as much as possible lest he should be gibbeted in the journals; he disguises his innovation because he knows he is in danger of being denounced as a heretic. No such narrowness exists in our ranks. Every method is open to us, and our limitations are those demanded by nature and her laws (and therefore are no real limitations at all), not by the arbitrary determinations of a despotic *collegium* or a rabid professional mob.

ON THE ACTION OF *BELLADONNA* ON THE URINARY ORGANS.

By DR. RICHARD HUGHES.

(Read before the British Homœopathic Society.)

I LAY upon the table an arrangement, in schema-form, of the symptoms caused by *Belladonna* in the urinary sphere. It includes those observed and collected by Hahnemann; those of some provings with *Atropine* conducted by Dr. E. M. Hale; the results of Dr. Harley's experiments with the same alkaloid; the effects recorded in a number of cases of poisoning collected by myself; and some miscellaneous observations. These symptoms form the text upon which the following remarks are a commentary.

I.—1. In the first section I have placed the changes in the *quantity* of the urinary secretion induced by *Belladonna*. As so often happens in our pathogeneses, they include two precisely opposite conditions, in this case, of *plus* and *minus*. Sometimes the flow of urine is much increased, justifying the application to the drug of the term "diuretic." But quite as often, for the first twelve or twenty-four hours after the ingestion of the dose very little urine is passed. It is not that it is secreted, but retained in the bladder; for no great evacuation occurs subsequently, and in one case the introduction of a catheter brought away but a scanty quantity. There are no observations on record of

the character of the urine when scanty ; when copious it is generally described as pale and watery.

In seeking for the rationale of these phenomena, we come at once upon the fact that *Belladonna* is eliminated with the urine. Dr. Harley states as the result of his experiments that "the kidneys are active in its elimination from the minute that it enters the circulation until it is entirely removed from the body." The analogy of its effects elsewhere leaves little doubt that in passing through the kidneys it excites the circulation in these organs. Such excitement, if moderate, would result in diuresis ; but if excessive or prolonged, congestion and stasis would occur here as they do in the throat, and the urine would become scanty or even temporarily suppressed.

I have little question of this being the interpretation of the varying influence of *Belladonna* upon the quantity of urine secreted. It appears accordingly that it is the primary or Malpighian circulation of the kidneys which is affected by the drug ; so that the aqueous portion of the urine is increased or diminished by its use. There is no reason to suppose, from the present symptoms or any which will subsequently come before us, that it has any direct action upon the secreting cells of the convoluted tubes. [It will be remembered that the renal arterioles break up first into the Malpighian tufts of capillaries, each of which is surrounded by the dilated extremity of an uriniferous duct. Here they give off the water of the urine, and then, reuniting into one or more efferent vessels,* pass out of the tuft to break up once more into a leash of capillaries surrounding the walls of the ducts, that from the now concentrated blood the cells which line them may form or separate the solid constituents of the secretion.]

The therapeutic indications hence resulting seem to be the following :

a. Belladonna is likely to be useful in the so-called "diabetes insipidus," in an early stage ; for the post-mortem examination of fatal cases generally reveals structural changes in the kidneys which may well have had simple hyperæmia for their commencement.

b. Acute renal congestion, with its diminished or suppressed urine, is no less within the sphere of the drug; but it would probably fail to reach the severer cases of this affection, which require the similar but more intense influence of *Terebinthina*.

c. This brings us to the question of its relation to the renal effects of the scarlatinal poison, of which, in most things, it is so striking an analogue. If these were limited to hæmaturia, and even albuminuria, they would indicate only a congestion of the Malpighian tufts similar in kind to that caused by the drug, if higher in degree. But they so often include also tubal nephritis, with its accompanying desquamation, that *Belladonna* cannot be expected to neutralise the mischief in its whole extent. Nevertheless, as with *Terebinthina* itself, the benefit resulting from unloading the Malpighian capillaries, and so setting free a copious flow of urine to flush the tubes, is so great, that I can well believe the favourable results which Dr. Harley, in common with some of our own body, reports from its use. His statements as to its effects on the quantity of albumen poured out in these cases evidence its homœopathicity to albuminuria from renal congestion, though it has not hitherto produced that symptom in the healthy subject. "If the dose be administered," he writes, "when the exudation of albumen is at its minimum, the first effect will usually be a slight increase, unless the dose be a very small one; and if the dose be excessive the albumen will be increased to the maximum amount. If the dose be not excessive the albumen begins to decrease as the symptoms of the *Belladonna* action decline, and after a few hours the beneficial influence of the drug becomes apparent. This is strictly in accordance with what has been observed of the effects of large and small doses on the heart and larger blood-vessels, and in using *Belladonna* in renal disease, we must be careful to avoid large doses."—*Old Vegetable Neurotics*, p. 253.

I.—2. My next section contains the morbid *appearances* of the urine. On these I have no comment to make at present. Some of them cannot be interpreted, from our want of knowledge of their connections; and others belong to the influence of the drug upon the bladder.

I.—3. The *physical and chemical characters* of the urine, on the other hand, present features of great interest. For our knowledge of these we are largely indebted to Dr. John Harley. He confirms the previous observations of Böcker and Parkes that *Atropine* increases the excretion of the urinary solids. He finds this increase most marked in the case of the phosphates and sulphates, which are sometimes doubled; next in the urea, least in the uric acid, while the chlorine is generally diminished. The phosphates are so abundant that their characteristic opalescence, cleared by nitric acid, appears when the urine is heated. Its reaction, moreover, is usually alkaline, and triple phosphate often deposits in the course of two or three hours in considerable quantity.

These phenomena point to processes going on behind the renal organs, but of which the composition of the urine is an index. They speak of an increased disintegration of tissue, and, if the opinion be sound that an excess of phosphates means special waste of nervous substance, it points thereto in the present instance. In the general effects of the drug we have the actions which explain this increase of the urinary solids; for a febrile condition is set up, and the nervous centres especially show signs of undue excitement. Hence excessive disintegration and waste of tissue, which shows itself in the urine. "If the action of the medicine," writes Dr. Harley, "take place during a period of fasting, and the maximum acceleration of the pulse be great, and sustained for an hour, the urine will resemble that voided after the digestion of a hearty meal, in the richness of its solid constituents." The difference is that the richness is at the expense of the tissues instead of the food. Nor is the diminution in the chlorine excreted less significant; for this is a characteristic symptom of pneumonia and other local inflammations, and we know how many of these are liable to be set up by *Belladonna*.

The results now obtained confirm the perfect homœopathicity of the drug to those febrile and inflammatory conditions for which we so continually use it. Again to cite Dr. Harley: "An infinitesimal quantity of *Atropia*—a

mere atom—as soon as it enters the blood, originates an action which is closely allied to, if it be not identical with, that which accompanies meningitis, enteric or typhus fevers.” And as he himself finds it so very beneficial in these same affections, he has to suggest that “two similar effects, the one arising from a local irritation, and the other from the presence of *Belladonna*, like spreading circles on a smooth sheet of water, interfere with and neutralise each other.” We have no homœopathy more positive than this.

II. From the symptoms of the urine we pass to those of micturition ; and here *Belladonna* displays a potent influence upon the bladder. Symptom 34 is taken from a patient of Dr. Jenner’s, who had placed a *Belladonna* plaster on a somewhat abraded surface of his body. Two hours after the first appearance of the symptoms “he was affected with an extreme desire to micturate, though he could pass only a few drops of perfectly colourless urine. From this time, till he lost consciousness, his desire to pass urine was constant ; whenever he could retire, he did so, but succeeded in expelling from the bladder, with considerable effort, only a few drops of colourless fluid.” Symptoms 38 to 39 are to the same effect. I cannot agree with Dr. Harley that this frequent and urgent micturition is “the result of repeated calls to empty a distended and weakened bladder.” It seems to me a true strangury, very different from the dribbling which takes place from a distended and weakened bladder, such as we have in paraplegia. The fact that the introduction of a catheter is unimpeded may show that no spasm is present, and the absence of pain disproves the hypothesis of inflammation. But I think that *irritation* to no slight extent is set up. Perhaps the “whitish urine” and “white, thick sediment” observed by Hahnemann (S. 25, 26) depended upon the presence of bladder epithelium, as so often seen in idiopathic irritability of the organ. Böcker, indeed, states that the vesical mucus in the urine is increased by *Belladonna*.

I cannot think, therefore, that retention is the one effect of *Belladonna* upon the bladder, from which all its disturbances of micturition result. Indeed, apparent “retention”

may be really scanty secretion, as in Symptom 3, where the catheter found but little urine in the bladder. But I do not deny that a true paresis of the detrusor urinæ (as in S. 41, 43) may occur, giving rise to genuine retention. The loss of power may extend to the sphincter, leading to enuresis (as in S. 44—48); incontinence of urine and involuntary faecal discharges being, according to Lusanna's observations, the effect of *Atropine* in its fullest degree of action. This is not the only part of the body where irritability and paralysis are the alternatives of *Belladonna* influence.—I cannot say whether the irritation of the bladder is secondary to the alterations caused by the drug in the urine, and the paresis to its action on the spinal cord; or whether both are primary symptoms.

Once again we turn to therapeutic inferences.

a. For simple "irritation" of the bladder, short of actual inflammation, I know of no medicine so valuable as *Belladonna*. The pathology of the complaint is not always obvious, but phenomenally it is well recognised. The presence of bladder epithelium in the urine rather indicates the medicine than otherwise; nor need obvious acridity of the secretion forbid it, whatever be our accessory measures.

b. The place of *Belladonna* in paralytic affections of the viscus is less certain. It is very useful in post-partum retention of urine, but has yet to be tried in that which accompanies paraplegia. It is occasionally of striking benefit in the nocturnal enuresis of children, but quite as often utterly fails; nor have I yet learnt what is the form of the affection to which it is specifically suited.

A word in conclusion upon some of the medicines analogous to *Belladonna* in their action upon the urinary organs.

Of these *Terebinthina* possesses the closest similarity. In the kidneys it acts like *Belladonna* upon the Malpighian circulation, and in the bladder causes a corresponding strangury. But its effects are more severe, intense, and inflammatory, and hence therapeutically it reaches a higher grade of disease.

Another close urinary analogue of *Belladonna* is *Ferrum*,

especially in its action on the bladder. But the urgent micturition to which *Belladonna* corresponds is not diurnal, as Dr. Cooper has well shown that of *Ferrum* to be, but is rather nocturnal if anything.

Eupatorium purpureum causes diuresis and vesical irritation, but the former has less hyperæmia with it, and the latter more, than that of *Belladonna*. It causes acute vesical catarrh, which *Belladonna* does not.

Cantharis is still less similar. In the kidneys it acts directly upon the secreting cells, and its action on the bladder is purely inflammatory.—None of these drugs have the paralysing action of *Belladonna*.

I hope that this brief commentary on the urinary action of *Belladonna* may elicit the views and experiences of many of my colleagues.

Discussion on Dr. R. Hughes's paper.

Dr. HOUGHTON wished to know what Dr. R. Hughes thinks of the action of *Belladonna* in Bright's disease of the kidneys.

Dr. YELDHAM thanked Dr. Hughes for his very lucid and succinct exposition of the effects of *Belladonna* on the urinary organs. In speaking of the effects of *Belladonna* in increasing the quantity of urine, Dr. Hughes was, of course, aware that that was a very common effect of medicines. He (Dr. Yeldham) had observed this on his own person, when he had occasion to take medicines of different kinds. This effect was attributable to the fact that the kidneys were simply excretory organs through whose agency nature endeavoured to throw off foreign matters. He then alluded to two cases of enuresis nocturna of sixteen years' standing, related in the *Lancet*, as having been cured by Dr. Yeo, in a short space of time, by 15-drop doses of the ordinary tincture of *Belladonna* three times a day. These cases were very interesting to him in reference to the question of dose. He (Dr. Yeldham) had at the present time two cases of the same kind under treatment; one an adult somewhat advanced in years, who was afflicted with weakness of the bladder, and was taking 5-drop doses of the tincture of *Bell.* 3 times a day. The other, a little girl of seven years old, troubled with common wetting of the bed, who was taking a pilule of the 3rd decimal of *Bell.*, 3 times a day. He awaited the results with much curiosity.

Dr. DUDGEON said, that when Dr. Hughes read a paper on physiological points connected with the narcotic medicines, he

felt that he could not venture to criticise but must listen and learn. He would only refer to one point that had been touched on by the author, the action of *Belladonna* in the enuresis nocturna of infants. He had found it very successful in some cases, but in many others it had completely failed. One case of very obstinate enuresis recurred to his memory. It was that of a gentleman, at present an officer in the army, who had wetted his bed he believed every night since he was an infant and continued the practice still. He had tried every remedy in vain to stop it. The only nights he escaped were those when he wore a mechanical contrivance pressing on the urethra. He was the most profound sleeper he (Dr. Dudgeon) had ever met with. Nothing would apparently rouse him from his slumbers before his usual time. This almost comatose sleep had no doubt much to do with the enuresis.

Dr. HALE expressed his great satisfaction in listening to Dr. Hughes's paper, which proved that in working out the physiological action of drugs, he was working in the right direction. Dr. Hale's experience confirmed the value of *Belladonna* in the early stage of albuminuria, but only in the early stage. The case of a young boy of twelve under Dr. Hale's care at present, very aptly illustrated many of the points mentioned in Dr. Hughes' paper; the symptoms were involuntary urination during sleep, which was always very heavy and soporous, urine retained well during his waking but when voided was expelled with great force. A marked symptom of *Belladonna*; there was headache and spinal tenderness in the dorsal region; urine lemon-coloured and frothy. This boy was inordinately fat. The case was steadily improving under *Belladonna* 3, 3^r, and 1^r.

Dr. LEADAM remarked that in Dr. Hughes' résumé of the symptomatology of *Belladonna*, retention of urine occurred as well as enuresis.. He had found *Belladonna* used in the 30th dil. very efficacious in the retention that often results from a difficult or exhausting labour.

Dr. RANSFORD rose chiefly for the purpose of expressing his pleasure with Dr. Hughes's scientific and practical paper. He wished to ask Dr. Hughes whether he had tried *Cannabis sativa* ϕ in cases of incontinence of urine and irritability of the bladder, especially when a degree of paralysis of the bladder existed with or without enlargement of the prostate. He had used *Cannabis sat.* ϕ in one striking case of this sort. The patient was a clergyman, upwards of eighty years of age, who had been treated by various able allopathic surgeons without benefit. The employment of the catheter was necessary night and morning; constant dribbling away of urine was present. Immediate relief was given by *Cannabis*, the condition of the patient rendered comparatively comfortable, and his life prolonged. He had also used *Cannabis* σ . in other cases of incontinence of urine in which *Belladonna* had failed in his hands. In gonorrhœa with ardor urinæ *Cannabis*

Indica ϕ had succeeded with him better than *Cannabis sativa*. He presumed that in the remarks made by various members respecting the action of so many drugs upon the kidneys due allowance had been made for the greater or less warmth of the surface of the body.

Dr. MADDEN said it had been a real treat to him to hear Dr. R. Hughes's paper. He had always found that on subjects connected with physiology Dr. R. Hughes's remarks were most instructive. He much wished that a certain allopath, who had recently accused us of following an imaginary similarity, could have heard the paper. Dr. Yeldham had reverted to his favourite subject, the dose, in commenting on the use of *Bell.* in nocturnal enuresis, and had observed that he believed the lowest attenuation would do more good than the 3rd. He quite agreed with him as regarded the probable inefficacy of the 3rd, but experience had taught him that the extremes of the scale agreed more with each other in their effects, and hence his own custom was to give the 30th, 12th, or 1st when treating enuresis by *Bell.* As regards Dr. Yeldham's remarks that many medicines increase the flow of urine, and that hence this should not be considered as a specific action of *Bell.*, he would observe that he believed that enuresis was in many persons a natural mode of relief, and that in such cases everything which did that patient good, whether it were a drug or some hygienic measure, seemed to produce enuresis.

Dr. DRURY wished to make a remark or two without following exactly in the track of other speakers, or even the valuable communication of Dr. Hughes. While listening to what had been said he had endeavoured to recollect what medicines had proved most serviceable in his hands in urinary affections, and, while admitting the claims of *Belladonna*, he thought he had seen more marked effects from some others. He considered *Belladonna* of use in nocturnal enuresis, also in similar cases to those alluded to when otherwise indicated, but he thought he had seen other medicines more decidedly useful for the purposes for which he had given them. Thus *Dulcamara* was a favourite remedy of his where the urine was foetid, also when there was muco-purulent discharge. *Squill.* when there was smoky or bloody urine, *Apis* also had helped him in similar cases; *Cantharis* with violent strangury, and *Opium* when drowsiness and suppression of urine existed; *Lycopodium* with deposit of lithic acid. Of course many other remedies might be added to this short list, of greater or less efficacy, were he to enumerate those that might be of use under certain circumstances, but merely as a comparison with *Belladonna*, he thought he had seen more marked results from those he had named.

HOW FAR IS THE PRESENT NOSOLOGY ADAPTED TO HOMŒOPATHIC THERAPEUTICS?

By Dr. BAYES.

(Read before the British Homœopathic Society.)

THE following paper is to be looked upon more in the light of a few suggestive remarks than as an essay.

It must often have occurred, to many physicians practising homœopathy, that its opponents have at least some show of reason for the hostile criticism which stigmatises homœopaths as mere symptom curers, and which accuses them of being medical iconoclasts, who would overthrow or render wholly useless all the idols of accumulated scientific observation, which, having been handed down through long centuries, have enabled modern physicians to construct a nomenclature of diseases that differentially describes morbid conditions in their genera and species as definitely as zoology and botany describe those of animals and plants.

Of what practical use, to the physician, is it to know whether the disease in front of him is laryngitis or laryngeal phthisis if he is to treat the disease solely by its symptoms, and to accept no indication founded on its natural history or on its pathology?

Of what use is it to diagnose differentially between a strumous gland and one tumefied by syphilis, cancer, or the irritation of some skin affection, if the symptom and not its cause is to guide us?

These charges have been made, and with great show of reason, by men of the other school. They are true with regard to those homœopaths who follow the name rather than the genius of Hahnemann, and they are true as regards a very great deal of our practice; and it is this charge and these peculiarities, that have damaged the progress of homœopathy among the profession more perhaps than any other circumstance connected with the new system of medicine.

If nosology is to be no guide to our practice at the bed-

side, then either our diagnosis is so much vain research and our medical education to a great extent a sham, or else our therapeutics are imperfect, and its science has advanced less rapidly than that of our own knowledge of the natural history of disease.

Feeling this want of correspondence between the science of disease and our knowledge of its remedy, seeing that although one single morbid cause is able to create within the body a morbid direction, which runs through a number of different phases of diseased action, yet that we are compelled to use many medicines in successive order to cure the disease, many men have refused to accept the essentiality of disease, and have declared that there are no 'diseases' but only diseased conditions; that disease is not an entity but a succession of states—that each state requires a separate medicine to remove it, and that the whole disease is only to be removed in detail, fragment by fragment.

Such a position is not consistent with the requirements of a scientific age. If in many diseases we find ourselves compelled to adopt this fragmentary and imperfect method in practice, we ought to do so under protest, and simply to use it on the ground of expediency until we shall have discovered a more perfect and concrete means.

The weakness of symptom treatment is apparent even to intelligent laymen. I remember well an old major consulting me for chronic bronchitis. Knowing that he had for years been under a (so-called) purist, I asked him why he left him. "I left him, sir," he said, "because he was more like a little linen-draper than a doctor; it was always thus: 'Pins, sir? yes. What is the next article I shall serve you with? Neck-ties? yes, sir. What is the next article, sir?' and so on for ever. So with the doctor, he never took a comprehensive view of my case but it was thus: 'Cough, sir? yes, worse in the morning. We must cure that first, and then turn our attention to the next article. Constipation, sir? yes. What shall I cure you of next—headache? Certainly, sir, and then afterwards we'll attend to the pain in the shoulder.'"

Now, it occurs to me that those physicians who refuse to

look upon diseases as entities,* do not sufficiently consider the long series of observations on which the classification of diseases rest. There are undoubtedly a large number of diseases which run a specific course, and which are as certainly entities as plants or animals. For example, scarlet fever, smallpox, measles, syphilis, gout, typhus, typhoid, and intermittent fevers, and many other diseases run as definite a course and present as marked a speciality as they did in the day when they were first described. The scarlet fever of Sydenham exists to the present day with the same signs, symptoms, and general behaviour under similar conditions as it did in his day ; it has a specific form of propagation and of multiplication. It produces its like, and is followed by the same sequelæ now as ever.

We find that all the other diseases named, and many beside, invade certain tissues or organs in a definite succession, and induce certain lesions in a regular order.

It is quite true that there are *varieties* in each of these diseases, but these varieties themselves are capable of definition, and their presence no more argues against the "entity" of the disease than does the fact of varieties in a species of plants or animals argue against the correctness of their special classification.

Bearing in mind that the nomenclature of disease is not an arbitrary grouping of symptoms into classes, but that our present nosology really describes processions of "disease entities," which in many cases incubate, develope, effloresce, fructify, and disseminate their germs, which propagate the same disease from generation to generation of men, how far can our homœopathic method of therapeutics be brought to meet these diseases and to cure them ?

To me it seems that the first duty of the physician is thoroughly to differentiate his case by a careful and accurate diagnosis, not only arriving at the true nature of the disease, but also of its variety. Having done this he should seek for a drug which should cover not only the symptoms present, but whose pathogenesis shows that

* Using the word *entity* in its signification of "a particular species of being" (Webster).

the course of the symptoms it has the power to induce affects the same tissues or organs invaded, and beyond that, that it invades them in the same definite order as the natural disease follows, and induces the same sequelæ as are apt to occur subsequently to the natural disease.

That this is possible is shown by the action of *Mercury* in the Hunterian form of syphilis. Sir Astley Cooper said in one of his lectures that the mercurial disease was so similar to the venereal that it was often difficult to discriminate whether a case of secondaries was caused by the natural disease or by the remedy. *Quinine* too in common intermittent fever presents a case of true homœopathic cure; for long-continued saturation of the system with *Quinine* not only may induce an intermittent fever, but is followed by the sequelæ common to the fever, the cachexia, and engorgement of the spleen, the ringing or humming in the ears, and tightness across from ear to ear over the vertex, &c.

Therefore when we see a case of ague of the common type we give *Quinine* alone; we do not attempt to meet separately the chill stage, the hot stage, and the sweating stage, each by its similar, but we give a medicine which includes all these. Why then should we not seek to treat other diseases in the same manner, bringing nosology and pathogenesis into apposition?

I do not say that this is in our power at present in every case, but we ought to set this before us as our aim, and ought not to content ourselves until we are able to treat all other specific diseases with the same success as we are able to bring to bear on these. But when we shall have discovered single medicines bearing the same relation to each specific disease as *Mercury* does to syphilis, *Quinine* to ague, *Belladonna* to the common smooth scarlet fever, *Pulsatilla* to measles, *Chamomilla* to teething, we still have the varieties to meet. The soft chancre form of syphilis, the phagedenic, the many varieties of ague, the miliary form of scarlatina, &c.

And here we find ourselves forced to diverge largely from the *Nomenclature of Diseases* as issued by the joint committee of the College of Physicians of London.

The ordinary nosology fails to meet the essential differ-

ences in the nature of the varieties of disease. Take for example ague: the standard nomenclature above named has divided this disease into four varieties.

- a.* Quotidian.
- b.* Tertian. Sub-variety double tertian.
- c.* Quartan. Sub-variety double quartan.
- d.* Irregular. Brow-ague.

The homœopathic system of medicine does not recognise that classification of varieties, nor, indeed, does the allopathic system of therapeutics. The same medicines cure all these varieties, and therefore the inference is fair that the disease is the same, and that the different chronicity of the diseases arises from the diverging circumstances in which the same morbid cause finds itself placed rather than from any essential difference in the morbid cause itself.

The varieties recognised by homœopaths, on the other hand, are real, and not simply apparent, since they demand a divergence from the common treatment for their cure. Ague from a homœopathic point of view may be thus classified:—

The common form without complications, consisting of the three well-marked stages, neither preponderating greatly over the other, and none missing or masked; the chills with blue lips and nails; the hot stage, one of genuine fever; the sweating stage, giving almost full relief, and the interval between the paroxysms, leaving the patient well enough to follow his avocations, though with some languor, corresponds exactly to *Quinine* or *China*, and is usually well cured by this remedy. Then follow these varieties:—

a. Corresponding to *Arsenicum*. Threatening to run into a remittent fever; stages less marked, less irregular in their mutual chronicity; greater irritation of mucous membrane.

b. Corresponding to *Cedron*. Paroxysms exact in their reappearance as to time, but not being quotidian, tertian, or quartan (*e. g.* coming on every four hours during the day, or once in three weeks, but always attacking exactly at the same hour of the day).

c. The chill fit appears alone, or is very greatly predominant.

Veratrum is useful in these cases, but so also is *China*.

d. The hot fit predominates largely, or appears alone. *Aconite* corresponds to this.

e. The sweating stage predominates largely, or appears alone. *Phosphoric acid* corresponds to this form.

f. Localised intermittents. There are very numerous sub-varieties of this form.

1. Affecting the brain ; corresponding to *Opium*, *Belladonna*, *Hyoscyamus*, &c.
2. Affecting the peripheral nerves and causing neuralgia ; corresponding to *Belladonna*, *Spigelia*, *Kalmia latifolia*, &c.
3. Affecting the chest ; corresponding to *Arsenicum*, *Phosphorus*, &c.
4. Affecting the stomach and abdominal viscera ; corresponding to *Ipecacuanha*, *Nux vomica*, *Mercurius*, &c.
5. Affecting the kidneys ; corresponding to *Cantharis*, &c.
6. Affecting the uterine system ; corresponding to *Pulsatilla*.
7. Affecting the joints and synoviæ ; corresponding to *Bryonia*.
8. Affecting the ligaments and tendons ; corresponding to *Rhus*.
9. Affecting the muscular system ; corresponding to *Arnica* and *Cimicifuga*.

This last variety and its sub-varieties are usually chronic, but I have seen them at the outset of the disease in a very acute form, particularly as affecting the lungs, the stomach, and the kidneys.

Usually the medicine corresponding to the local affection cures the patient, but I have seen it in a few cases merely change the venue of the disease, which has developed into common intermittent when the local manifestation has been overcome. These cases ought properly to be classed with the variety next named.

g. The suppressed form, which demands, as the former, that the organ chiefly affected should be relieved by the corresponding medicine.

h. The cachectic, to be treated by high dilutions of the medicines primarily indicated.

It may be objected that such a system of nosology is far more complex than that at present in use, but I would answer that it is not more complex than the varieties of the diseases it professes to describe, and a nomenclature based upon such a classification would possess the advantage of giving some indication of the remedy. Let us take as another example Rheumatism.

The standard nomenclature thus describes the disease and its varieties :—

1. Acute rheumatism. Variety, sub-acute rheumatism.
 2. Gonorrhœal rheumatism.
 3. Synovial rheumatism.
 4. Muscular rheumatism. Local varieties : *a.* Lumbago.
b. Stiff neck.
 5. Chronic rheumatism ;
- And, under the head of disorders of the generative system,
6. Rheumatism of the uterus.

Under the head of acute rheumatism no variety thus appears, as the subacute form is simply a question of degree. As homœopathic practitioners, this nomenclature gives us only a very vague indication ; to make it practically useful to us we must differentiate acute rheumatism into at least four varieties.

1. Acute rheumatism or rheumatic fever.
 - a.* Worse during movement ; corresponding to *Bryonia*.
 - b.* Worse during rest ; corresponding to *Rhus*.
 - c.* Worse when warm ; corresponding to *Mercurius*.
 - d.* Neuralgic, with more sweating than fever, and more pain than swelling ; corresponding to *Phosphoric acid*.
2. Gonorrhœal rheumatism ; corresponding to *Pulsatilla*.
3. Synovial rheumatism ; corresponding to *Arnica* and *Ledum*.
4. Muscular rheumatism ; corresponding to *Arnica* and *Cimicifuga racemosa*. Sub-varieties :
 - a.* Lumbago ; corresponding to *Aconite* and to *Rhus*.
 - b.* Stiff neck ; corresponding to *Bryonia* and *Cimici-*

fuga racemosa (so far in muscular rheumatism we follow the standard nomenclature, but must enlarge it).

c. Intercostal rheumatism ; corresponding to *Arnica*.

d. Rheumatism of the face and scalp ; corresponding to *Mercurius* and to *Kalmia latifolia*.

5. Chronic rheumatism (no varieties are named in the standard nomenclature, but we know there are many).

a. Rheumatic neuralgia (to enumerate the sub-varieties of this one variety would require almost an essay in itself, since we must in that case pass in review every nerve lying near enough to the surface to have its sheath affected by climatic influences). The best known sub-varieties are—

a. Rheumatic, facial neuralgia ; corresponding to *Kalmia latifolia*.

b. Rheumatic sciatica ; corresponding to *Colocynth*, and sometimes to *Aconite* or *Belladonna*.

c. Rheumatism of the articulation of the jaw ; corresponding to *Causticum*.

6. Rheumatism of the womb ; corresponding to *Aconite* and *Pulsatilla*.

To the varieties above named we might add, under the head of complications, rheumatic endocarditis, and pericarditis, rheumatic ophthalmia, &c., since these complications each demand specific treatment.

Necessarily short and imperfect as this sketch has been, I think I have sufficiently indicated a field of inquiry which, if successfully explored, would lead to great practical results.

Therapeutics and nosology ought to fit the one into the other, as the ball into the socket-joint ; when these two parts of medicine do so fit one into another, curative results are frequent and satisfactory ; where they do not so fit we ought to strive to make them do so.

Where therapeutics and nosology cannot be made so to fit, then either therapeutics or nosology is at fault, and is untrue : such fault or untruth ought to be exposed, corrected, and reformed, and we must set ourselves fearlessly to this task if we would remain, as we profess to be, in the van of medical progress.

Discussion on Dr. Bayes's paper.

Dr. HALE, while acknowledging the subject of Dr. Bayes's paper to be of some interest, considered nosology, although useful for giving names to diseases, was to the homœopathist of comparatively less value practically than to allopathic practitioners, and, indeed, may become a positive evil if it lead to treating a *name* instead of a *condition*. Nosology, moreover, very imperfectly expresses the actual pathological condition existing in disease: for example, pyæmia, uræmia, certainly conveniently express the presence of pus in the circulating fluid in the one case and urine in the other, but these words do not represent strictly the diseased condition or its cause. Nosology is useful in so far as it enables medical men of both schools to understand what each may be talking or writing about. Dr. Hale could not agree with Dr. Bayes's statement that disease was an *entity*; such a theory he believed has been given up by most modern physicians. Disease is a perversion of the normal or physiological conditions of organs, health being the result of the normal or physiological functions of all the organs of the body in harmonious action.

Dr. HOUGHTON hoped that Dr. Bayes would fully explain the difference between his system of nosology and the leading idea of Dr. Sharp's "organopathy." Remedies act not only on special organs, but also on special tissues and parts physiologically related. Nature knows nothing of the names which we give to diseases, but they are useful for purposes of classification. Our present definitions of disease are far from accurate. Is the *materies morbi* of scarlet fever or smallpox a disease? and may not the patient be considered well until some symptom shows itself? He considered the reaction of nature against abnormal conditions or noxious substances to constitute the essence of disease.

Mr. POPE said that he thought Dr. Bayes would have added to the clearness of his paper had he defined the meaning which he attached to the word "entity." As ordinarily understood this word meant a being in separate existence. So thoroughly was the notion that disease was a separate existence exploded that he could scarcely think that this was the meaning Dr. Bayes attached to the word. That disease was of the nature of an entity; that it was something which entered into the body and consequently had to be expelled from it was indeed the view held by pathologists many years ago. But it was quite otherwise now. It was almost universally admitted that disease was simply a state implying a disturbance of the ordinary physiological processes which when conducted normally we called health. Dr. Bayes had argued that because the diseases resulting from the application of specific poisons are the same now that they ever have been, therefore they

must be entities. But this consistency depended upon the consistency of the nature of the poison. A hammer of a given weight forcibly applied to the thigh would produce just the same contusion now that it would have done thousands of years ago; but it did not follow that the contusion was an entity. Those who might have any lingering fondness for the idea that disease was an entity might very advantageously, Mr. Pope thought, read a paper by Dr. W. B. Richardson, an abstract of which appears in two recent numbers of the *Medical Times and Gazette*.^{*} There Dr. Richardson clearly shows that the specific diseases, scarlatina, smallpox, and so on, do not depend upon the reproduction within them of entities, but arise from the application of an organic poison to a secretion for which it has an affinity, and by reason of this contact vitiating that secretion in identically the same direction as the poison, and converting that secretion—normally harmless—into a rank poison. With regard to the value of nosology he would ask what is nosology? and would reply that it is simply a system of naming diseases. A nosological name is a name which enables us to recall to mind certain collections of symptoms. Thus the name rheumatism suggests the presence of pain, swelling, and lameness. But it does no more. It does not, and never can, guide our treatment. The pathological condition which the word rheumatism will suggest will entirely depend upon the view which the person who hears the word pronounced may have of the nature of the disease; and hence rheumatism may pathologically mean one thing to one physician and another to some one else. The end attained by nosology is that which all naming is intended to achieve—it assists us in communicating our thoughts to others. But it serves to very small purpose in guiding our treatment. The latter portion of Dr. Bayes's paper, that where he illustrated his amended nosology, might, Mr. Pope thought, be utilised in the preparation of the therapeutic part of the *Hahnemann Publishing Society's Repertory*.

Dr. DUDGEON was surprised to hear Dr. Bayes say that it was unphilosophical not to regard diseases as entities. He thought the tendency of modern philosophy was quite the other way. Dr. Bayes seemed to him to have taken a step backwards. Possibly Dr. Bayes might say it was a case of *réculer pour mieux sauter*; and if Dr. Bayes's backward step enabled us to practise more successfully nothing could be said against it. But the further development of Dr. Bayes's views did not show this to be the case. The present nosology as given to us authoritatively, Dr. Bayes found to be not sufficiently subdivided, so he would divide the classes and species of disease into further varieties, in order to attach to each of these varieties the idea of a remedy. Thus he had said that he would take the disease rheumatism of the nosological work and subdivide it into rheumatism ameliorated by motion for which the remedy was *Rhus*, rheumatism ameliorated

^{*} October 29 and November 5.

by rest, the remedy for which was *Bryonia*, rheumatism from gonorrhœa with *Pulsatilla* for its remedy. Now, with respect to these examples, we had all met with cases of rheumatism that were much aggravated by motion, which were curable by *Rhus*, he would instance that very painful disease lumbago; again, many rheumatisms relieved by rest were not curable by *Bryonia*, and as for *Pulsatilla* being the remedy for gonorrhœal rheumatism he only wished it was. He had lately treated two cases of gonorrhœal rheumatism simultaneously; in both the right knee was affected. One was cured in three days by *Bryonia*, the other lingered on for weeks, and many remedies were unsuccessfully given before it was cured; among the rest *Pulsatilla* was tried, but it was of no use.

Mr. CAMERON, having doubts whether he fully comprehended the scope and purport of Dr. Bayes's excellent paper, would not occupy the time of the Society by venturing any remarks which might be wide of the mark upon it. He felt himself, however, compelled to intrude for a few minutes upon the attention of the meeting in consequence of what he could not but consider the erroneous ideas which some gentlemen who spoke in the discussion seemed to attach to "vitality" whether as regarded health or disease. The words "entity," "principle of life," even "principle of *death*" had been used to mean something material or immaterial superadded to, or distinct and separate from, the phenomena to the sum of which we give the name of life. Now of life we know nothing, and never can know anything, separate from those phenomena which we observe. Take away these phenomena and nothing remains for our investigations. The actions of living bodies are not more obscure or mysterious than the actions of inorganic bodies. Then the phenomena of chemical or mechanical action are more in need of a "principle" for their explanation. Life is a series of actions resulting from the property of irritability called into play by its appropriate stimulus, just as chemical action is the result of properties of matter each excited by its proper agent or stimulus. There is nothing in the nature or condition of vital actions which requires us to reason upon them in any other way than we do on the phenomena of the inorganic world. We may as well call in a principle of combustibility, of crystallization, of elasticity, of adhesion, of repulsion, of motion, &c., to explain the phenomena included under these terms, as demand a principle of vitality to explain the phenomena to the aggregate of which we give the name of life. We know no more of the nature of combustibility, or of attraction, or of crystallization, than we do of the nature of vitality; we know them only from their phenomena, we know nothing of them in the abstract. It is unscientific to propose to ourselves any other object of research in such matters except these phenomena. To our powers none others exist.

Dr. YELDHAM said, nosology was merely name-giving, and was employed to indicate a disease the general characters of which

were previously known, and, in this way, to save the trouble of definitions and descriptions; and so long as it answered the purpose, the simpler it was the better. An elaborate nosology tended to hamper one's thoughts and freedom of action.

Dr. MADDEN observed, that he thought Dr. Bayes had scarcely done justice to himself or that the speakers had done him justice. He very much doubted if Dr. Bayes held the views about "entity" which his paper would give him the credit of holding. He believed that Dr. Bayes rather meant that many diseases had an assemblage of phenomena so marked and so almost universally recurring that it stamped an individuality upon the disease. This, however, by no means implied that there existed any substantial "entity" of which the disease could be said to consist. As regards specific poisons he did not agree with those who traced them to germs, if such germs are to be looked upon as distinct organisms foreign to the body. On the contrary, he believed that what were called germs were modified normal tissues gifted with the fatal power of modifying in the same direction the healthy similar tissues with which they come into contact. Just as one terrified animal in a herd of cattle will affect the rest and create a stampede, so a particle of intestinal epithelium, modified by cholera (for example), will influence large tracts of intestinal membrane, and drive to destruction masses of epithelium which were previously sound and healthy.

Dr. BAYES said in reply, he feared the real drift of his paper had been a little less plainly stated than it ought to have been, and had been somewhat overlooked in the discussion. His intention was to show the defective mutual working between nosology and therapeutics, and to endeavour to point out some method of reconciling them. If disease be not accurately described according to its natural phenomena by our present nosology, then our nosology ought to be corrected in such a manner as to describe what we have to treat, *i. e.*, to give us our therapeutic indication. As to the question of the being, the existence of zymotic morbid causes as "entities" as "essential," he thought we had the best possible proof of it in the facts relating to the infectious nature of scarlet fever. He knew an instance where scarlet fever broke out in the family of some people of wealth living in the country; they left their house and travelled abroad for two years; the rooms were cleansed and purified and whitewashed. But on their return a governess, who slept in the room infected, took the disease at once. Now, there must have been some scarlatinal "entity," some scarlatina being, remaining in that room all this time which fastened on and fructified within this new comer. He believed scarlatina and smallpox to be as much "entities" as the grains of wheat or the seeds of any other plant. It is all very well to say, as Mr. Pope did, that they could only develop themselves where the human body is present and had no separate existence. The first may be true, but as to the second the facts

prove otherwise. The grain of wheat cannot show its vitality until it meets certain surrounding physical conditions. It may lie on the mantel-piece or be enshrouded with a mummy for thousands of years, but put it in the ground and moisten it and give it warmth, and it shows its living existence. The scarlatina in like manner does not develop until it meets with its congenial soil and is fertilised by the living blood, but then it shows itself, though for two years (as in the case named) it had lain dormant. Mr. Pope, who objects to zymotic morbid causes being called "entities," himself terms them "specific poisons." Is not a "poison" an "entity?" There are undoubtedly many diseases which are not "entities," but only functional derangements; still these negative diseases do not disprove the existence of the positive. But all this is a digression from the subject under discussion. The ruling idea of the paper is that nosology, to be practically useful to us, should first describe the common form of a disease which shall correspond to the specific remedy, and then should further describe the varieties in such a fashion that each variety shall also correspond to a specific remedy. With regard to some other observations on the individualisation of each case. When we are told that we are not to treat diseases in their entirety, but merely to treat the exact picture of the symptoms present at the time of our examination, we are liable to be led into very grave errors. When he (Dr. Bayes) was but a beginner in homœopathy, he was once misled by an adherence to this rule, and it had been a lesson to him ever after. He was called to see a lad who was in a state of high fever following a chill and gave *Aconite*; perspiration set in soon after the administration of the *Aconite*, and in a few hours the lad seemed well. But the day after a repetition of the chill, fever, and sweat showed the case to be one of ague. He thought that this illustration proved the danger that may arise from accepting the totality of the symptoms before us, at the time of our visit, as the indication for our treatment, and conceived that a good nosology founded on the true natural and pathological history of the whole disease, past, present, and imminent, should be the indication which we ought to meet. In spite of all that has been said to the contrary he believed that all physicians do take nosology as their guide more or less. If they did not the study of each case would take up so much time that no man, in active practice, could follow out his profession with such a want of system. We treat ague as ague in nine cases out of ten or even more. We treat syphilis as syphilis in a still larger proportion, each by their specific remedy, and when we find varieties in these diseases which fail to correspond to *Quinine* or to *Mercury*, we then fall back upon individualisation according to the idiosyncrasy. He hoped some day nosology would be so reformed as to be a real guide to us in the treatment of all diseases.

CASES TREATED AT THE LONDON HOMŒO-
PATHIC HOSPITAL.

By Dr. R. D. HALE.

CASE 1.—*Infantile atrophy.*

HENRY CLARKE, 'æt. 3 months, out-patient, admitted July 7th, 1869. This infant, a first child, the mother a small delicate woman, is in a state of extreme emaciation and debility, constantly crying, and evidently suffering great pain in the abdomen, which is somewhat tumid and very tender on pressure; bowels relaxed, the fæces being pale in colour and very foetid. Nearly all the milk taken is vomited. There is a chain of hard and enlarged cervical glands. The face has an old and distressed expression, and the skin is tightly stretched over the prominent bones. The pulse is scarcely to be felt, and almost uncountable in frequency, the case presenting altogether a most distressing and almost hopeless condition. The medicine prescribed was *Calcareo carbonica* 30 ter die. The only food to be two parts of fresh cow's milk diluted with one part of water.

July 21st.—Much the same. *Calc. carb.* 6 ter die.

28th.—Better; fewer motions; better colour; less pain; no vomiting. Repetition.

August 4th.—Still rather better; fæces more healthy; gaining some flesh. *Calc. c.* 30 ter die.

11th.—Not so well; has lost flesh; fæces undigested and too frequent; verge of anus excoriated. *Cham.* 12 4tis horis.

18th.—Better; fæces green; there is excoriation of the anus. *Merc. sol.* 12 ter die.

25th.—Looking better; gaining flesh again; has fits of crying; cold sweats after crying; fæces foetid. *Ars. alb.* 30 ter die.

September 1st.—Motions very green and foetid. *Rheum* 3 ter die.

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8th.—Better; fæces much less foetid. Repetition.

15th.—Much better. Repetition.

29th.—Diarrhœa returned; fæces foetid and very green, and passing directly food is taken. *Ars. alb.* 30, glob. i, 4tis horis.

October 20th.—Much better; gaining flesh; fæces more healthy. *Ars. alb.* 100, glob. i, bis die.

November 3rd.—Much better; fæces healthy; some tenesmus. *Cham.* 12 2dis horis.

17th.—Greatly improved; bowels are costive; urine ammoniacal. *Bryonia* 30 ter die.

December 8th.—Urinary symptoms much the same; the urine excoriates. *Merc. sol.* 30 ter die.

22nd.—A pustular eruption has appeared on the scalp, and there is an enlarged lymphatic gland behind the ear. *Calc. carb.* 30 ter die.

January 19th, 1870.—Urine still scalds; bowels are costive. *Sulphur* 30 ter die.

February 2nd.—Urine still scalding and excoriating; cutting teeth. *Cham.* 12 4tis horis.

16th.—Better. *Cham.* 30 ter die.

March 2nd.—Better. *Cham.* 12 ter die.

From this date until May 30th the child, with the exception of the disturbance in the system caused by dentition, continued steadily to gain flesh and strength, the limbs became plump, and the only medicine prescribed was *Cham.* 12.

April 13th.—The secretion from the kidneys is still high coloured (brown), there are enlarged cervical glands, and the teeth are very slow in being developed. *Calc. c.* 30 ter die.

27th.—Glands still enlarged. *Baryta c.* 30 ter die.

May 25th.—Glands still enlarged; the eyes are red, and a good deal of discharge. *Euphrasia* 3^x ter die.

June 29th.—The child is thriving well, and with the exception of a tendency to diarrhœa, may be considered quite well.

Remarks.—This case offers nothing very remarkable, and to experienced homœopathists nothing which will be con-

sidered very noteworthy, yet it is hoped that it may be found instructive and encouraging to those commencing the practice of homœopathy, inasmuch as it illustrates and proves the power of infinitesimal doses of the medicines employed, one especially, namely, *Calcareo carbonica*, which may be said to have formed the basis of the treatment, and which has a well-known and well-proved relation to the young organism, especially when the 'strumous diathesis is well marked, as it was in this case ; and it was chosen because of its extraordinary dynamic action not only upon the glandular system in particular, but having apparently a much wider range of action, penetrating as it appears to do and energising the various organs and tissues of the entire economy. *Calcareo carb.* is one of those medicines whose action it is maintained cannot be localised ; and be it observed in this child's case a general dyscrasia had to be dealt with, the origin of which probably commenced in what is loosely termed mesenteric obstruction, but by a more modern and correct pathological definition would be described as a suspension of cell-life, and the metamorphosis which accompany it, and which are essential to the formation and transmission of the chyle into the current of the general circulation. Whether this be the true explanation or not of infantile atrophy, the condition of the child proved beyond doubt that there was some cause profoundly affecting the processes of assimilation and secretion essential to the preservation of life.

The other medicines employed in this case were to meet intercurrent symptoms during its progress, such as *Arsenicum*, which played an important part in arresting the diarrhoea and supporting vitality. *Rheum* acted well in altering the character of the intestinal secretions when they were very foetid, and *Chamomilla* in soothing the irritation in the system caused by dentition. It is but right to state that the short and hurried notes of this case, as they necessarily must be of the out-patients, give but a very imperfect description, either of the child's condition when first admitted or of its progressive improvement ; and only those who had the opportunity of seeing the case from the day when it was

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admitted a wretchedly emaciated and suffering little patient, can realise the complete restoration effected. When last seen the child presented the appearance of a lively, plump, and thriving baby.

CASE 2.—*Eczema impetiginoides.*

Elizabeth Christmas, æt. 34, was admitted on October 24th, suffering from *eczema impetiginoides*.

History.—Patient states that she had a very severe attack of smallpox three years ago. For about ten months afterwards she enjoyed good health, but since that time she has been suffering constantly from weakness, giddiness, dimness of vision, &c., catamenia present about every six or eight weeks, and the flow is always scanty. Three weeks before admission an eczematous eruption appeared on the back of the neck, and soon spread forwards over the face and scalp.

Symptoms on admission.—*Eczema impetiginoides* involving the neck, face, and scalp, with patches on both elbows and below the armpits. These parts are covered with thin crusts, and discharge very freely. A good deal of burning pain in the affected patches. Discharge semi-purulent and very offensive. Patient weak and low-spirited, complaining of constant headache. Appetite very bad; tongue clean and moist; bowels regular. *Bell.* 3, gtt. i, 4ta quaque horâ. Eczematous patches to be covered with water-dressing, and kept constantly moist.

October 28th.—Much the same. *Sulph.* 6, gtt. i ter die.

31st.—Little change; same dressing to be continued. *Bell.* 3^r, gtt. 4tis horis in die. *Sulph.* 1^r, gr. i, horâ somni.

November 3rd.—Considerably worse. Eczema has spread rapidly, and the constant discharge is telling on the patient's strength. Both eyes inflamed. Patient so weak that she can only lie on her back. Headache and restlessness at night continue, and she is slightly delirious at times. She

can hardly be got to take any food. *Arsen.* 3, gtt. 4ta quaq. horâ; *Rhus* 3^x, gtt. 4ta quaq. horâ, alternately.

6th.—The symptoms have gone from bad to worse up till this time. Patient very low, and evidently in a critical state. Radial pulse almost imperceptible; patient unconscious and delirious. The discharge from the eruption is profuse, and its smell, which closely resembles that of smallpox, is so offensive as to taint the whole ward. Eyes more inflamed, and the eyelids agglutinated. *Arsen.* 1, gtt. 2dâ quaq. horâ; *Merc. cor.* 3, gtt. 2dâ quaq. horâ. Three doses of each to be given alternately. Ordered four ounces of brandy, two eggs, and two pints of milk daily. Patient to be fed every two hours.

7th.—Much improved; is conscious to-day; appetite returning; pulse stronger. *Merc. cor.* 3, gtt. i, 2da quaque horâ.

14th.—Patient has improved every day during the last week. Eruption is rapidly drying up. Patient is regaining strength. She has continued steadily with the *Merc. cor.*, with an occasional dose of *Arsenicum* whenever the symptoms of depression were very marked. Complains of headache and depression of spirits. *Bell.* 3, gtt. i, 3tia quaq. hora.

21st.—Crusts quite gone. Skin still red and tender, but there is no discharge. Very restless at night. *Pil sacchori lactis* ter die. *Hyoscyamus* 3, gtt. horâ somni.

December 1st.—Convalescent, and able to be up for two or three hours every day. *Calc.* 30, gtt. i, ter die.

CASE 3.—*Disease of brain.*

Walter Wackett, æt. 41, a stationer, admitted an in-patient February 9th, 1869.

The history of this case is, that he enjoyed good health until twelve years ago, when he met with a railway accident in consequence of a collision, causing as it is conjectured concussion of brain and spine. For some months following the accident was unable to keep liquid on stomach well.

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Has been married two years, and has always been a very steady man. Continued in what appeared to be very good health until six years ago, when he had a fit which seized him like an electric shock. Since then the sight of the right eye has been imperfect. Had a second fit two years ago, followed by paralysis of the left hand; the fits occurred at night. Had a fit on Sunday night last (February 7th), and on awaking in the morning found great difficulty in articulating, and could not swallow.

February 10th.—The house surgeon gave him *Pulsatilla* 3^r every four hours.

14th.—Was seen by Dr. Hale. There is complete aphasia; the tongue is tremulous and thickly furred; breath foetid. He tries to articulate, but can only move his lips. The pupils are sluggish, but not dilated. His hand is so unsteady that his writing is illegible, and it is impossible to make out anything as to whether he is suffering pain or any other subjective symptom. Ordered *Nux vom.* 3 every four hours; diet, minced meat, beef tea, and milk.

18th.—From the last date the symptoms remained much the same, but to-day there is some flushing of the face and heat of head, and he appears to have pain in the head. *Bell.* 3, one drop every four hours.

21st.—The symptoms to-day are very alarming; there is great nervous prostration; the face is ghastly; lips bloodless; pupils dilated, and insensible to light; pulse excessively feeble, and to all appearance he is sinking. *Arsenicum* 3, one drop every four hours. Beef tea and milk.

28th.—After a few doses of the *Arsenicum* he made a remarkable rally; reaction has set in during these few days: so much so that it is thought necessary to repeat *Belladonna*, and it was prescribed in the 30th dilution, to be taken for a few hours, and then *Arsenicum* 30, every four hours. Ordered 1st diet.

March 7th.—Symptoms of paralysis of tongue and retina continue. *Plumb. c.* 12, one drop three times a day.

14th.—Symptoms much the same; pupils dilated and insensible to light. *Bellad.* 30, one drop three times a day.

25th.—Paralysis extending; urine voided during sleep;

cannot speak yet, and every attempt to do so is accompanied by that sort of hysterical laughter which is so frequently noticed in the paralytic. *Causticum* 5, one drop three times a day.

April 1st.—The most prominent symptom is the nocturnal enuresis; speech improving. *Cicuta vir.* 3 \times , three times a day.

7th.—Much the same: involuntary urination at night continuous. *Bellad.* 3, three times a day.

11th.—Bowels relaxed; debility. *Ars.* 1, every four hours.

18th.—Still weak. *Ars.* 3, every four hours.

April 20th.—Stronger, but in other respects much the same; pupils still sluggish. *Bellad.* 6, half-drop, every four hours.

May 2nd.—Rather better. *Bellad.* 12, half-drop, every four hours.

6th.—Improving steadily, but there is still great feebleness of brain, and although he articulates better, his speech is still very indistinct, and the urine is passed involuntarily when asleep. *Phosphorus* 6, one drop three times a day.

From this date until the 8th of June, when he became an out-patient, his progress was most satisfactory. Articulation becoming more and more distinct, the nocturnal enuresis has ceased, the pupils have regained their contractility. *Phosph.* 3.

June 8th.—Became an out-patient; aphasia improving. *Phosph.* 3, three times a day.

28th.—Better, speaks more articulately. *Phosph.* 30, three times a day.

July 6th.—Better; can retain urine better; speaks better; has pain over the right eye; feels weak. *Canth.* 3, three times a day.

20th.—Better. *Phosph.* 3, three times a day.

August 10th.—Better. Repetition.

24th.—Feels weak; right eye swollen. *Ars. alb.* 30, three times a day.

Sept. 14th.—Feels stiffness in the joints; crampy feelings in the lower limbs. *Cap.* 5, three times a day.

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21st.—Papular eruption on the chest ; pain in the head. *Sulph.* 30, three times a day.

Oct. 5th.—Eruption better ; head painful ; slight ptosis of the right eyelid. *Baryta c.* 30 three times a day.

19th.—Better. Repetition.

Nov. 2nd.—Improving ; some ptosis of the right eyelid remains. *Baryta c.* 12, three times a day.

16th.—Better in all respects, but the slight ptosis and some thickness of speech remain. *Baryta c.* 30 three times a day.

Remarks.—This is an instructive if not an interesting case. It illustrates very forcibly the well-known fact that the remote effects of concussion often take a very long time to develop themselves. Six years appear to have passed, during which time his wife reported that he was in very good health, when, without any apparent cause, he had a succession of fits, followed by partial loss of vision of the right eye. For two years he had no fit, but at the expiration of that time had a fit, on recovering from which his left hand was paralysed. Two days before admission into the hospital he had a fit, and on the return of consciousness he could neither articulate nor swallow, although he made attempts to do both. Here, then, we have evidence of deep-seated central mischief involving the origins of the second and third and ophthalmic division of the fifth and ninth pair. The spinal marrow also must have participated in the shock caused by the accident, as was shown by the paralysis of one hand, which followed one of the fits, and subsequently, while in the hospital, the paralysis of the sphincter muscles of the bladder, whose nervous supply is derived from the sacral plexus. With regard to the treatment a difficulty arose from the impossibility of getting any subjective symptoms from the patient ; the objective symptoms were, however, well marked, and the treatment was based upon a clear observation of these, a characteristic feature of which, during the first week or two of treatment, was the marked alternations between cerebral vascular excitement on the one hand and nervous prostration to an extent on one occasion resembling a moribund condition on the other.

These conditions were met by *Belladonna* during the hyperæmic state, and *Arsenicum* during the period of collapse; the restorative power of the latter being most remarkably shown by the result of its administration on February 21st. It ought to have been mentioned in the hospital notes that the power of swallowing very soon returned after his admission; it ought also to have been stated that after he had been in the hospital for two or three weeks, his appetite became almost voracious. In the further management of this case two other medicines played a very important part in restoring brain powers; these were *Phosphorus* and *Baryta carbonica*. That a complete cure is to be looked for is more than can be hoped, because it is to be feared that there is structural lesion deeply seated. Homœopathy can, however, fairly claim the credit of having not only saved this man's life when threatened with extinction, but it has succeeded in removing the functional lesions as far as the condition of things would allow.

CLINICAL RECORD.

Nervous Congestive Headache. By ROBERT T. COOPER.

The following case, it occurs to me, will be considered of interest as being a small contribution to our knowledge of the curative sphere of *Cannabis Indica*, the real Indian hemp. The form of headache below described I had met twice before, and in both instances failed to give relief, and therefore rejoice to find a remedy for it in the Indian hemp.

Rebecca S—, æt. 18, by occupation a dressmaker, was admitted to the Southampton Homœopathic Dispensary the 22nd June, 1870, then suffering from a severe headache which she had been afflicted with for the last two months. She is very nervous, and trembles at the least source of agitation. Feels a heavy weight at the back of her head, whence the pain takes its rise: this sensation is continual. A shooting pain extends from this and darts up the sides of the head to the temples and vertex capitis. These attacks are worse in the middle of the day; is obliged to cry, so fierce is the pain; feels as if her head was opening and shutting at the top, and as if the calvarium was being lifted up—in fact as if it was “too full just there.” Her countenance is dejected and careworn when she gets up in the morning, she suffers a good deal from flatulence, and there is much pain in the small of the back, which is worse at the catamenial periods. These last are scanty and make their appearance every second week.

Sleep is very restless; bowels are regular; appetite is very bad, and she complains of a nasty taste; her tongue, however, is quite clean. R. *Cannabis Indica* ϕ , gtt. v, Aquæ ʒij m, gtt. v in Aquâ ter die.

June 29th.—Improvement has been most marked, but on Thursday and Friday her headache was very bad; has been sleeping very comfortably. Period came on more profusely than usual—in fact quite naturally. Continue.

July 9th.—Expresses herself as being nearly well, though she still complains of a severe headache, which seems to be worse on Thursdays and Fridays. “Her nerves are much stronger,” and she does not tremble, nor can she be easily worried as she used. There is no bad taste, and her appetite is much improved. *Sacch. lactis*.

This was her last visit in person, but in about ten days afterwards she sent a request for more medicine and a message to say she had improved in every way.

Compare this case with the assertion that, among the toxic effects of Indian hemp, “there may be pain in the head, and ‘a feeling as of the brain boiling over, and lifting the cranial arch like the lid of a tea-kettle.’”—*Handbook of Therapeutics*, p. 394, by S. Ringer.

A near approach to this form of headache obtains in the symptoms of *Argentum nitratum*, “sensation as if the body, and especially the face and head expanded; *he feels as if the bones of the skull separated with increase of temperature.*”

The next case is one of chorea, which was met with a short time ago, and is, we think, remarkable as being so strikingly confirmatory of E. M. Hales’s remarks.

Sarah R—, æt. 15, a delicate-looking country girl, was brought to the dispensary by her mother on the 20th April, 1870, suffering from the consequences of delaying menses. Last Christmas the catamenia first made their appearance, and since then she has been but once unwell. She gets gradually weaker, and choreic movements are now showing themselves; she cannot keep still and lets drop anything she takes hold of. Her bowels are confined and the urine is cloudy, but there is no history of rheumatism. *Arsenicum* 3^x, a grain in seven dessertspoonfuls of water, a dessertspoonful three times a day; to have cold baths every morning.

April 27th.—Is feeling stronger, and the bowels are more regular. I find the tongue to be rather coated and the tonsils are enlarged. Continue.

May 11th.—A favorable report returned per messenger.

18th.—This morning the mother brought her to see me, as she is exceedingly anxious about her progress. Her daughter has been getting much worse, and this although her general health appears to be improving. The bowels are regular and her appetite is good, but the choreic movements are on the increase. She

cannot dress herself, and is affected with constant twitches and silly movements of the hands and legs. All her body moves about; *twitching of the legs, even when asleep*; is unable to talk so as to be understood. *Stramonium* 12.

28th.—Dispenser (in my absence) reports her “much worse.” Continue.

June 1st.—Continues to become very much worse; *cannot walk at all*; *vomits food*; cries at the least trifle. *Veratrum viride* 1^x.

14th.—Is greatly improved in every way; seems to be quite a different person. Continue.

28th.—Continues to be wonderfully better—in fact she seems quite well. Continue.

There was no farther report, but we regard the case as an evident confirmation of the good effects to be derived from the American *Hellebore* in cerebro-spinal diseases.

Phosphorus in Fungus Hæmatodes.

By Dr. RICHARD HUGHES.

Having had, contrary to my expectations, the opportunity of following up the case recorded under this heading in the last No. of the Journal (p. 794), I can relate its ultimate history.

The last report, dated August 31st, found the increase of the fungus consequent on the discontinuance of the *Phosphorus* at once by its resumption. Again, however, in spite of its daily use, no diminution of size appeared; and on September 24th I once more resorted to *Thuja* 30, this time taking the precaution of giving *Phosphorus* with it on alternate days.

On October 19th my patient exhibited the good effects of this proceeding. I found the fungus becoming detached at its root, and hanging only by the slenderest of pedicles. I directed her to continue the medicines, and to give the growth one daily twist upon itself until it should fall off, when she was to bring it to me. I designed to send it to some competent microscopist, that the question of its malignancy might be set at rest.

But in this I was disappointed, though the patient was not. On November 8th she returned to tell me that about a week after

her last visit a gush of blood had taken place from the breast, after which the fungus had rapidly withered away, and in a few days had disappeared. There was now nothing to be seen at its side but a small cicatrised sore.

I recommended a dose of the two medicines to be still taken daily in alternation, and the patient was to come to me should the fungus show any sign of returning. Up to the present date (December 16th) I have not seen her.

I have already mentioned the two successful cases which led me to give *Phosphorus* in this instance. A third is extracted from an old German periodical in vol. iv of the *United States Medical and Surgical Journal*, p. 517. Dr. John Komarck, of Prague, relates it thus:—

“A fungus hæmatodes was observed in the inguinal region of the right thigh of a newly born child. As this seemed to the parents to be a serious matter, it was seen by a physician of the city, as well as by a surgeon, before I was called. It was recognised by both of them as a fungus hæmatodes, and an immediate operation for its removal was recommended. On one side the tender age of the child, and on the other the large size of the fungus and its unfortunate location, presented contra-indications. I thought it best to delay the operation, and all the more as I had, as a practising homœopathic physician, seen similar pathological conditions entirely disappear after the use of specific means, and especially after the use of *Phosphorus* in the 3rd or 4th dilution, two to three pellets given each day. Here, again, I had a fortunate case; for, after the use of this remedy for fourteen days, the fungus shrivelled up, turned pale, and finally, after three weeks, left only a bluish discoloration of the skin. At the end of six months the child was seen again, and a discoloration of the skin was apparent.”

The affection here cured seems to have been a nævus rather than a true medullary growth. But it was probably one of those mentioned by Druitt, which “increase rapidly, invade every adjoining tissue, ulcerate or slough at the most prominent parts, and so may destroy life by hæmorrhage, or keep it in constant danger.” Hence the appellation “fungus hæmatodes” given it by the three medical men who saw it, and hence, I suppose, the suitability of *Phosphorus* in its treatment.

I had hoped to have had to add a case of my own in which the same medicine had cured an undoubted encephaloid cancer,

springing from the caruncula lachrymalis. It is certain that under its use—a dose of the 30th every other day—the growth diminished to less than half its size, and the eye, long closed, opened again to the light of day. But the patient, a woman of over sixty, showed coincident symptoms of break-up of constitution, and finally died hemiplegic and dropsical; so that I cannot say but what the subsidence of the tumour was a part of the general fatal change taking place in the system. Sufficient, however, has now been put on record to make us hope that in *Phosphorus* we have a really potent remedy for those hæmorrhagic and disorganizing growths which, whether truly cancerous or not, are known by the name of fungus hæmatodes.”

The action of the *Thuja*, moreover, in the first case is interesting, and is quite of a piece with what we already know of its virtues.

Ledum in Itching of the Feet. By J. DRYSDALE, M.D.

In April, 1870, a lady who had come about her children asked if anything could be done for a troublesome symptom that had annoyed her for some time, although she was otherwise in perfect health. It was a violent itching on the dorsum of both feet and the ankles, especially at night. In the repertories several medicines were given as having “itching in the feet,” but on referring to the *Mat. Med.*, *Ledum* had the symptoms exactly. Accordingly some pilules moistened with the pure tincture were given, one to be taken night and morning in a spoonful of water. The patient reported some time afterwards that after the first day of taking the medicines the itching ceased and did not recur.

Cases cured by Kali carbonicum. By Dr. H. GOULLON, jun.*

1. Mrs. S— had had a tolerable confinement, but retention of the placenta occurred. As she lived far from town eighteen hours elapsed ere I could visit her to remove the placenta. The lower

* *Allg. Hom. Zeit.*, 3rd Jan., 1870.

portion of the womb retained it like a ring. I succeeded in getting it away without much pain by cautious and gradual introduction of the hand. The hæmorrhage was insignificant. On the whole the patient had not lost much blood, but her tone was not very high. Eight days elapsed favorably, she was even able to nurse, which she had been unable to do with her two former children. Now, however, in spite of the contraction of the uterus induced by the nursing, considerable hæmorrhage set in, which had all the more effect on her as she was nursing and was already disposed to anæmia. It became a matter of some importance to stop the tendency to hæmorrhage as soon as possible. This I succeeded in doing by merely giving a drop of *Kali carb.* night and morning for four successive days. At the end of that time this hæmorrhage that depended on atony of the coats of the vessels was completely arrested.

2. Gustavus R— was said to have caught cold; after vomiting once he had headache, nervous timidity, pale unwholesome-looking countenance, and without the slightest morbid condition of his heart, complete intermissions of pulse. The boy is about eleven years old, tall and thin, and he had had a malarious affection nine months previously. *Belladonna* was of little use. The pulse was not the *Belladonna* sort. His appetite did not return, the excessive irritability remained, and the disease assumed more and more a typhoid character. The marked intermissions of the pulse, which correspond so accurately to *Kali*, led me to this medicine. I ordered eight powders of the 3rd dilution, one to be taken every three hours. This succeeded in checking the disease. The patient was again able to leave his bed with impunity.

3. Should any one ask me which is the most sovereign anti-dysmenorrhœic remedy, I must reply, *Kali carb.* Mrs. B— has lain in bed since yesterday suffering violent spasms in the abdomen. It was the time for her catamenia, but they had not appeared. She has a tendency to congestion. The domestic remedies she has tried have done no good. She is liable to too profuse menses; great confusion of head; restlessness; heat. Two drops of the 12th dilution of *Kali carb.* had in a few hours such an excellent effect on the patient, that the following morning she got up and went about her business as usual. Whilst taking the medicine (two drops in two tablespoonfuls of water, a teaspoonful every two hours) the catamenia soon made their appearance.

Yellow Fever. By Dr. CORREA.*

In the middle of December of last year an epidemic fever broke out among the population of Guataqué, which alarmed the inhabitants of that place much, for Guataqué has always enjoyed a reputation of healthiness, though the climate is the same as that of Ambalema, and, like it, it is on the banks of the Magdalena. This fever, which has as yet not extended further, is ushered in with symptoms of inflammatory bilious fever. After a day or two of general malaise the patient is suddenly seized with violent headache, the face is congested, the eyes sparkling and injected, the pulse considerably accelerated (120 to 140 in a minute) and very hard, copious perspiration breaks out all over the body, the veins are prominent, the carotids beat strongly, and there is generally considerable thirst. But this type of fever rapidly changes and it assumes the putrid character. Then occur violent vomitings of matter, at first green or yellow, then of the colour of chocolate or coffee-grounds; there is diarrhœa (rarely constipation) of the same colour, which in severe cases assumes the intense black colour and fœtor characteristic of gangrene with hæmorrhage.

At the end of January last Mr. Vidal Briceño (a young Venezuelan about twenty-two years old, of plethoric constitution, and apparently suffering from organic disease of the heart), who was by chance in Guataqué, felt indisposed. He immediately went to the estate of Paquiló, four leagues distant. There the fever declared itself, and the patient sent for Mr. J. N. Restrepo, an intelligent young man, who practises homœopathy with considerable skill and is a great enthusiast for our system. Mr. Restrepo prescribed *Acon.* 6 the first day, and by evening the fever had lost two thirds of its intensity. The following day there was no fever, but the patient complained of some gastric symptoms and paralytic pains in the back, waist, and limbs. These symptoms yielded entirely to *Bryonia* 6. The third day the patient felt very well, the appetite returned, his countenance was animated, complexion good, and spirits excellent. Mr. Restrepo left him after prescribing careful diet; but no doubt Mr. Briceño felt quite well, for in place of attending to the directions for diet,

* *La Homœopatía*, Vol. iv, , p. 269.

he thought he might neglect them ; which he did, eating great quantities of food, which, though unwholesome, was grateful to his palate. The consequence was a violent relapse, with all the signs of the worst form of *yellow fever*. On the 31st July I was summoned to Paquiló to see Mr. Briceño. The day before he felt very ill in the morning, he had rigors, headache, nausea, vomiting and high fever. Mr. J. J. Obeso gave him eight grains of *Calomel*, to purge him. The patient had several stools, but the disease had not yielded, and I found him greatly prostrated. Before I saw him I had recommended him to have *Acon.*, but as the nausea and vomiting persisted I dropped the *Acon.* and gave *Ipec.* instead. This day the fever was extreme ; pulse 150 per minute, scarcely to be counted, though strong. For night I gave *Nux v.* in order to allay the great tenesmus caused by the purgative.

I forgot to mention that, besides the indigestion, the patient had the most abject fear of yellow fever, a state of mind which is very powerful in predisposing to the disease. The prostration of strength was more marked, the expression languid and melancholy, the sclerotic dirty yellow, the face and rest of the skin likewise yellow, the skin hot and dry, rheumatic pains in the fleshy parts, teeth, and joints, great pain in the sacrum, waist, and spine. The head was throbbing, heavy, and giddy, and there was much nausea. Breathing oppressed, pains in chest, with feeling of fulness. Breath hot and foetid, the carotid and temporal arteries beat strongly ; he complains much of ringing in the ears and noise in the head. There was great derangement of the stomach as of the whole body. As the restlessness and heat were great I determined on again administering *Acon.*, followed by *Puls.* In the night I repeated *Nux v.* on account of the continuance of the tenesmus ; later I gave *Bryon.*

February 1st.—Symptoms all aggravated, skin yellower, countenance more decomposed, pulse as strong as the first day, urine as red as blood and foetid ; stools yellow, pain throughout the body, much thirst, and bitter taste. I now gave *Ars.* At 1 p.m. nausea came on. I caused the vomiting to be promoted by means of warm water, and a large quantity of green liquid stuff, like the juice of chewed herbs, was ejected. This was followed by two stools—one black, the other of the same colour as what had been vomited. The skin became moist, which gave me hopes of amend-

ment. I gave *Ipec.* The pulse grew harder, I gave *Acon.*; for night *Carb. v.*

2nd.—The fever continues in the same state. The skin is bathed in clammy sweat; the urine consists of pure blood; at each emission he loses about half a pound of blood. The patient is much prostrated, but the mental faculties continue intact. I was much concerned respecting his alarming state. During the short snatches of sleep he got he was somewhat delirious. I gave *Rhus.* During the first nights he had suffered from total sleeplessness. I administered, according to the various symptoms that appeared, *Ipec.*, *Nux v.*, *Arn.*, *Chin.*, and *Nitr. ac.*

3rd.—Fever as before; skin more yellow; the eyes, the hair, and even the beard took on the same colour. Much nausea; *Ipec.* I then gave *Dulc.* and *Chin.* alternately; but, as the hæmorrhage was great, I gave occasionally *Lach.*, *Arn.*, as well as *Puls.* and *Ipec.* Cloths dipped in cold water were applied to the abdomen, and the patient was frequently sponged with cold water, but the hæmorrhage was frightful, and alarmed us greatly. The patient was much emaciated. The fever abating a little, I caused some sago to be offered to him; but, on taking a single spoonful, he fell back fainting, and I thought he was dead. I gave *Ethiops min.* (*Merc. sulph.*), which restored him to consciousness, and gave him back some strength; this was about 1 p.m. During the night he again had sago; the fit returned, and he again had *Ethiop.* I directed water to be given him by drops, for ever since he had taken the sago he could not retain larger quantities on account of a constant hacking cough. During this second fainting fit he passed involuntarily a stool of emerald green colour, and so foetid that the nurse was at once taken ill, and all present in the room ran away out of it, as they could not stand the foetor. Thus I was left alone with the patient. I opened the doors and windows in order to get fresh air, and turned to the patient to render him the aid he required; taking the place of the nurse, who was in a precarious state, bathed in sweat, with vertigo and violent headache, severe vomiting, rigors, and trembling throughout the body, and staggering. I prescribed for her a lemonade, and that she should be rubbed with warm water whilst I attended to my patient. I prescribed for the nurse *Carb. v.*, and she was soon able to give her aid to the patient. He had convulsive movements of the extremities, and the pulse grew so small that it could scarcely be felt; all the surface of the

body became cold, and covered with cold, clammy perspiration. All this led me to fear imminent danger, and I thought his last moment was come ; but, thanks to the resources of homœopathy, it was not so. Let my colleagues remember in such cases the good effects of the immediate administration of *Ethiops m.*, *Phos. ac.*, *Carb. v.* (I have not tried *Lycop.*). They are much superior in efficacy to generous wine and the other cordials used by the allopathic school. The patient recovered from his faint, but had a very bad night. He got *Ars.* and *Chin.* throughout the night.

4th.—The fever nearly the same ; the pulse more open. I prescribed *Merc.* and *Chin.*, and for the night *Cham.* He had during the night four abundant and very foetid stools. The hæmorrhage ceased about 2 p.m., the urine assuming a dirty chocolate colour, and on its surface floated pieces of false membrane, and a pellicle of grease similar to what we observe in the water in which meat is boiled. This phenomenon was thrice repeated, and on pouring out the urine it left the vessel tinged yellow like the yolk of an egg. The urine afterwards assumed the colour of ox gall, but always left a saffron yellow deposit on the vessel.

5th.—He passed a bad night, and owing to the increased quantity he has lost he is in such a state of prostration that I lost all hope of his recovery. *Carb. v.* and *China*.

6th.—The *Carb. v.* and *China* were continued. The urine has returned to its normal state. At 9 a.m., when seated on the close stool, he had another fainting fit. Since then he has slept a little, afterwards he evacuated much green and yellow bile ; the sixth stool was mingled with blood, but was passed without pain. I gave *Lachesis*. The skin is less yellow and slightly moist ; much prostration. As the pain is still strong I prescribed *Cactus grand.* 12 for night use, but it had to be left off after the second dose on account of the supervention of diarrhœa of green fæces. *Arsen.* and *Ver.* alternately were given.

7th.—The fever and also the diarrhœa continued. The stools were always of the same colour. I prescribed *Ipec.*, and after having taken this medicine three hours I ordered *Cham.*, but as the diarrhœa did not cease I changed to *Puls.* ; the urine became rather clearer than it was before, the colour of the skin was somewhat less intense, and he looked brighter.

8th.—He took some chicken broth with relish. The fever

declined much, but the green and foetid diarrhœic stools continued. I resolved to give *Carb. v.* every three hours alternately with *Dulc.* and *Sulph.* As there was at night a return of the fever and slight diarrhœa, I gave a few doses of *Rhus.* During the night he had about twelve motions.

9th.—A very good morning. I gave now *Ferr. met.*; the pulse was small, soft, regular; the tongue clean, somewhat pale, broad and moist; the breath good, the foetor was gone; the expression cheerful and the sclerotic clear. He had not more than four evacuations during the day. I prescribed *Ferr.* and *China* alternately. During the night he had about six motions. The urine normal.

10th.—Having taken too much broth, the diarrhœa increased. I gave *Ver.* and this somewhat allayed the diarrhœa. The colour of the skin is less yellow, but the patient felt much prostrated. I prescribed *Ac. phos.*, and for the night *Carb. v.*, there being much pain and diarrhœa.

11th.—The patient remained very weak. I gave *Ars.* alternately with *China.* During the day he had six stools. He had a good night, with only four stools.

12th.—The patient wished to eat chicken and bread. I allowed him bread with bouillon or sago. All at once he complained of pain in the region of the liver. I did not hesitate to give him a dose of *Merc.*, which changed the colour of the stools, which throughout the course of the disease had hitherto been green. They now assumed the appearance of beaten-up eggs. I gave *Merc.* and *Cham.* alternately. For the night I prescribed *Puls.* He passed it well, he slept a good deal, and had five motions.

13th.—I continued with *Merc.*, and throughout the day he had only two motions; the fever was imperceptible, but what caused me anxiety was that for thirteen days the patient never knew his bowels were moved. He could not contract his anus, and he had to be constantly placed on the close stool like a child, and cloths had to be spread under him to receive his motions.

14th.—The malady now assumed an intermittent type. In the evening he had a slight rigor followed by heat all night. During the day he was tolerably well. I prescribed *Ars.* and *Bell.* for the night, *China* for the day.

15th.—He ate chicken, bread, and sago. Convalescent.

16th.—The malady was quite gone, but the patient was still very weak.

18th.—I took my leave of my patient after telling him to be careful in his diet.

Cases of Enlarged Tonsils. By GEORGE MOORE, M.D.

CASE 1. July 2nd, 1870.—A girl, æt. 8 years. Her father is a M.R.C.S., and anything but a homœopath. Both tonsils have been enlarged for years. The child is thin and pallid, and does not thrive. Her voice is thick. She often catches cold and has attacks of sore throat. Her father has given tonics, but without benefit. About a month before she was brought to me an attempt had been made to effect excision with the guillotine. During the child's struggles the operator, whilst manœuvring with the instrument, succeeded, by some unaccountable mismanagement, in inflicting a severe wound on his own left forefinger. The tonsil was slightly torn, but no part of it removed. Naturally, the child was exceedingly timid and alarmed when I began to treat her, but by dint of patience and coaxing she allowed the operation to be performed. The enlargements were reduced after five applications of the escharotic of lime and soda, mentioned in my paper on this subject in last year's *Journal*. At the time I write (Sept. 12th) she is much improved in general health. Her father has cordially expressed to me his entire satisfaction with the safety and success of the treatment, and he does not fail to adduce his daughter's case, amongst his medical friends, as an illustration of a real advance in minor surgery.

CASE 2. April 11th, 1870.—A. F—, æt. 12 years; she has had enlarged tonsils from birth, according to the statement of her mother, who is matron at one of the chief London hospitals. Many physicians and surgeons, "eminent" and not eminent, have prescribed for her. There has been no lack of variety, and no want of perseverance; tonics, *Cod-liver oil*, *Iodine* paint, *Nitrate of Silver* gargles, &c., have been used in vain. Of late, excision has been recommended as the only resource, and four surgeons have separately attempted to operate, but they each and all failed; for the patient resolutely shut her mouth as soon as she saw the instruments. On examining the throat, I found

two enormous enlargements—without exception the largest tonsils that have ever come under my notice—touching in the middle line and completely blocking up the fauces. They were hard and dense to the touch. Of course, in such a case breathing and swallowing were seriously interfered with. Her dread of being “cut” was so great that her consent to the escharotic treatment could not be obtained until its action was explained, and she had examined the innocent-looking paste applicator. It took no fewer than *twenty-six* applications; by this test judge how hard and bulky the tonsils were, to effect a satisfactory reduction. Her general health, previously very indifferent, gradually improved in the most marked manner.

CASE 3. June 1st, 1870.—J. C—, æt. 32. Three years since he had an attack of quincy for the first time; it did not end in suppuration. Ever since then he has had trouble with his throat, frequent colds, huskiness, rough voice, tickling in the larynx, frequent fits of coughing, hawking of slimy mucus, &c. During the last three months he has not been able to sing, and for three weeks past he has entirely lost the sense of smell. The left tonsil is slightly enlarged, the right considerably so. Both enlargements are soft. The mucous membrane of the throat generally is congested and relaxed. The posterior wall of the pharynx is dotted with projecting follicles, overlaid with bits of streaky mucus. Some benefit has been derived from *Merc. sol.* 2 gr. ter die, taken by my advice. But, as I have never seen rapid and permanent recovery from such a condition of the throat so long as enlarged tonsils remain, the escharotic treatment for reduction was proposed, and assented to.

On July 25th the eighth and last application was made. The enlargements were, of course, removed, the sense of smell had returned, and the entire throat, although yet not quite right, presented a much more healthy appearance. For what remained, slight relaxation, alum spray gr. x to ʒj was prescribed night and morning for three or four weeks.

CASE 4. June 8th, 1870.—C. F—, æt. 7 years, a diminutive delicate-looking boy. He has never been strong and does not grow well. During the last six months he has lost flesh and appetite. Whilst at meals he snuffles a good deal, and leaves the table before he has half finished his food, as if he had difficulty in

swallowing. He snores and snuffles in his sleep, tosses about restlessly, and occasionally awakes up frightened and screaming. His mouth gapes by night and day. He is dull of hearing on both sides. He has frequently had colds and sore throat. On being told these symptoms, I at once examined the throat and found both tonsils hard and much enlarged. The lower permanent incisors were notched; the upper incisors, which were just coming through, were notched likewise. My note-book contains the following memoranda:

“June 25th.—Fourth application; better in all respects.

“July 25th.—Tonsils much reduced; still improving; treatment stopped; six applications.”

CASE 5. June 10th, 1870.—F. G. G—, æt. 13. This patient came under treatment for the following symptoms—great susceptibility to cold; frequent attacks of sore throat; thick muffled speech; snoring in sleep; accumulation of brownish slime in the mouth; hacking cough; deafness and discharge from the left ear; poor appetite, and altogether out of sorts. Both tonsils enlarged, especially the left; left membrana tympani ruptured.

On July 20th the treatment ceased. Six applications had been made with the irresistible result of reduction. The general health had much improved.

CASE 6. July 16th, 1870,—Mrs. S—, æt. 24, one child. Has had enlarged tonsils all her life; is always catching cold and having bad throat; the voice is thick; she looks pale and out of health; catamenia regular. Both tonsils hard and enlarged, especially the left. The treatment stopped with the tenth application. The appetite, strength, and colour improved notably, with the gradual reduction of the enlargements.

Kreasote in leucorrhœa.

Mrs. H— consulted me last summer. She is 32 years old, has been married ten years, but has had no family. She attributes her sterility to frequent and copious leucorrhœal discharge, which

colours the linen yellow and makes it look as if it had been stiffly starched. This leucorrhœa is worst in the interval between the catamenia. These come on every twenty-five days, and usually last five days. The flow is scanty, and she is much less weakened by that than by the leucorrhœal discharge. A careful examination by the speculum revealed no trace of disease. All the parts are healthy except, perhaps, the vulva, which seems somewhat irritated, which may be accounted for by the constant discharge, and also by her having walked about Paris in the hot weather. It was now July. Nothing more was to be seen, not the slightest excoriation of the neck of the womb. I even succeeded in obtaining a view of the cervical canal, which was perfectly healthy, and it was wonderful to think how organs in such a condition could be the result of such abnormal discharges for so many years. But equally extraordinary was the simple and rapid manner in which this lady was cured. I merely prescribed *Kreas*. 6, a globule twice a day, in a teaspoonful of water. Ten days afterwards her husband, who was extremely disturbed by his wife's condition, hastened to inform me that since she had become homœopathic—that is, had been taking my medicine—she had become very much better. Encouraged by this success I continued the medicine. In less than a month I received a letter informing me that my patient was quite well. The cure has proved permanent, and a few days ago I received a letter confirming the previous reports, and saying that except for a day or two previous to the catamenia there is no discharge whatever. I need scarcely point out that in the present case only one medicine was employed, that it was followed by immediate benefit, and finally by a cure which has lasted three months in a patient who had been ten years ill. (Dr. Landry, *Bull. de la Soc. Méd. Hom. de France*, vol. xii, No. 3.)

Cyanuret of Mercury in Diphtheria.

Miss L. B—, æt. 4, is a fair child with a rosy complexion, blond hair, blue eyes, who has hitherto enjoyed excellent health. On the 28th May she was seized with violent febrile symptoms, nausea and slight sneezing. Summoned the same day to see the child, I found the following state: febrile symptoms intense; pulse

small, 160; considerable heat; marked hoarseness; no cough. On the left tonsil there is a very thick coating of false membrane, very adherent and very tough. This false membrane, which projects from the tonsil, covers it completely, and reaches the extremity of the uvula. There is a very small spot on the right tonsil; the submaxillary glands are neither painful nor swollen. *Cyanuret of Mercury*, 3rd trit., 20 centigrammes in 125 grammes of water, a dessertspoonful every two hours.

May 29th.—The fever has considerably abated; pulse 116; heat much less; the child has no more uneasiness; the false membrane is as before; it is beginning to turn yellow, but the submaxillary glands are visibly swollen and very painful. Continue medicine. The child takes food; soup and asparagus are given.

30th.—The general state is excellent; sleeps and eats well; she gets up and plays during the day; the false membrane appears to be thinner at its lower edge; the tonsil is much swelled and the smell from the throat is very putrid; the pain has completely left the submaxillary glands, but they are still swollen. *Cyan. of Merc.* 6 two drops. I touch the tonsils with the following lotion, *Cyan. of Merc.* 1 ten drops, distilled water 75 grammes, *Alcohol* 25 grammes.

30th, evening.—The child had during the day an attack of epistaxis from the right nostril, which was stopped by *Perchloride of Iron*. She has eaten meat twice and drank two small glasses of Malaga wine.

31st.—Same general state, it has become difficult to examine the child. However, I can observe that the false membrane does not smell so badly, and that it is thinner. The swelling of the glands has abated. But I fancy, though I am not sure, that the false membrane has extended over the uvula. *Cyan. of Merc.* 2nd trit. 20 centigr. in 125 grammes of water.

June 1st.—Better; appetite quite normal; false membrane swollen; there is no more swelling of the glands; still the pulse is high; all these days it has ranged from 112 to 120; to-day it is 130. The throat is more easily examined to-day. I find the uvula quite covered with the false membrane and also partly the right tonsil. I am unable to determine what day this happened. Continue medicine.

2nd.—Decided amelioration. The pulse has fallen to 104 and 112. The general state is excellent. The swelling of the tonsil

has nearly disappeared; the false membrane has got much less. The mucous membrane is red, and bleeding in the parts where the false membrane has come off. The throat has also become painful to-day, whereas during the previous evening it was completely free from pains. I leave off the application to the throat and prescribe a gargle, which the child was able to use, composed as follows: *Cyan. of Merc.* 1, 5 centigr., distilled water 75 grammes, *Alcohol* 28 grammes. The mixture of the *Cyanuret* 2nd trit. is continued internally.

3rd.—The false membrane gradually diminishing, redness of the throat more intense, and the pain thus increased every day. Same general state. She eats less on account of the pain in swallowing. Cont. medicine.

4th.—Uvula quite clear of exudation; it is now of a blood-red colour; there are only a few small patches of false membrane on the tonsils; the throat is very painful; the pain extends into the ear on swallowing; pulse 100. I prescribe *Belladonna* 2 alternately with the other medicine; I omit the *Alcohol* in the gargle.

5th.—The throat continues to clean, and the patient is covered with a very violent eruption of nettle rash; the pulse has got up to 120. *Apis mel.* 3 for twelve hours, then the *Cyanuret* for the other twelve hours.

6th.—The throat still shows patches of false membrane; the pain in the ears prevents the child from swallowing; pulse 116; the nettle rash not diminished; it prevents the patient sleeping. *Urtica urens* 3, three drops in 125 grammes of water, a dessert spoonful every two hours.

7th.—Diminution of the nettle rash and fever; the child begins to eat again; still some patches of exudation in the throat. Cont. medicine.

8th.—The nettle rash has returned with its previous violence; the child eats little on account of the pain in the ears. *Cyanuret of Mercury* 2nd trituration, and *Astacus fluv.* 2nd trituration alternately every two hours.

9th.—Considerable amendment, which is continued on the subsequent days. The appetite becomes very good; the false membranes disappear completely on the 11th. The *Cyan.* is left off, and the *Astacus* continued on account of the nettle rash, which lasts for five or six days longer.—(Dr. Jousset, *Bull. de la Soc. Méd. Hom. de France*, vol. xii, No. 1.)

Case of Sarcocèle. By Dr. CRIOA, of Smyrna.*

Towards the end of 1860 I had a call from Mr. S. C—, a professor of music, aged 40, of lymphatico-nervous temperament, who complained of a cutaneous affection of the scrotum, which was very itchy. Of robust appearance and sound constitution, he had always enjoyed good health, with the exception of some venereal affections, which he imagined had been cured by allopathic treatment. I did not conceal from him that his malady was probably of syphilitic origin, and I begged him to come next day and get the medicine I would prepare for him, and to subject himself to a prolonged course of treatment. I did not see him again for some days, when he told me that he had judged fit to prefer the treatment of Dr. E. S—, who by means of an ointment to be rubbed in, caused the eruption to disappear completely. The satisfied air with which he told me of this pretended cure did not prevent me regarding him as a fresh victim of ordinary physic, for who can doubt that a number of the most obstinate chronic diseases are the consequence of repercussion of exanthemata?

Convinced that his cure was but the prelude to other sufferings, I watched him from that time with an interest easily understood. I met him some months afterwards; he was very much changed, he had a sad look, and told me that it was no skin disease he had now to complain of, but that his left testicle had gradually enlarged, and though it did not actually give him pain, it caused sufficient uneasiness to make him anxious. He told me also that seeing no improvement in his malady in spite of his doctor's prescriptions, he intended to spend his vacation in Italy in order that he might consult the medical celebrities there. I contented myself with saying that he ought to change his system, and that the only way in which I believed his health could be restored, would be to cause the reappearance of the skin disease his doctor had very improperly driven off; and as he was about to visit Genoa, I advised him to consult Dr. Gatti. He returned in September, 1861, with a rich harvest of allopathic prescriptions, having totally neglected homœopathy, and with a great increase of his disease. The doctors here in consultation, agreeing with the

* *Bibliothèque Homœopathique*, July, 1870.

majority of their European brethren, could prescribe nothing but *Iodide of Potassium* internally, and also externally in baths and frictions.

Again, later, at the period of the vacation of 1862, not only had the local disease increased, but his general health was also affected. It was with difficulty he could travel to Constantinople to consult a celebrated German doctor there, who only approved of the treatment he had had, and confined himself to advising cod-liver oil. On his return to Smyrna, finding his disease continue, a new consultation was held at which the six first physicians and surgeons of the place assisted. A lively discussion arose respecting the nature of the disease,—some saying that it was *sarcocele*, while others persisted that it was *hydrocele*. In order to decide the question, a puncture was made in the testicle, when only a few drops of blood came, so the hydrocelists were beaten. Recourse was had to external remedies, and draughts containing *Iodine*. However, towards the end of 1862, after two years of suffering, many kinds of treatment, and considerable expense, the only thing the patient got from his physician was a statement of his case, of which I have the original. This statement said, among other things, that the local swelling had attained the size of a large ostrich's egg, it confessed that the disease had resisted all remedies, and that medical science, recognising its powerlessness, abandoned the case to surgery. He was recommended to go to Italy to undergo an operation equivalent to semi-castration. It was then midwinter, but, as there was no time to be lost, he dismissed his pupils, and made preparations for his journey. I met him when he was so engaged. He was much dejected, had a cadaverous look, was much changed morally, walked and sat down with difficulty, could scarcely digest anything, and it was with tears in his eyes that he gave me the above details, and confessed his despair.

Though it was somewhat rash, I said to him that he could yet be cured by homœopathy. I prevailed on him not to run the risk of a journey and a dangerous operation. He placed himself entirely in my hands. I made a careful examination of him, and was able to note the size of the testicle, which had latterly become very hard, excessively sensitive, and there were pains extending up the spermatic chord.

Always persuaded that this kind of hypertrophy was caused by the repercussion of the exanthema of syphilitic character, and

guided also by the totality of the symptoms, I was about to prescribe *Merc. sol.*, when the idea came into my head to resort to *Merc. biniod.*, which I had seen strongly recommended by Dr. Schweikert in similar cases of secondary syphilis. I therefore prescribed the 3rd dilution of this remedy, two drops a day, in two ounces of water, a dose night and morning for a month (January, 1863). The month passed without any perceptible change in the local malady; but he looked better, had good appetite, bowels regular, good sleep, and better spirits. In the beginning of February the eruption reappeared on the genitals; I then prescribed the *Biniodate* in the 2nd dilution, and this he took till the beginning of March. During this period a scaly, foetid eruption spread over the scrotum and perinæum as far as the anus, exuding so much as to necessitate a continual change of dressing; on the other hand, the size of the testicle visibly diminished, and at the end of two months and a half of treatment it had recovered its natural dimensions.

The 1st dilution of the same remedy taken at longer intervals produced diarrhœic stools, during which the eruption dried and disappeared, and the cure was complete.

MISCELLANEOUS.

Hall, in Upper Austria, as a Sanitary Resort.

(Extract from a letter to Dr. Drysdale.)

July 6th, 1870.

YOU asked me to give you some account of Hall this year, as well as any little information I could gather for the benefit of others of my countrypeople who may be sent here for the sake of health. I am glad to tell you my favorable impressions of Hall last year are fully confirmed this, and I cannot but think of the efficacy of the waters and the salubrity of the climate were more generally known in England, many would gladly avail themselves of both, in spite of the distance. We left London, May 24th, by the Antwerp Steamer, having taken our fares through direct to Cologne. From Bonn we took the Rhine to Mayence, whence the direct route to Passau is by Darmstadt, Wurzburg, Nuremberg, and Ratisbon. At Passau it is well for those proceeding to Austria, and especially to Hall, to procure Austrian money, which can be had at the money changers'. Passau just ranks with Linz as one of the most beautiful spots on the Danube, which from here is a more important river, being joined by the Inn and the Ilz. We spent a day at Passau, and the next morning, June 1st, took the train to Wels, where we found several carriages at the station. We paid six florins for one to bring us to Hall, but the charge is usually eight in the height of the season. The distance is about sixteen English miles, but we took nearly four hours, as the road is not very good. The best approach to Hall is from the Wels Road, and before you reach it you see the little church and some of the houses nestling, as it were, among trees on the side of the hill. We at once drove to the "Schloss," having been comfortable there last year. The landlady asked us seventy florins a month for two good rooms, which will probably be eighty-five in the height of the season. There are 134 houses in Hall, in all of which lodgings

may be had, and good coffee, milk, or cream, or they will provide boiling water for those who like to take their own tea. The custom is to dine *à la carte* in one of the hotels, but neither the meal nor manner of dressing it is good, but the bread is always so. One of our first walks was to the "Park," which name is given to the grounds, which are nicely laid out in front of the Kurhaus. The park is well supplied with seats and shady walks for hot weather, and a band plays here for two hours morning and evening. An artificial mound was erected last year, from which you get a good view of the mountains and the richly-wooded country across the little valley of the Sulzbach. I hear the mountains are called the Steyerische Gebirge, yet one of the more distant ones appears to have snow on its summit. To the right rises the lonely Traunstein, 5342 feet high, the summit of which is said to resemble the profile of Louis XVI. Immediately in front, on the other side of the valley, is Pfarrkirchen; this is but a small village, but some of the peasants who live there must be well to do, as their church is so very nice and well furnished with pictures. You are told also the baronial château of Feyregg belongs to Pfarrkirchen. Feyregg is a pretty object from wherever you see it. It has within the last few years been bought by a banker of Linz, whose son is now come into possession, and as he does not like the loneliness of a great house, he has let part of it to two families from Vienna, who have taken these apartments for the whole summer. Hall is situated 1189 feet above the level of the sea, and the air is considered so pure and good that many families come here in summer for the air only, without any reference to the waters. The air is always fresh in the mornings and evenings, sometimes even cold, so that all who come here need warm wraps occasionally as well as strong boots. On the other side of the park, towards the town, is the Kurhaus, which is a moderate sized building, with a colonnade in front and a wing on each side. In these wings are the baths, between fifty and sixty in number. In the centre is the Kursaal, reading, billiard and smoking rooms. Tea, coffee, chocolate, and ices may be had at the Kurhaus at moderate prices. There is a "soirée dansante" in the Kursaal every Saturday evening from 8 till 10, and the game of "Tombola" once a week, which, however, is held in the open air, in front of the Kurhaus, when the weather permits. This Tombola is like the children's game of Lotto, and it is amusing to see people of all ages taking part

in it, and the Herr Director of Kurhaus, who has the management, calling out the numbers. I am told, however, that it is also a means of income to him, as the prizes are of little value, and the money paid for the tickets about five times as much. The iodine spring is in the valley, to which a pleasant path leads from the park, and the Quellenhaus was very convenient there, and surrounded with shady walks, but quite lately the water has been conveyed by pipes to the Kurhaus, and this is found more convenient for infirm visitors, of which there are many. In the vicinity of the old Wellhouse is also the Kaltbad, which appears large enough to be a swimming bath. It is open to ladies in the afternoon, and to gentlemen mornings and evenings. The doctors are about six in number, and are, I believe, all from Vienna. The cure consists principally in bathing, and from thirty to sixty baths are taken in one course; but drinking the iodine water and compresses of it are also used. Not a bath or glass of iodine water can be had without a doctor's prescription. The visitors for the "cur" are of all ages, but there is a great proportion of children, some with bound up throats and faces, others wheeled about, who have such a suffering and enduring look as excites one's very pity.

There are two good hotels in Hall, the "Kaiserin Elizabeth" and the "Erzherzog Carl," the former being the most frequented, the latter in a quieter position, at the Wels end. There are a great variety of pleasant walks, and in every direction farm houses, where good coffee and milk may be procured. Many patients are ordered milk, and so this, or "Obers," which I think is merely good milk with the cream left, is in great demand. There are also many pleasant drives, and a "Fahr-Tarif" may be procured, which regulates the prices for different excursions. A summer theatre has been erected this year, in which are given daily representations at 4 p.m., which last till six. The nearest Protestant church is an hour's drive from Hall in the Linz direction, and as the number of English visitors is so small, no English service can be yet thought of. It would be hardly right to close this description of Hall without at least alluding to its vicinity to Gmünden, Ischl, and Salzburg, which all may be reached in a few hours, and are places of interest and beauty, and those who like to vary the homeward route may do so by going to Salzburg, and thence taking the express train to Munich and

Stuttgard. Visitors to Salzburg ought not to omit seeing the Königsee and Berchtesgaden.

Since writing the above I have spoken to a money changer, who says he will take English gold or paper, according to the current rate, which is telegraphed from Vienna daily and posted up in the Kurhaus. The weather is now bright and hot in the sun, and this reminds me to say the climate of Hall is usually more settled in May, August, and September, than in June and July. The season begins May 15th and ends September 15th; but the doctors arrive May 1st and some few patients. The Kurhaus was built in 1853, and it is only since that year that Hall has been an established watering place. There are two hospitals, one for children and one for military students.

I hope these details may prove of use to some who may hereafter visit Hall.

Involuntary Proving of Thuja. By Dr. DUDGEON.

On the 10th of July last, when taking a walk, I happened to pass a thuja tree laden with green cones. I plucked one, chewed it a little, and thought no more about it. That same evening I observed a very disagreeable scalding on making water, which continued all next day, and I was horrified to observe on undressing that my shirt was spotted all over in a manner extremely repugnant to one's notions of respectability. I found a considerable gleety discharge from the urethra which was evidently swollen and inflamed, as the stream of urine was small and split and the burning had increased. I had quite forgotten the circumstance of having chewed the thuja cone the previous day, and I could not imagine what could have produced in me, a decent paterfamilias, such a very incongruous complaint. The following day the discharge had become yellow, while the other symptoms remained as before. I now remembered the cone-chewing on the 10th, and regarded the malady with more composure. I resolved to take no medicine to interfere with its course. The discharge still continued, though in a diminishing degree, until the 15th, but the scalding and interrupted stream of urine were by that time gone, and on the 16th I was again quite well. The symptoms while they lasted

were precisely those of an ordinary attack of gonorrhœa, but their medicinal origin was evidenced by the short duration of the attack. In the provings of *Thuja* by Hahnemann and Meyerhofer there are certainly indications of a decided action in the urethra, but not such a complete picture of gonorrhœa as that presented in my case. The medicinal action of *Thuja* in some of the secondary effects of gonorrhœa is well known, but it has not been much employed in the earlier stages of the blennorrhœa itself for which this involuntary proving would show it to be eminently adapted.

I should add that two colleagues who, at my suggestion, chewed a cone of thuja as I had done, were unaffected by it.

Jenichen redivivus.

Apparently we are never to hear the last of Jenichen and his unhappy invention which has proved an apple of discord in the homœopathic camp from its promulgation until now. The bad example set by this muscular horsebreaker has been, like all bad examples, much followed, and we have had others vaunting their own high potencies as an improvement on the original. Thus we had Rummel's high potencies, which he asserted to be better than Jenichen's, and we have Lehrmann's high potencies, and Carrol Dunham's high potencies, the exact preparation of which he honestly gives; and lastly we have Fincke's high and highest potencies for which he took out a patent, whereby it appears that his 10,000th, 20,000th, and 50,000th potencies are prepared by letting 10,000, 20,000, and 50,000 drachms of water flow through a drachm bottle in which a drop of the medicine was originally placed.

In a recent number of the *Allg. Hom. Zeitung* we have a little from Dr. Fielitz, of Brunswick, about Jenichen. It runs as follows:—

“In 1845 I had a frequent interchange of letters with the late Jenichen. Judging from this correspondence I must pronounce Jenichen to have been an honest, truthful man, very zealous for homœopathy. He had a profound thorough knowledge of the *Materia Medica*. He occasionally communicated to me little

histories of cures always with one medicine only, which were very striking. When Rummel expressed in the *Allg. Hom. Zeitung*, his disapproval of the secrecy Jenichen maintained respecting his process, he wrote to me in a state of extreme irritation. He appeared to think that the thing was not yet ripe for publication, and he said, "Rummel has no conception of the damage he has done to the affair."

"It was only after Jenichen's death that I could rightly make out the meaning of this expression. Jenichen was a man *propositi tenax*, and it may be taken for granted that he carried his secret with him to the grave. He was such an enthusiast for his invention that he wrote about the high potencies in this strain: "Their employment is the only way that leads to the true goal. He who does not pursue this path sinks." On another occasion he wrote respecting a remedy he had advised for the suffering of an acquaintance: "It will give me much pleasure, but me only, for I cannot take any one into my confidence on this subject, if your painful disease is removed. But should this result not ensue, you must be satisfied with the good will of a weak mortal." Surely a charlatan would not have written in this style.

"Finally, Jenichen said this about the highest potencies: Most of them have required 3, 5, 8, and many 10 times as much labour as the high potencies, 200. They must have had above 3,000,000 vigorous strokes with the arm to bring them to their present state.

"I imagine that this last passage may be of some interest with respect to the recent revival of this subject. That any one is in possession of the secret of the preparation of Jenichen's high and highest potencies, is scarcely credible, for who could have so long kept it a secret from science?"

On Belladonna in Nocturnal Incontinence of Urine. By J. BURNET YEO, M.B., Assistant-Physician to King's College Hospital.

There are one or two facts in therapeutics which are extremely encouraging to those who believe, as I do, that medicines generally have a very important influence in modifying or arresting

disordered conditions of the bodily organs. They serve also as an antidote to that widely-spread scepticism with regard to the value of medicinal agents in combating disease, which is as irrational as was the excessive credulousness which formerly existed on this point, and to which our modern incredulity probably owes its origin.

Of such facts, the action of *Belladonna* in arresting nocturnal incontinence of urine is one of the most remarkable, especially when its operation can be witnessed in such cases as the following.

The conditions which give rise to this distressing symptom are most commonly observed in young children ; but the cases which form the subjects of these remarks were persons who had reached the age of puberty, and this circumstance has enabled us to analyse the mode of action of the remedy in a manner which would have been difficult with younger patients.

The first case was that of a young man, sixteen years of age, an apprentice as a compositor, who became an out-patient at the hospital about two months ago, complaining of nocturnal incontinence of urine. He was a thin, delicate-looking boy, with a hectic flush on his face ; and he manifested in a marked degree that *shamefacedness* and mental distress which so embarrassing an infirmity may well produce. The accident happened every night, and had persisted ever since his infancy. He was ordered to take five minims of the tincture of *Belladonna*, with ten minims of the tincture of the *Perchloride of Iron*, three times a day, and to avoid drinking any fluid for some hours before bedtime. After taking this prescription for a week, he reported himself as only slightly better ; he had passed urine in bed three or four times since he had taken the medicine. He was ordered to take ten minims of the *Belladonna* tincture instead of five. This reduced the frequency of the accident to first once in seven days : then once in about fourteen days ; and, on increasing the dose to fifteen minims, once in three weeks. With the latter dose he took no *Steel*, as his general condition had improved marvellously. There had been, so far, little or no alteration in the dimensions of his pupils, and vision had in no wise been interfered with ; but on the recurrence of a slightly increased frequency of the incontinence, he was ordered twenty minims of the tincture of *Belladonna* three times a day. The nocturnal incontinence now completely ceased, but the vision became so indistinct that he could not "read his copy."

He was therefore ordered to take only one dose of twenty minims in the twenty-four hours, and that at bedtime. He continues quite free from a return of the complaint. This patient had never suffered from any irritability of the bladder by day, and had no occasion to pass water more frequently than other people.

The next case was that of a girl, also between sixteen and seventeen years of age, who had similarly suffered ever since her infancy from nocturnal incontinence of urine. She came to King's College Hospital as an out-patient on March 5th last, and at that time she was nightly passing her urine in bed. She was living in the country, and was a fresh-looking, healthy, and robust young woman; but her mental distress and embarrassment were extreme. This wretched malady unfitted her for going into service, might be justly regarded as an impediment to marriage, and altogether made her life a misery. She was put on the same kind of treatment as the preceding case, except that she at once commenced taking ten minims of the tincture of *Belladonna* thrice daily, and she was ordered no *Steel*, as her general condition was good. For the first week little or no improvement was manifested; the dose of *Belladonna* was therefore increased to fifteen minims, which dose she has continued to take ever since. The inconvenience rapidly abated, and now scarcely ever occurs. Her appearance and manner have at the same time undergone an entire change; a bright and cheerful aspect has replaced a taciturn, morose, and discontented mien. She has, for nearly a month, been engaged in a situation as a domestic servant, for which she voluntarily applied. Here, then, is a remarkable instance where the whole course and prospects of a life have been rapidly changed by the administration of a few minims of a vegetable tincture!

Let us now inquire, What is the pathological condition which in these cases give rise to this symptom; and in what manner has the *Belladonna* acted in overcoming it? I apprehend that a weakness or atony of the sphincter muscle of the bladder was at the root of the evil in each case. There was no irritability of the bladder leading to frequent micturition by day in either case, nor was there any abnormal condition of the urinary excretion.

We know that the involuntary action of the sphincters can be supplemented and strengthened, when necessary, by voluntary effort. It is therefore probable that, during the waking hours, a

very slight amount of voluntary effort is superadded to the involuntary contraction of the sphincter vesicæ, and the urine is thus prevented from escaping from the bladder. But during sleep this voluntary effort is in abeyance, and so soon as the accumulation of urine becomes sufficient to stimulate the detrusor fibres to contraction, the weak sphincter gives way, and the bladder is evacuated in bed. I imagine that the *Belladonna*, acting it may be through the sympathetic nerve fibres, strengthens the involuntary efforts of the sphincter fibres at the neck of the bladder, and thus prevents the nocturnal incontinence. In each of the cases I have alluded to, there was the additional difficulty in treating them, as I remarked at the time, that we had to overcome an inveterate habit. The bladder had acquired during fifteen or sixteen years the habit of discharging its contents during the night. Now in the girl's case we had a striking example alike of the influence of habit and of the efficacy of the remedy. The habit prompted the bladder to empty itself during the sleep; the remedy, producing firm contraction of the sphincter, resisted this effort; the sum of the two opposing influences was sufficient to wake the patient out of her sleep every night, at first two or three times, now only once, when she has to yield to the force of habit, and gets up to pass water. This circumstance points, I think, clearly to the fact that the action of the *Belladonna* influences, directly or indirectly, the contractile force of the sphincter of the bladder. Its efficacy cannot be due to its diminishing any irritability of the bladder, for none exists; during the day-time the bladder distends to quite as great an extent as is common. It cannot be due to its checking the secretion of urine, because quite as much urine is secreted now as formerly. Neither can it be due to its altering the character of the secretion, which has never been other than natural. We conclude, then, that the efficacy of *Belladonna* in relieving nocturnal incontinence of urine is owing to its influence in giving tone to the weakened sphincter vesicæ. This observation is in harmony with the statements which have recently been made as to the action of *Belladonna* as an aperient by promoting the peristaltic contractions of the involuntary muscular fibres of the intestinal canal. But the most valuable comment that I can make on these two cases is, "Let them teach us not to despise the influence of medicines or the study of therapeutics." So long as we know of a few medicines which possess such remarkable curative powers, we

are encouraged to believe that by searching we shall discover more.—*Lancet*.

On a purely Milk Diet in the Treatment of Bright's Disease. By
ARTHUR SCOTT DONKIN, M.D., Lecturer on Forensic Medicine
to the University of Durham.

In my previous contribution I detailed two cases of Bright's disease treated by a skim-milk diet. In one the treatment was perfectly successful; in the other—a much more serious and advanced case—only partially so. I shall now describe a third, one of a remarkable and interesting class of cases in which the renal affection is associated with and apparently produced by lead-poisoning, and to which attention has been specially directed in recent years by several pathologists, especially by the late Dr. Todd, Dr. Garrod, M. Ollivier, M. Charcot, Dr. W. Begbie, Dr. Grainger Stewart, and others.

Case of lead-poisoning and Bright's disease, with anasarca and epileptic coma; recovery.—J. S—, æt. 46, a plumber, who had been much engaged in casting lead during the previous seven years; in this occupation he frequently suffered much from the fumes arising from the melted lead, which always affected his gums and loosened his teeth. The symptoms of lead-poisoning commenced about six years ago, and first showed themselves in pains in the legs and head, commencing in the occiput and extending over the cranium; the pain in the head had been continuous and severe, accompanied with great sleeplessness. During the whole period he suffered much from colicky pains in the abdomen and obstinate constipation, also from discharges of blood from the bowels, which continued more or less for several weeks, and then subsided for a period, to return again. With these symptoms there was great loss of flesh and strength, his weight having fallen from fourteen to nine stone; he suffered much from feebleness in the forearms and wrists, with emaciation of the extensor muscles. About four months before I examined him he became gradually affected with loss of sensation on the left side of the body, face, and extremities, and shortly afterwards with attacks of convulsions

accompanied with loss of consciousness, recurring at first every other day, and then not oftener than twice a week. He had been under medical treatment on several occasions during his illness.

In March, 1869, this patient consulted me at the Sunderland Dispensary. I found him suffering from extensive anæsthesia of the left side and extremities, severe pain in the head, and sleeplessness. The epileptiform seizures had continued up to this time. There was loss of appetite and great feebleness. A characteristic blue line on the gums was well marked. He also complained much of constipation. There was slight œdema about the ankles. The urine was scanty, and highly albuminous. With the view of eliminating the lead from the system, the following mixture was prescribed: *Iodide of Potassium*, three drachms; *Tincture of Iodine*, three drachms; *Tincture of Cinchona*, an ounce and a half; *Infusion of Calumba*, ten ounces and a half: two tablespoonfuls to be taken three times a day. An ounce of *Sulphate of Magnesia* was also ordered to be taken in the morning occasionally. The diet to be of the ordinary kind, but nutritious. Under this treatment he improved somewhat as to the symptoms referable to the nervous system; but the œdema of the feet and legs gradually increased until the 14th of May, when he presented himself, suffering from general dropsy. There was great œdema of the lower extremities and scrotum; so that he was almost unable to walk. There was also much puffiness of the hands, forearms, and face; the urine was scanty, high coloured, very albuminous, sp. gr. 1015, and deposited great quantities of granular and hyaline casts of the uriniferous tubes. The epileptiform seizures were very frequent and severe, and the anæsthesia of the left side had become more complete.

A thorough change was now made in the treatment. The patient was placed on an exclusively skim-milk diet; six pints, warmed, were allowed daily, and *Acetate of Potash* in doses of twenty grains in water, thrice daily, was substituted for the *Iodide-of-Potassium* mixture. He began to pass at once about six pints of urine daily, and the anasarca gradually subsided. This treatment was continued for six weeks, and by the end of this period the fits became much less frequent, and the cephalalgia much less severe. The patient was now allowed, *in addition* to the milk diet, bread in the morning and butcher's meat to dinner; and in the course of three weeks afterwards he resumed his work,

but not amongst lead. Sensation gradually returned to the left side, and the albumen disappeared altogether from the urine. His health and strength gradually improved up to the beginning of October, when the epileptiform seizures ceased entirely, as well as the cephalalgia, and sensation was restored to the left side; the anasarca had never returned, and he had gained 1 st. 3 lb. in weight.

I last saw this patient on the 17th of December, when he continued quite well. He had, up to that date, continued to take large quantities of skim milk daily, with butcher's meat to dinner, and bread thrice daily.

In this case it would be difficult to decide whether the epileptiform seizures and paralysis of sensation on the left side were the result of the *cervic* action of lead as a poison on the nervous system, or of uræmia, or of both. It seems probable that they were due to lead, from their appearance before the renal affection was well developed, and their persistence after it has been relieved. But from the coexistence of the two disorders they may have possessed a mixed character. Pure *renal epileptic coma* is a most fatal and formidable affection, generally destroying life after a few seizures.

From the history and clinical features of the three cases of Bright's disease of which I have given a description, it is evident that they were illustrations of that class of renal affections to which Virchow has given the name of *parenchymatous nephritis*, in its chronic condition—cases in which the disease is seated chiefly in the glandular epithelium of the uriniferous tubes; and they were apparently, too, in the second or fatty transformation stage of the epithelium. In the case of W. M—, who only partially recovered, the second stage was far advanced, or probably the third stage—that of contraction—had begun. This may reasonably be inferred from the predominance of hyaline over the fatty casts in the urinary deposits, and from the less controllable character of the disease than in the other cases.

So far my experience in the skim-milk treatment of Bright's disease has been confined solely to the class of cases to which I have just referred. I have not had an opportunity of testing its efficacy in the waxy form of the malady: generally associated with, and dependent on, some cachectic condition—such as phthisis, caries of the bones, syphilis, and the like. Neither have I tried the remedy in the gouty or cirrhotic kidney.

In order to explain the therapeutic action of milk in chronic parenchymatous nephritis, it is necessary to comprehend fully the character of certain pathological conditions pertaining to the disease. In order fully to appreciate the morbid alterations which the kidneys undergo, and effect of these alterations on the blood, it is necessary to remember that the kidneys are provided with a *double* capillary system—namely, a *primary* set of capillaries forming the Malpighian tufts, and a *secondary* set formed by the ramification of the efferent vessels of the Malpighian tufts into a network of fine vessels distributed between and around the convoluted uriniferous tubules. In the second place the kidneys are completely invested, each by a firm fibrous coat, or capsule, of a very unyielding nature.

It follows from these facts that, in the disease under consideration, as the convoluted cortical uriniferous tubules become swollen and distended by their tumid, morbid, epithelial contents—glued together into solid casts by a fibrinous effusion,—and as the capsular investments of the organs yield but slightly and slowly to the distending pressure from within, generated by the swollen tubules, the *secondary* capillaries, being *external* to them, are mechanically compressed to a greater or less degree, and the circulation through them more or less impeded,—the blood is dammed back in the *primary* capillaries of the Malpighian tufts, which become congested and distended, the result being a large and persistent escape of albumen through their walls from the blood. This seems to me to be the correct interpretation of the phenomenon of *permanent* albuminuria in this form of Bright's disease after the early inflammatory period has passed away, and of the *pale* exsanguine appearance presented by the cortical substance of the kidneys. The pressure exerted by the confined swollen tubules must be very considerable, as shown by the expansion which the dense inelastic capsule undergoes in the enlarged kidney. The *primary* capillaries, or Malpighian tufts are better protected from the influence of this pressure than the *secondary*, by their position *within* the dilated flask-like termination of the tubules, by their shortness and compact distribution, and by their *proximal* position to the arterial system and the propelling force of the heart.

The effect of the continued drain of albumen from the kidney is to impoverish the blood to such a degree that its albumen is reduced in some instances to as low as 16 parts in 1000, the

healthy proportion being from 60 to 70 parts in 1000. By this serious deprivation of albumen, the specific gravity of the blood-serum is lowered from 1028, its average in health, to 1013 in some instances. This hydræmia, or watery state of the blood, in its turn rapidly destroys the red corpuscles, and produces general dropsy, aided, no doubt, by the fulness of the vascular system from the diminished withdrawal of water by the kidneys.

Lastly, the diseased epithelium of the uriniferous tubules only partially secretes the solid constituents of the urine, and the blood becomes poisoned by its own excrement, and epileptic coma may supervene.—*Lancet*.

On the Use of Arsenic in certain Painful Affections of the Stomach and Bowels. By ARTHUR LEARED, M.D., M.R.I.A., Senior Physician to the Great Northern Hospital.

As the scope of these remarks is to be exclusively practical, I do not stop to consider the nerve distribution of the gastrointestinal tube in relation to pain. This has been carefully done by Dr. Eulenburg in his papers on visceral neuralgia now being published in this journal.

Pain after food is a very common symptom of dyspepsia, and in many cases seems to constitute the disease. This pain usually yields to medical treatment and proper diet. But there is another kind of gastric pain far more severe than that which depends on food, and which does not yield to ordinary remedies. I have elsewhere pointed out how this pain may be removed, and the subject seems of sufficient importance for some further remarks.*

In case of the stomach, the pain we have to deal with happens to the same individual at one time when it is full, at another time when it is empty. But cases are met with in which the presence of food in the stomach is clearly the exciting cause. The typical case is that in which there is pain independent of the

* "The Successful Use of Arsenic in certain kinds of Gastric Pain." (Read at the meeting of the British Medical Association at Dublin, 1867.) *British Medical Journal*, November 23 and 30, 1867.

act of digestion. In this form it commonly seizes the patient in the middle of the night, and is not preceded or attended by any dyspeptic symptoms. The pain in these instances, which are fortunately not very common, is extremely severe, and attended with alarming prostration, lowering of the heart's action, pallor, and cold perspiration. Brandy and other stimulants give but little relief, and after a period of agony sometimes extending to several hours, the attack ceases as suddenly as it had commenced.

Persons of middle age who have been exposed to some great cause of mental depression are peculiarly the subjects of this affection of the empty stomach. Dr. Budd has also noticed mental disturbance as an exciting cause of the disorder, and he adds that "it is closely allied to waterbrash." In this statement I do not concur, for without entering into the vexed question of the nature of water-brash, it is sufficient to say that a particular remedy which cures the one proves injurious to the other.

Further experience has taught me that the bowels, and especially the small intestines, are subject to the same kind of pain. I do not include colic from the effects of lead; but many cases of so-called colic unaccountably occurring at longer or shorter intervals are from the same cause. For whether the pain attacks the stomach or intestines, its nature is the same; it is essentially neuralgic. Upon this circumstance the success of the new treatment, which consists in the judicious exhibition of arsenic, depends. As may be inferred from what has been stated, the difficulty of diagnosis between the neuralgic and the more common forms of gastro-intestinal pain is sometimes very great. The best rule of practice is, when gastric or intestinal pain resists all ordinary treatment, and cannot be traced to gall-stones or any organic source, to test the matter by the effect or non-effect upon it of the remedy. By this method I have succeeded in effecting several cures. On the other hand, the arsenical treatment has failed in two cases in which, so far as the diagnosis could be established, it ought to have succeeded. In both instances the patients were women past middle age, of stout habit, and too freely addicted to the use of alcoholic stimulants.

The curative effects of arsenic are most striking in severe cases of paroxysmal pain, and its success becomes doubtful in proportion as the case assimilates to those in which a lower degree of

pain is traceable to the influence of food. In determining the question of the fitness of a case for the arsenical treatment certain circumstances may render essential aid. If the disease came on after some mental shock or severe trial, if the patient has previously unmistakeably suffered from neuralgia, if he has lived in a marshy district, and especially if he has had hemicrania or ague, and if in addition to the occurrence of one or more of these circumstances the pain is paroxysmal, it will almost certainly yield to *Arsenic*. But as already said there are other cases suitable for the treatment, and they are the most numerous, in which the pain closely resembles that which attends dyspepsia. It is sometimes extremely difficult to make the diagnosis between neuralgic pain of the stomach or bowels and the pain caused by gall-stones. But in my previous paper I have gone into details respecting this and other sources of error.

A few words will suffice as to the particular preparation of arsenic to be selected, and the extent to which it should be used. In most cases the liquor arsenicalis answers every purpose, but when the symptom is more than usually susceptible of the action of the mineral the liquor sodæ arseniatis seems to irritate less, and in a few instances the acid solution of *Arsenic* is to be preferred to either. Whatever preparation be selected it should always be taken immediately after food, and notwithstanding that its beneficial action may have been previously observed it will be proper to continue the medicine until its constitutional effects are well marked. Notwithstanding what has been said to the contrary I do not believe that the proper use of *Arsenic* as a medicine is followed by any injury of the system.

The following brief notes illustrate the utility of the treatment in cases in which the pain is increased by food, and also its effect in cases in which the intestines are affected:—A lady, 40 years of age, who had met a reverse of fortune by the death of her husband two years previously, was sent to me by her medical attendant in January, 1869. I was informed by him that every ordinary means, including a milk and farinaceous diet, with entire abstinence from meat for eight months, had been exhausted in his attempts to relieve her sufferings. These consisted in constant pain in the gastric region, extending round the left side to the centre of the back. The pain was much aggravated by meals, especially by breakfast and tea, and at times it amounted to perfect agony. Vomiting frequently ensued, and then some

relief of the pain was obtained. There was great flatulence, a sense of oppression in the stomach, and obstinate constipation. The patient, naturally of stout habit, had lost over 50 lbs. in weight. She was immediately put on the arsenical treatment, which was speedily followed by great improvement. This treatment was continued, and the dose gradually increased until the constitutional symptoms, which consisted in this instance of itching of the eyes, soreness of the soles of feet with a red rash upon their sides, ensued. By this time her cure may be said to have been complete; she rapidly gained flesh and strength, and has since remained well.

The symptoms in this case resembled those of ulcer of the stomach, but the fact that a rigid milk and farinaceous diet increased rather than diminished them was opposed to this view.

A gentleman, 28 years of age, much engaged in commercial speculations, consulted me in the early part of the present year. He had been for a long period subject to a violent but dull pain in the umbilical region, coming on about two hours after meals. For the three previous weeks it had happened daily after breakfast, luncheon, and dinner. Liquids, even plain water, induced it more than solids. There was neither flatulence nor any other stomach disturbance, and the bowels were quite regular. Various plans of treatment had been found ineffectual. He had suffered from neuralgia in the left temple two years previously. Notable relief was afforded by the liquor arsenicalis after it had been taken only two days; it was continued altogether for about three weeks, when his eyes became affected. At this time the disease had quite subsided.—*Med. Times and Gazette.*

On the Propriety of Bandaging after Parturition, or when Ribs are Fractured.

Dr. Ryer remarks that the abdominal parietes towards the latter part of gestation are so distended in many instances as to be incapable of returning to their pristine condition. The abdominal viscera by the act of delivery lose the support to which they have become accustomed; and hence follow exhaustion, faintings, and even mania. The necessity of support and pressure

to remedy this state of things has long been recognised, and very generally practised. But every cloth bandage that a recently-delivered woman can support will work upwards from the hips to the waist, since the latter is smaller, and the bandage at once becomes ineffectual, if not injurious. But another objection to bandaging is that it tends to depress the uterus; for if the finger be introduced into the vagina at the moment when strong traction is made upon the bandage enveloping the abdomen, the uterus may be felt to descend to a considerable extent; hence, a bandage too long worn will encourage prolapse. A case of pendulous belly was treated with such success by Dr. Ryer with adhesive strips instead of a bandage that he has since frequently adopted the plan. The method of bandaging Dr. Ryer has practised is to take about three fourths of a yard of the adhesive plaster and cut it in strips parallel with the warp, so that each strip is one and a half inch wide, and long enough to extend from three or four inches from the spine on one side, over the abdomen, to within three or four inches of the spine on the other side. Having cut the entire piece into strips (say ten) and warmed them, he commences at the lower part by drawing the heated plaster from the back of the hips—when the assistant passes the end against the skin—tightly over and above the pubis, and around to the corresponding place on the opposite hip. Thus is applied each strip above the other, until we arrive at the umbilicus, in some cases one inch above; but too high bandaging is subject to some of the objections attached to the ordinary system. As adhesive plasters slip and stretch, they should be applied tightly drawn, gathering the looseness and folds of integuments under the plaster. These folds disappear within a few hours after bandaging. Having thus applied successive strips from below upwards to the umbilicus, we commence again below, and over the first we apply a second layer of adhesive strips. The object of this second layer of strips is to give body and firmness to the first, for the abdomen in its lower part requires all the support the most ingenious can give. Thus, the inferior portion of the abdomen becomes encased by adhesive plaster, whilst the epigastric region is free, and the relief is often so great when properly applied that patients who have lain upon the bed one or two weeks bandaged with cloths will, upon the application of this casing of plaster, ask permission to sit up or to leave their beds. But there is one drawback to the use of the

adhesive bandage, and that is, with some an itching occurs about the fourth day. This itching passes away by removing the bandage and washing the parts with warm water, and immediately renewing the bandages by fresh adhesive strips as at first. Similar to the usual mode of bandaging after labour is the bandaging by rollers and towels when the ribs are fractured. This cumbersome and unphilosophical wrapping around of the chest, by its heat and pressure, is more likely to produce internal disease than hasten the union of the fractured ribs. Disengaged, loosened, and displaced as soon as applied, they are ineffectual, except for evil. Without pressure upon the lungs the ribs can be held perfectly still by the judicious application of adhesive plaster, prepared as above mentioned, in strips of one and a half inch in width. Commencing at the sternum, apply the strips obliquely downwards and backwards on the side affected, to the lumbar region; having applied several of such strips, a second layer over the first may be used for the purpose of giving firmness. For the fracture of ribs upon one side it is not necessary to embarrass the opposite side with bandages, as is so often practised. From personal experience following the fracture of a rib, Dr. Ryer has satisfied himself that the ribs may be held quite as much at rest by the adhesive plaster drawn from the sternum to the lumbar region on the side affected, with the side not affected free from restraint, as they can be by the tightest rollers.—*California Medical Gazette*, Nov., 1869; quoted by *The Practitioner*, January, 1870

Chloralum.

Chloralum is the name of a disinfectant which has just been introduced to the public, and so unique is the compound in itself, and so remarkable are its properties, that we cannot fail to recognise in it one of the greatest scientific discoveries of the day. As such it is well worthy of a notice at our hands, the more so, perhaps, in that the discovery is due to a gentleman whose name is well known as having made great strides in the art of arresting decay and in preserving animal substances. We allude to Professor Gamgee. For some time past the Professor has prosecuted inquiries into the efficacy of the chloride of aluminium as a dis-

infectant, and has made it the subject of exhaustive experiment. This substance, which he has named chloralum, is a salt, and for all practical purposes it may be viewed as the analogue of common salt. It is a hydrated chloride of aluminium, and has the active properties of chlorine or of hydrochloric acid without any causticity, odour, or any other hurtful property. Chloralum has a sweetish astringent or aluminous taste, and resembles as an antiseptic the solution of chloride of zinc, which has been extensively used, although its poisonous nature has led to serious accidents. But the great peculiarity of chloralum is that, although it is as powerful as carbolic acid, it can be taken like common salt. Indeed, medical men are now using it largely, and it is an invaluable agent as a gargle in cases of sore throat, diphtheria, scarlet fever, or whooping cough. Harmless as carbolic acid is generally supposed to be, we are aware of a case in which two horses were much injured by a quantity of the liquid falling over them. It is deadly to cats and other small animals, and recently a child died from sitting on a log which had been sprinkled with it. These are of course avoidable accidents, but it must be admitted that an absolutely harmless disinfecting agent which can with impunity be washed over the body or swallowed, must prove a great boon to all. Professor Gamgee unquestionably directs a deadly blow at the root of the evil—he attacks the germs of disease with this new disinfectant. Disease germs are unclean and noxious things, and they have to be removed and absolutely destroyed. When a mass of putrid matter has been treated with chloralum its decomposition ends. Whenever contagious or eruptive fevers, either in men or animals, prevail, their extension may, under prudent management, be restricted within very narrow limits by washing the body, immersion in baths, dipping clothing in hot solutions, purifying woodwork and all things capable of holding the deadly and microscopical particles which are so minute and invisible as rarely to exceed one twenty-thousandth part of an inch in diameter. Gaseous or volatile antiseptics are much used and recommended; sulphurous acid, condensed in alcohol, for instance, is portable, powerful, and effectual, but it affects respiration and rusts every metallic object wherever it is disengaged. On the other hand, the use of chloralum is attended by no annoyance whatever, the patient at the most experiencing the agreeable sensations of a mild vapour bath, whilst purification is ensured. The simple method now adopted of

fumigating a room by means of what is known as a scent-disperser can be applied to chloralum. By forcing a jet of steam or air across the top of a capillary tube, which projects from a bottle containing the chloralum, every portion of a chamber may be thoroughly purified without the presence of suffocating or acrid gases, as in the case of muriatic acid, which is sometimes used. Wherever chloralum adheres it has a tendency to attract moisture, to imprison the baneful dust, arrest the delicate and dangerous microzymes which float about the atmosphere, and thus its effects will be more lasting than those of a gas possessing less adhesive and persistent properties. To sum up the matter, we have in chloralum a strong preventive of decomposition and a powerful arrester of it when it has commenced. It absorbs the noxious gases resulting from putrefaction, as we have recently had occasion to know, and it destroys parasites and germs in alimentary and other substances. It may be used alike for medical purposes and for preserving food, and is, in fact, the safest and most efficacious antiseptic and disinfectant the labours of the chemist have as yet given to the world.—*Mechanics' Magazine*.

Ague Poison.

M. P. Bolestra has communicated to the French Academy some observations on ague poison. He says, that in examining marsh water he always finds, in proportion to its degree of putrefaction, a granular microphyte, somewhat resembling in form the *Cactus Peruvianus*. It is always accompanied by a considerable quantity of small spores 1-1000 of a millimetre in diameter, greenish yellow and transparent, and also by sporangia or vesicles containing spores from 2-100ths to 2-300ths of a millimetre in diameter, and of very characteristic form. This plant grows on the surface of the water; when young, it is rainbow-like in tints, and looks like spots of oil. At the low temperature of cellars and in water containing no vegetation, it develops slowly, but in contact with air and exposed to solar rays in the presence of decomposing vegetation, it grows fast, disengaging small gas bubbles. A few drops of *Arsenious acid*, *Sulphite of Soda*, or, still better, neutral *Sulphate of Quinine*, stops its vegetation at

the surface of the water, the spores become thin and transparent, and the sporangia alter so that they would not be recognised. These changes may be seen under the microscope. M. Bolestra states that these spores can be found in marsh air. He caught agues twice during his researches—once after having been exposed to air from water in fermentation covered with fresh algæ in full vegetation, mixed with an extraordinary quantity of spores. He thinks these spores constitute the ague poison.—*Manchester Examiner and Times*, Nov. 19th, 1870.

On the Nitrate of Potash as an Antiphlogistic.

By Dr. S. SAMUEL, of Königsberg.*

Dr. Samuel, in an interesting paper, gives an account of some experiments he has made upon the origin and course of inflammation, setting up that process by rubbing the ears of rabbits with pure *Croton oil*. He goes on to say:—

Against the inflammation caused by the *Croton oil*, whether taken just as it was beginning or when already lit up, I have employed numerous methods in the way of subcutaneous injection to ascertain their antiphlogistic value. Of all I tried, including the various preparations of *Opium* and its alkaloids, atropine, ergotin, and several neutral salts, the drug by which I was able to effect the most decided influence upon the course of the inflammation was *Nitrate of Potash*. If two rabbits are “crotonized,” first on one ear and three days later on the other, and one of the rabbits is regularly injected subcutaneously with this salt, the other not, but in all other ways treated exactly alike, while at the same time they are kept apart, there will be a great difference observable, in the case of the former animal, from the normal inflammation as seen in the uninjected rabbit.

In the animal treated with *Nitrate of Potash* the inflammation of the second ear will be markedly slighter; the hyperæmia, temperature and exudation are very much less observable than in the one where the process is allowed to run its course naturally.

The subcutaneous use of the *Nitrate of Potash* is so far attended with difficulties that concentrated solutions easily cause local inflammations if they are often applied to the same places. The

* From Virchow's *Archives of Pathological Anatomy and Physiology*.

formula I have found least injurious is the following:--*Nitrate of Potash*, oz. 3, simple syrup, oz. i, distilled water, oz. iv; of this about fifteen drops are injected twice a day in different parts of the back, avoiding the neighbourhood of the ears. The local inflammation then remains very insignificant, the animals bear the salt in this form some five days or so, and except decided emaciation in strong specimens, which are always to be preferred for these experiments, they undergo no injury, most of them recover perfectly and survive the complete experiment. The internal administration of the salt has, in rabbits at least, a much less certain effect.

Sulphate of Nickel in Neuralgia.

The interest of the following remarks does not lie, the *Richmond Medical Journal* avers, in the employment of *Sulphate of Nickel* in neuralgia, but in its therapeutic effects. We are told that it is a gentle tonic, acting like the preparations of *Iron* and *Quinia*. In this case, however, it seemed to exercise a sedative influence, more closely resembling that of the *Bromide of Potassium*. Mrs. B— has suffered with neuralgia more than three years. During the last two months the paroxysms have been very violent and frequent—occurring every few minutes. She has taken *Iron*, *Quinine*, *Arsenic*, *Strychnine*, *Colchicum*, *Aconite*, *Morphine*, *Chloroform*, *Valerian*, *Zinc*, *Mercury*, electricity, and many other remedial agents, with only temporary relief. As Professor Simpson had used the *Sulphate of Nickel* successfully in a case of severe and obstinate periodic headache, I concluded to try it, and began February 19th by giving her half-grain doses three times a day. In less than a week the paroxysms were reduced to only one within twenty-four hours: this came on at noon. On Sunday last (March 1st) it did not commence until about 3 p.m. I was present, and gave one grain of the *Sulphate*, notwithstanding she had taken her regular doses that day. Its sedative action was speedily manifested in reducing the pulse and producing sleep. All symptoms of the paroxysm disappeared, and Mrs. B— states that they did not return until 7 o'clock. In this case the *Sulphate of Nickel* has given more permanent relief than anything else. Mrs. B— tells me that it soothes her quicker than *Morphine*, and is not followed by any unpleasant effects.—*Public Opinion.*

O B I T U A R Y.

CHRISTOPHER DAVIS, M.B., C.M.

Homœopathy, the profession at large, the poor, but more especially the wretched sufferers from the war now devastating France, have sustained a heavy loss in the removal of Dr. Davis, who died of smallpox, after a very few days of illness, at Pont Mangy, on the 27th ult.

Dr. Davis was born at Bridgetown, Barbadoes, in 1843 ; he was educated at an excellent college there, and was originally intended for the ministry, but a complete change of view on religious subjects induced him to adopt medicine as a profession.

He passed through the usual curriculum at St. Bartholomew's Hospital, where he earned many distinctions (among others the third scholarship in the junior division and the examiner's prize in practical anatomy), and gained the respect and friendship of all with whom he came in contact, indeed, Mr. Paget considered him to be one of the most able students that ever worked under him.

Dr. Davis was holding (by deputy) the post of House Physician to that Institution when he was summoned so sadly—so suddenly away.

Dr. Davis passed the *annus medicus* of 1869-70, at Marischal College, Aberdeen, where the amount of mental and bodily fatigue he sustained was something enormous. In addition to his duties as dresser and clerk in the Royal Infirmary, he acted as private assistant to the professor of surgery ; he wrote his graduation thesis "On the two Diseases confounded under the name of Syphilis," prepared seven advanced subjects for the M.B. examination, attended the dissecting-room, frequently preached the gospel (for he was a true "Christopher") in the ballroom, which he hired himself—though it easily held 2000 people it was always crowded to excess—evangelised at distant towns in Scotland, such as Perth, and whilst this was going on he issued several works, displaying an unusual amount of critical knowledge of the Scriptures.

During this time Dr. Davis had not thoroughly recovered from

a dissection-wound, received in London, which had caused erysipelas, spreading over the thoracic wall.

Dr. Davis took his M.B. and C.M. in April, 1870, with the highest class honours in surgery (another homœopath, Dr. Bodman, of Devizes, we are pleased to add, took the second or government prize, with honours in midwifery). He also took a prize in botany, and got an honorary certificate in zoology. Whilst in Aberdeen he received his appointment as house-physician at Bartholomew's.

In September Dr. Davis took sole charge of an ambulance at Sedan containing 500 wounded and sick soldiers in a dreadful state of filth and wretchedness. Bavarians and French, dead and dying, closely packed on the beaten earth floor of an old wool factory; men's limbs literally bathed in the pus and blood from their wounds; windows permanently closed; every sort of filth abounding (Dr. Davis had forty cartloads removed as a commencement); a horrible stench pervading the rooms; no surgeons, no nurses, no supplies (there were two lemons and a bottle of brandy in the ambulance when Dr. Davis took charge). Dysentery and smallpox crowned those accumulated miseries.

This ambulance was cleared by Dr. Davis, with the assistance of his personal friends, at the cost of about £20 a day.

Even before this heavy charge was off his hands he formed and carried into execution the project of opening gigantic soup kitchens for the relief of the peasantry of Pont Mangy and Balan, where the unfortunate inhabitants were actually dying of want; Dr. Davis also assisted other ambulances in a state of starvation.

So eager was he to maintain his soup kitchens that, on one occasion when the supply was inadequate, he took the watch which he had gained as a college prize from his pocket and declared that he would rather sell that than suffer any to go away empty-handed.

Dr. Davis had been compelled to pay a flying visit to England; whilst there he worked so hard in soliciting aid for his poor people, and travelled so continuously, that he was quite worn out when he got back to Pont Mangy. In spite of his fatigue he insisted on visiting the Military Hospital at Sedan, where, going through the smallpox ward, he caught, as is supposed, the disease which, alas! proved fatal. He had always opposed vaccination, and had never been vaccinated. Had he availed himself of Jenner's great discovery, his useful life might probably have been preserved.

Dr. Davis's memory will live in many hearts and under various skies, but nowhere will it be more treasured than in the villages about Pont Mangy, whence, for years to come, one may venture to prophesy, pilgrimages will be made to the grave of "the good negro doctor," in the quiet nook at Fond de Givonne, where he was laid. To that grave he was followed by French, Germans, and English indifferently (amongst them Herr Strenger, the Prussian Sous-Préfet of Sedan), and by a long stream of sorrowing peasantry upwards of 2000 in number, a most touching and eloquent tribute to the memory of him whose devotion to them had cost Dr. Davis his life in the flower of his youth. There were not many dry eyes when M. Philipoteau, the Mayor of Sedan, concluded his address by the following words:—"Must it not be that God recompenses those who, like you, fall victims to their charity and devotion? Have we not the right to affirm to this numerous assembly, that, dying at the age of twenty-eight for the love of your kind, you have found there on high a bright immortality? May our ravaged districts not be long in finding a worthy successor of the good works of him, who was known to us as 'Le Bon Docteur Noir!' Adieu, Dr. Davis, adieu; or rather, *au revoir*, if only, one day, God might grant us an ending resembling yours, however slightly." The Fond de Givonne gates of Sedan were thrown open for the funeral procession, which had not been done before, even for the funerals of officers.

In person, Dr. Davis was of tall, commanding figure, with a broad chest and open, pleasant face; though very dark he did not possess the prognathous type of the true African, there was English blood on the paternal side.

He spoke fluently and with very pure and agreeable accent; he had the power of modulating his voice to a remarkable degree. He was singularly courteous and agreeable in manner, and was possessed of considerable *savoir faire*.

The doctor was a large-hearted man whose ear and purse were always alike open. He was indeed a true gentleman, and would rather have died than stoop to a mean and dishonourable action.

The University of Aberdeen, where Dr. Davis took his degree, may well be proud of him, and Barbadoes, where he was born, will hand down his name to posterity as a bright example.

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THE
BRITISH JOURNAL
OF
HOMŒOPATHY.

DISEASE OF THE CERVICAL VERTEBRÆ.

By J. H. NANKIVELL, Surgeon, York.

“THE case here to be considered is a disease of the spine, sometimes originating in an ulceration of the intervertebral cartilages, sometimes in a morbid condition of the cancellous structure of the bodies of the vertebræ, often followed by a more or less complete loss of the power of using the legs.” The first sentence in “Disease of Vertebræ” in S. Cooper’s admirable *Dictionary of Surgery* has been here transcribed as an introduction to the case I am about to describe. My friend Dr. Wolston, of Edinburgh, was sojourning in York during the latter part of August, 1868, and was requested to visit a little boy affected with paralysis of the legs and arms; he detected disease in the upper cervical vertebræ, and requested that I would admit the patient to the York Homœopathic Dispensary, at the same time stating that he feared the termination of the case would be unfavorable.

Frederick Walker, a scrofulous child, aged 3 years, was seen by me on the 29th of August, 1868. The history of the disease which I was able to elicit from the mother was very scanty. When the child was seven months old she

noticed that he had some specks on his eyes, but although he was always thin and delicate he walked at the age of twelve or thirteen months. At the age of sixteen months his head, it was noticed, dropped forwards, the chin resting on the sternum, and he began instinctively to support his head by placing his hands under the chin, and when lifted up, especially if his hands were not supporting the chin, he opened his mouth in a gaping manner. The parents did not suspect any spinal affection, as at this time there was no very palpable or evident deformity in the neck; by-and-by it was noticed that the child had no longer any power to hold things in his hands; he would grasp his toys and at once let them fall again. Soon after this the power of walking and then of standing ceased, nor had he even the power to sit up. He had no difficulty in urinating, but the bowels were much constipated. The appetite was good, the intellect "forward," and the memory excellent. I found the child lying on a sofa; he was much emaciated; he lay in his usual position, with both hands carefully supporting his head. Before I enter upon an account of the treatment adopted it may not be amiss to make a few observations on the records and opinions which our surgical works afford us respecting maladies of this nature. And, first, with respect to suppuration, Sir B. Brodie thought that disease which begins in the cancellous structure of the vertebrae is more immediately followed by suppuration than that which commences in the intervertebral cartilages; be this as it may, in the present case there were no indications, local or constitutional, of the formation of pus, and, as it terminated in a very pronounced angular curvature, the removal of portions of the cartilages or bodies of vertebrae must have been affected by interstitial absorption. The celebrated Pott recommended a large discharge of matter from the integuments on each side of the distempered bones, and thought it of little importance how this discharge was procured, provided it was large, came from a sufficient depth, and was continued for a sufficient length of time. Were he living at this time he would probably adopt a less (shall we call it?) heroic treatment. As our homœopathic journals are read

by many persons not members of the medical faculty, it may be instructive to recite what *has been* done, and, perhaps, in a few instances, is *still* done, with a view to cure this formidable "distemper" of bones. Thus, without tediously quoting the learned authorities, we shall string together a few passages as to treatment.

"Eschars having been formed by the application of caustic potash and then removed, a row of peas or beans connected together with thread should be laid on the sores and confined there with sticking plaster; a compress of sheet lead is then to be bound over the peas with a roller; these soon form little hollows for themselves, in which they should be regularly placed every day; if the granulations rise too high the surgeon must repress them by sprinkling a little savine powder and subacetate of copper mixed together (!). If this application is unavailing the reapplication of caustic becomes indispensable." Pott has known two years occupied with this treatment, and no doubt believed that the restoration to health and the use of the limbs was the plain and simple effect and consequence of his painful chirurgery. Desault advised the moxa and repeated cupping, a fact which is quite in keeping with the severity of French surgery as compared with English, even to the present day. Mr. Baynton and M. David thought that rest alone would effect a cure of diseases of the spine, but S. Cooper, in his concluding remarks, says, "In some cases caustic issues fail to afford relief, and when they are of *no use rest in the horizontal posture below ground*, I believe, must generally be the patient's doom" (!).

The opinion of surgeons with respect to the advantage to be derived from the support afforded by mechanical instruments in vertebral diseases has been very contradictory. By some it has been alleged that such appliances are not of the least use, whilst others have maintained that they are very beneficial. In my own practice, whether in the treatment of true vertebral disease with angular deformity or in that general disorder of the spinal column with (from whatever cause it arises) the want of equable muscular support terminating in more or less of lateral

curvature, I have always trusted to general hygienic measures, alternating rest in the horizontal position with active exercise for short periods, as it could be borne, and with the best results. Before I give the brief notes of my case I cannot refrain from alluding to one case of spinal deformity in the cervical vertebrae produced by spasm and contraction of the right sterno-cleido-mastoid muscle. The contraction had been the effect of severe scarlatina. The patient, a young lady of fourteen, had when I first saw her been affected with wry-neck for about six years; the parents had hoped that as she grew this deformity would disappear spontaneously, but in vain—it rather became worse. In truth, from the unfortunate delay in procuring advice, the muscles and ligaments attached to the four or five affected vertebrae had become accommodated to the altered position of the bones, and the sterno-cleido-mastoid muscle projected like whipcord, especially at its lower extremity. My advice was that the patient should frequently, every day, stand before a glass and practise such muscular movements as might tend to restore the symmetry of the neck; and that if after two months no improvement took place the best course would be to divide the tendinous insertions of the muscle which had caused the deformity, and then, as early as might be, to use means to bring into and keep in right position the cervical bones. This advice was not followed, and the patient was taken to London, where she was placed under the care of an orthopædic surgeon. She remained under his treatment and discipline for a few months, and then returned into the country. I was invited to see the wonderful apparatus which had been invented and contrived for the purpose of antagonising obstinate muscular pulleys. To my surprise the sterno-cleido-mastoid had not been divided. The apparatus consisted of an iron saddle fitted to the back and shoulders, and from the mesian line of this wonderful machine sprang a rod of steel which ascended even to the occiput; to the upper end of this rod was attached a curved piece of metal in the form of a horseshoe, which, encircling the head, reached to the chin. Then there were divers cunning

screws and straps and buckles, to make all right and straight. But although the much-enduring child had slept with this apparatus on her back for three months, and day and night had endured the misery of it, there was not the slightest change or improvement in the state of the parts, and no wonder.

But to return my patient, Frederick Walker had prescribed for him a very nutritious diet, strict confinement in bed or on a sofa, and to be very carefully removed from one to the other.

During the month of September he took *Silicea* 3 and *Arnica* 3 with no apparent result.

In October he had *Silicea* 3 in weekly alternation with *Calc. c.* 3, when the only difference apparent was some improvement in his general health and complexion.

During the latter part of November he had *Silicea* in the 30th dilution, and on December the 5th a rash broke out on the body (*Sil.* has itching pimples on the nape of the neck), and the mother thought that there was further improvement, because, as she said, "My boy strives more," by which she meant that he did not lie in so still a manner as he was wont on his couch. I cautioned her that she should never place the child on his legs for the purpose of trying whether or not he could stand, assuring her that when he was able to do so he would instinctively find out some way of getting down from the sofa.

On the 12th of December the report was that the tongue was white and the breath offensive, so he had *Sulph.* 30.

On the 19th the mother joyfully informed me that there had been a fresh crop of the rash (*Sulph.*?), but that he had unassisted got off from the sofa several times and stood by it, steadying himself with his hands on the cushions. And so during the course of four months we had, it would seem, made a steady and most satisfactory progress. Fearing that the child might be too adventurous, I directed that he should only be allowed to stand for a short time twice a day, and as he was a bright and intelligent little fellow there was no difficulty in getting this "ordination" carried into effect.

1869, January 2nd.—The rash is gone and the improvement is progressive. To go on with the *Sil*.

On the 30th the pleasing report was that he had started from the sofa and toddled across the room, thus practising his first steps for the second time. I had from time to time examined the neck, and found that there was a distinct projection of the spinal process of the fourth cervical vertebra, but gentle pressure did not cause pain, and the disease appeared to be arrested, judging from the general and local symptoms.

During February my little patient steadily gained flesh and strength, and the only thing to be remarked in his appearance was the forward projection of the head; he now walked about the house without any support or assistance, and the strength in his arms increased in proportion to that of the legs; the action of the bowels had become quite normal, but he complained of an occasional pain in the lump, as he called it; for this *Arnica* was given with good effect.

I have no further records of this case. During the year I saw the child at long intervals, and found that he was going on well; he continued to take his *Silicea* pilules, more from habit than necessity, and was sent to school, where, doubtless, he will distance his fellows in all mental athletics, although never in cricket or football.

Moral.—It is not required, in helping Dame Nature for the cure of vertebral disease, that the patient's frame should be enclosed in an iron frame, nor marred and scarred with issues.

P.S.—The following extract of a letter from Dr. Wolston may be of interest:

75, Queen Street, Edinburgh;
Jan. 20, 1871.

“ My dear Doctor,

“ I am delighted to hear of the happy issue that little boy's case has had in your hands. Certainly I entertained a very bad view of the case at the time I saw him, and should have been less surprised to hear of his sudden death

than that he now walks to school. So far as I remember his condition the only time I saw him, he was utterly helpless, could not lift his head, was spent and weak, and gave every evidence of advanced disease in the bodies of some of the cervical vertebræ. His parents being poor and unable to give him such nourishment and sea-side change as one would have liked to administer, added to the apparent certainty of the disease progressing, and, in a shorter or longer period, terminating fatally. The case is certainly worthy of record, and may, perchance, awaken a little helpful inquiry in the minds of some of our brethren in the old school as to whether there be any *truth* in the despised doctrine of similars, and any *value* in the despised drugs *Calcarea carb.* and *Silicea*."

DR. BOLLE'S HOOPING-COUGH ULCERS.

By Dr. H. GOULLON, jun., of Weimar.*

OUR colleague Dr. Bolle, of Aachen, has several times shown that it is not the allopaths only who cultivate "exact medicine." I may instance his observations respecting the specific remedy for uvulitis, which elicited the fact that *Merc. corr.* did everything that could be wished; the announcement of his new vulnerary method (*Arnica* and cotton-wool bandage); and lastly, his excellent treatise on hooping-cough (*Pop. Hom. Ztg.*, 1870, No. 5). His discoveries and deductions were acknowledged and confirmed repeatedly. And now I hasten to testify that the *ulcers under the tongue* first noticed by him are not peculiar to the hooping-cough epidemic described by him, but that they have been observed in the epidemic actually raging in Weimar. It is only thus, viz. by individual confirmation or refutation, that we can rightly answer Bolle's inquiry, "Are

* *Allg. Hom. Ztg.*, Bd. lxxxi, No. 24.

these ulcers *accidental*, or have they an *essential* connection with hooping-cough?"

The more than doubtful utility of actual specific remedies, which must be confessed by homœopaths themselves if they are honest and candid, was owing to our inability to discover the pathological processes. (The editor of the *Allg. Hom. Ztg.* denies both premisses and deduction.) Even post-mortem examinations of children who had died of hooping-cough failed to reveal anything positive. Hence medical men consoled themselves by reckoning this epidemically occurring spasmodic cough among the neuroses, just as they speak of nervous deafness and nervous blindness when their short-sighted eyes are unable to detect any obvious pathologico-anatomical product. But as many of these supposed nervous affections have, on closer investigation, been proved to be of quite a different character, it will be the same with the *tussis convulsiva*, whose prevalence in an epidemic form chiefly among children should have warned us against regarding it in such a light.

Considering the importance of the subject, before proceeding to record my confirmatory clinical observations and my reflections thereon, I shall give Bolle's first case that led him to his discovery.

"It was not until towards the end of the epidemy," says Bolle, "that I observed the visible circumstance that established, or at least seemed well adapted to establish, the existence of a virus of hooping-cough which had been diagnosed intuitively. It happened thus: I was visiting a family in which for four or five weeks I had been treating three children, from two and a half to seven years old, suffering from hooping-cough, without having been able to effect any real improvement with any of the vaunted remedies. At this visit, also, I was informed that the poor children still coughed terribly, and that one of them would eat nothing since the day before yesterday, as he complained of his tongue hurting him when he ate. I raised the point of the tongue and saw on each side of the frenulum a whitish spot about the size of a split pea, which very much resembled the ulcerated spots observed in secondary syphilis,


on the tongue, buccal cavity, lining membrane of the cheeks, tonsils, and palate. But the edges of these apparently ulcerated spots seemed to me to be less sharply defined than they are in the syphilitic affection, and the surfaces of the ulcers were, moreover, not depressed, but apparently level with the surrounding parts. They also resembled those whitish, creamy, or cheesy spots which we find in diphtheritis on the tonsils and velum palati. But they appear to be distinguishable from those croupy spots in this, that the latter are raised above the level of the surrounding mucous membrane, whilst the hooping-cough ulcers are not so. Thus they occupy a middle place between the syphilitic and diphtheritic spots."

I should also state that Bolle observed the same symptom in the same place, but only on one side of the frenulum, in the youngest child. The eldest child had nothing of the sort. He further examined five other hooping-cough patients, who had also been ill for from four to six weeks. In three of these he found nothing, but in one girl of five the spots were distinct on both angles of the frenulum, and in the fifth child there were distinct traces of the spot in one angle.

It remains to be mentioned that Bolle believed he had found the proper remedy for these ulcers, which, according to his view, are essentially connected with hooping-cough, in *Mercurius corrosivus*. He therefore gave this remedy to the hooping-cough patients in whom he could not detect these ulcers. He promises at a subsequent period to give the results of this treatment.

I may remark that this treatment may not prove successful; this will not overthrow the idea of these ulcers having a pathognomonic significance.

After reading Bolle's discovery I carefully examined the first hooping-cough patient that came under my care. It was a well-marked case of hooping-cough, with vomiting of all the ingesta, short breath, very quick pulse, and hot skin, so that the child, a boy of one year, always kicked off his bed-clothes and would not allow them to be laid over his legs. The remedies used, *China*, *Ipecacuanha*, even *Calomel* and

several others, did no good, but for some days past, by the mother's own admission, an improvement had followed *Sepia* 9. What did I find on examining him to-day? Just where the frenulum linguæ runs forward there was a distinct, real, broadish ulcer. Its border was somewhat caudate, somewhat resembling an  placed horizontally, its surface whitish, deeper than the borders; no other ulcers were visible in the neighbourhood. I was naturally struck with this discovery, although I could detect nothing similar in a brother of the child, who also was suffering from hooping-cough.

It may be asked, how is it that these ulcers have never been noticed in post-mortem examinations? But we have seen that these ulcers are not visible in all living children. Perhaps they bear the same relation to the general disease that the intestinal ulcers do to ileo-cæcal typhus, *i. e.* they are met with at a certain stage. The child I examined was in the same period as that observed by Bolle, it had, namely, been labouring for from four to five weeks under hooping-cough.

It may also be asked, how can ulcers under the tongue excite the characteristic irritation of the larynx? Leaving out of consideration that, if not actual ulcers, it is possible that erosions may exist in the tracheal and bronchial mucous membranes, it is conceivable the characteristic spasm may be owing to reflex action, just as coughing is produced by irritation of the meatus auditorius and irritation of the stomach, through the instrumentality of the ganglia connected with those parts.

Some light will also be thrown on the significance of this phenomenon when we consider that aphthæ, stomacace, and other morbid processes, not unfrequently attend hooping-cough; hence we may infer a vicious (dyscrasic) condition of the blood, which produces those ulcers, but which also causes an irritation to cough without the formation of ulcers. Hence we doubt much if Bolle will obtain universal success with his favorite remedy, but we rather think that a favorable modification or abbreviation of the disease is attainable by mild narcotics, such as *Drosera*, *Hyoscyamus*,

Viscum album, *Belladonna*; or by antispasmodics, such as *Cuprum*, *Zincum*, *Ferrum*; or by bitters, and especially by an antipsoric, such as *Sepia*, *Calcareo carbonica*, *Sulphur*, *Nitric acid*, or even *Mercurius*; but I don't believe we shall be able to cut the disease short as *Merc. corr.* does uvulitis.

I may here allude to a novel remedy, as it seems to show that ulcerous processes have something to do with whooping-cough. It has been observed that vaccination performed during whooping-cough has caused the latter to give way. Might not the suppuration of the arm have caused a cessation of the suppurative process in the mouth? As it is unvaccinated teething children who are most endangered by the effects of whooping-cough, the corroboration of this method introduced by Lachmund might be of great practical importance.

ADDITIONAL NOTES ON THE DIOPTRICS OF VISION.

By Dr. DUDGEON.

SINCE the publication, in the January number of this Journal, of the "Notes on the Dioptrics of Vision," I have pursued the investigations there commenced, and I take this opportunity of recording the results of my latest researches.

The winter season is not well adapted for the pursuit of experiments beneath the water, as the best swimming baths are closed during the cold months, and, moreover, a long-continued attack of rheumatism prevented me from engaging in this pursuit until the beginning of the present year. When I was again enabled to take to the water I was limited to one of the baths open during the winter, of a very second-rate character, as it is very poorly lighted and the water in it was generally in far from a satisfactory state as regards clearness, being often so turbid that the bottom

could not be seen on a tolerably bright day. Notwithstanding these drawbacks, I contrived to make it answer my purpose.

I find that the estimate I made of the magnifying power of the anterior lens of the eye formed by the aqueous humour was rather below the mark. I previously laid it down as a lens with a focus of two inches, as I found that good vision was restored under water by means of an air lens of that power, which was the highest power I had at that time made. This lens enabled me to see distinctly objects at a considerable distance below the water, and to read, though with difficulty, type of the size in which this is printed. But since then I have succeeded in constructing air lenses of much greater power, and I am enabled to state that a lens with a focus under water of one inch and a half enables me to read under water test type several sizes smaller than that in which the foot-notes in this Journal are commonly printed, while, at the same time, it allows of distinct perception of distant objects under the water. A lens of higher power enables me to read the same type with still greater facility, but this it does by magnifying the object, and it is not so good for distant objects as the lower power. Hence I conclude that the focus of one inch and a half accurately represents the power of the anterior aqueous meniscus of the eye.

There is another correction I must make in regard to the glasses employed in the construction of the air lenses. When I wrote my former paper I had to seek my watch-glasses in the shops of watch-makers, and to calculate their degree of convexity by a rough measurement by means of plaster of Paris casts. Since then I have found a manufacturer who cuts the glasses of any required convexity from the globes of glass. I have thus been able to ascertain that the glasses required to make an air lens of two inches focus in water are segments of a globe two inches in diameter, not two and a half inches as previously stated. In order to make an air lens with a focus of one inch and a half, segments of a globe one inch and three quarters in diameter are required, while for the production of an air lens with a focus of one

inch segments of a globe one inch and a half in diameter are needed. By combining segments of globes of various diameters any intermediate powers may be obtained.

At first I got an optician to connect these glasses by means of a metal ring, but in spite of all his care he was unable to make the lenses perfectly water-tight; the consequence of this was that some dampness penetrated to the interior and sufficed to cover the glasses with a film of condensed vapour utterly destructive of their transparency. I succeeded in obviating this fatal defect by means of rings of hard wood which I had turned in a lathe, and into these rings I fitted my glasses and cemented them into their sockets by means of a solution of shellac in alcohol, with which solution I thickly varnished the wood itself and thus obtained a perfectly water-tight air lens. Latterly I have improved on this plan by having the rings manufactured of ebonite, into which I cement the glasses with the shellac varnish. I have likewise designed a spectacle frame in which these lenses can be mounted and dismounted at pleasure, so that I can readily fix into the same frame lenses of any required power.

The refractive power of sea-water being somewhat greater than that of fresh water, lenses somewhat less concave are required for the former. Thus, an air lens made with a segment of a globe of two inches diameter united to the segment of a globe of one inch and three quarters diameter has in salt water the same focus as an air lens made of two segments of a globe of one inch and three quarters diameter.

A glass lens of three-eighth inch focus in air has in water a focus corresponding to an air lens made of two segments of a globe one inch and three quarters in diameter, viz. one inch and a half. A glass lens of one-quarter inch focus in air corresponds in water to an air lens made of segments of a globe one inch and a half in diameter, viz. one inch.

It is very difficult, I may say impossible, as far as my experience goes, to obtain glasses sufficiently thin and sufficiently free from imperfections that they shall offer no obstruction to sight in the air. The best of them will cause a slight diminution in the size of objects seen through them,

but the diminution is so very slight in tolerably thin and transparent glasses as to be scarcely perceptible.

The segments of globes I employ are cut to one inch and one eighth in diameter, as this gives a sufficiently large glass for securing a wide field of vision, and the highest powers of these can be fitted into rings not much more than half an inch in depth. Larger glasses would require deeper rings, which would render the spectacles cumbrous and unsightly.

The only writer I have yet met with who has attempted to estimate the power of the aqueous humour as a lens is Dr. Thomas Young, who gives the measurements of his own eye. He reckons the "focal length of the cornea for objects ten inches distant" as a fraction more than thirteen tenths of an inch, which would be an inch and a third, and a somewhat higher power than I have found it to possess.* I presume, by "focal length of cornea" he means the focal length of the aqueous humour, for the cornea itself, without the aqueous humour, can no more have a focus than a thin watch-glass. By the way, Dr. Young preceded Cramer, Helmholtz, and the rest, in assigning, as the cause of the accommodation of the sight for near objects, an increase in the convexity of the crystalline.

In my former paper I gave an account of my measurements of the anterior surface of the crystalline in an ox, which taught me that its form was not a segment of a sphere, but was rather that of an oblate spheroid, that is to say, flatter in the centre than at the sides. Sir John Herschel has kindly

* See *Phil. Trans.* for 1801, "Bakerian Lecture on the Mechanism of the Eye." Dr. Young's measurement of the focal length of the cornea is wrongly stated in the *Encycl. Brit.* (art. "Optics," p. 694) to be $\frac{11}{10}$ ths of an inch. If he means, as I have no reason to doubt, the focal length of the anterior lens, it is surprising how near the truth he was, and I am at a loss to know how he arrived at this result. He sets down the radius of curvature of the cornea at .31 inch, and that of the anterior surface of the crystalline at .30 inch. But this would give a concavo-convex lens of insignificant power, refracting the light divergently! Curiously enough, I find by calculations according to the method laid down in Brewster's *Optics* that a water meniscus of the curvatures given by Donders, viz. a radius of 8 mm. for its convex surface, and of 10 mm. for its concave surface, has a focus of rather more than $\frac{1}{3}$ rd inch (Young's estimate), and rather less than $\frac{1}{2}$ inch (my estimate).

pointed out to me that this was long since shown to be the case by M. Chossat, and he referred me to the paper of that author in the *Annales de Chimie et Physique*, vol. x (1818), entitled "De la Courbure des Milieux Refringens de l'Œil chez le Bœuf." I find, on reference to this paper, that M. Chossat gives the most minute and precise measurements of the curvature of the cornea and crystalline. I give, in his own words, the summary of these careful measurements :

"1. The cornea of the ox is a segment of ellipsoid.

"2. This ellipsoid is one of revolution round the major axis of the ellipse that represents the horizontal section of the cornea.

"3. This axis of revolution is always inclined inwards (towards the nostrils), and in oxen of from seven to nine years of age makes with the apparent axis an angle of 9° to 10° .

* * * *

"6. The surfaces of the crystalline are segments of ellipsoid of revolution round the minor axis of the ellipse.

"7. The true axis of each surface is always inclined outwards, and these two axes form between them an angle of about 5° in oxen of from seven to nine years old."

M. Chossat gives enlarged figures of these various curvatures, drawn with the utmost care, and it is curious to note that, while the figure of the cornea is such as to throw its greatest convexity into the centre, that of the crystalline, and more particularly of its anterior surface, is such as to make the centre the portion of the least convexity. The effect of this arrangement is that the more the pupil contracts the greater becomes the power of the anterior lens of the eye formed by the aqueous humour, for its convex surface becomes more convex and its concave surface less concave. As the pupil contracts much in accommodation for near objects, this increase in the power of the anterior lens must have some influence in producing the alteration of the optical condition of the eye. It is true that writers on ophthalmology cite cases where the power of accommodation remained though the iris was absent, but it is scarcely probable that this faculty was so perfect as when the eye was

eye of a skate which I likewise examined was remarkable. The internal capacity of the skate's eye is nearly the same as that of the turtle, but the ball of the former is much flatter anteriorly and the cornea much larger. But the chief difference is in the size of the crystalline. The skate's crystalline is nearly spherical, and its diameters are $\cdot 3$ and $\cdot 4$ inch against $\cdot 2$ and $\cdot 27$ inch in the turtle. But the distance from the posterior surface of the crystalline to the retina is nearly the same in both animals, that of the fish being $\cdot 3$ inch and that of the turtle $\cdot 32$ inch. There is also a great difference in their densities. The crystalline of the fish is extremely hard, whilst that of the reptile is very soft, and consequently possesses much less refractive power. Hence the smaller lens of the reptile, though nearly spherical, owing to its inferior density has a focal length somewhat greater than that of the fish. Were the crystalline of the turtle as dense as that of the skate, or were that of the skate as soft as the turtle's, in neither case could the focus be accurately projected on the retina. The one would fall as far short of the distance as the other would reach beyond it. On the whole, the turtle's eye is very similar in construction to that of a fish, and as there is no reason to doubt that the reptile sees perfectly in the air, so we may conclude that the eye of a fish is quite capable of seeing in air as well as water. I should mention that the turtle possesses eyelids and a fully developed lachrymal apparatus, showing its adaptation to vision in the air.

Before dismissing this subject I may mention that the eye of the skate has a very odd shape. Not only is it very flat anteriorly, but its upper surface is also quite flat, giving it the appearance of being compressed from above. The iris, too, is a very remarkable object. The pupil is not closed, as in terrestrial vertebrates, by a general orbicular contraction, but a fan-like process descends from its upper margin, and spreads over the pupil until it covers all but a narrow crescentic chink, and this process contracts when the pupil is uncovered, leaving only a little fimbriated fringe at the upper part of the pupil.

RECENT NOTICES OF HOMŒOPATHY.

THE stage on which homœopathy has now entered is that of unacknowledged reception. Our theories and practices are day by day being adopted by those who nominally are our bitter opponents. Our apologetic and polemical work has nearly ceased, and we are at leisure to devote ourselves to the improvement of our own modes of art and views of science, noting the while the process by which the ranks of the enemy are being leavened. Every now and then, however, a skirmisher comes to the front, and we must be ready to encounter the attack, whatever form it take, if only to show that we are alive and on the alert.

The *Saturday Review* is one of the few non-professional journals which avows hostility towards us. Its review of the pamphlet entitled 'The True and the False Sciences' showed its animus, and a paragraph in its issue of February 19th gives evidence that the same feeling still prevails. The paragraph in question reminds us of what Cuvier said of the definition of a crab submitted to him by its author for approval. It was that it is "a red fish, which walks backwards." He replied that it was a very pretty definition, except that a crab is not a fish, is not red, and does not walk backwards. So we must say of the *Saturday's* illustration, that it would be very effective if it were not that none of its statements are correct, and that the real facts tell just the other way. But our readers shall judge for themselves. We give here a letter which we wrote to our contemporary, but which (as was only to be expected) it declined to insert.

To the Editor of the 'Saturday Review.'

SIR,—In the number of your journal published on the 18th inst. there is an article upon the recent trial of Mr. Voysey. We are not concerned, as we now write, with the

general position taken up in that article. But there is an illustration given of the act of the Anglican Church in this clergyman's deprivation, which is founded upon a misapprehension, and would really tell the other way from that intended. We think that not ourselves only, or your reviewer, but the public at large, are interested in the rectification of the error; and we therefore crave space in which we may attempt to supply such correction.

The paragraph to which we refer is the following :

"If it is objected that these prohibitions are injurious to freedom of inquiry, it may be answered that they are no more so than the rules of any association which claims to be in possession of certain truths must be. The College of Physicians, for example, is not held to put undue limits on freedom of inquiry because it forbids its members to become homœopathists. It encourages them to weigh carefully the new truths that homœopathy claims to teach, but it maintains that the system, as a whole, is founded in error, and that in rejecting the usual Pharmacopœia homœopathists are guilty of a great medical blunder."

We do not stay now to ask for the evidence that the College of Physicians "encourages its members to weigh carefully the new truths that homœopathy claims to teach." We had thought, and grieved as we thought, that the reverse obtained; that the encouragement was to disdainful ignoring rather than careful weighing. This at least has been the fruit of whatever admonition has been given. Nor will we ask what is that "system as a whole," which the College of Physicians maintains to be "founded in error," and where is the record of its examination and condemnation. We are not ourselves aware of the existence of any such "system," and understand our position as simply that of endeavouring to infuse into medicine the "new truths" of which the reviewer previously speaks.

But the point to which we wish to direct attention is this, that the College of Physicians, instead of "not being held to put undue limits on freedom of inquiry because it forbids its members to become homœopathists," is (so far as it is a licensing body, and not a mere medical club) expressly

restrained by law from such forbidding, and on this very ground. The twenty-third clause of the Medical Act of 1858 runs thus:—"In case it shall appear to the General Council that an attempt has been made by any Body, entitled under this Act to grant qualifications, to impose upon any candidate offering himself for examination an obligation to adopt or refrain from adopting the practice of any particular theory of medicine or surgery, as a test of condition of admitting him to examination or of granting a certificate, it shall be lawful for the said Council to represent the same to Her Majesty's Most Honorable Privy Council, and the said Privy Council may thereupon issue an injunction to such Body so acting, directing them to desist from such practice; and in the event of their not complying therewith, then to order that such Body shall cease to have the power of conferring any right to be registered under this Act so long as they shall continue such practice."

So that the argument, if based upon this illustration, tells against your reviewer's case, and condemns, not Mr. Voysey, but the Privy Council. But the truth is that the two proceedings are not parallel. The Church believes (rightly or wrongly) that it is the guardian of a revelation, the maintainer and expositor of a sacred deposit of faith. No College of Physicians, or Medical Council, or any representative body in the profession, makes such claim on behalf of medicine. It is universally admitted that it is an art, founded upon sciences the boundaries of which are perpetually changing. It can never be stereotyped in creeds or articles; it knows no Master and has no scriptures. By inference, therefore, it has no heretics. Such statements as those of your reviewer tend to foster the opposite assumption—one only too readily engendered in professors of other arts and sciences. They forget how often the novelties denounced in one age have become the accepted truths of another; and they are just as hasty still to denounce the novelties, not because they are false, but because they are new. The art of medicine is too important to the public that such tendencies should be permitted to hold unrestrained sway; and the same consensus which approves the

deprivation of Mr. Voysey sustains the law which forbids the exclusion of the homœopath.

We are, Sir,

Your obedient servants,

(Signed)

J. J. DRYSDALE,

R. E. DUDGEON,

R. HUGHES,

} Eds. of *British Journal*
of *Homœopathy*.

77, Fleet Street.

We have nothing worse than misapprehension to charge against the *Saturday Review*. There is some one on its staff who dislikes homœopathy and occasionally vents his dislike, that is all. But it is otherwise when we read such an article as that on Dr. Rogers' book in the *Medical Times and Gazette* of February 4th. Here we have an example of the style of reviewing to which weekly medical journals are driven by their petty-tradesmanlike relation to their customers. Independence of thought is simply impossible to them; they live by pandering to the prejudices of their readers quite as much as by catering for their information. Dr. Rogers himself blesses homœopathy almost as freely as he curses it. But his reviewer, though professing to agree with him, ignores his commendations and exhibits only his allegations to our disadvantage, and takes the opportunity of serving up again the usual stale arguments and false assertions with which we are so familiar. The animus displayed and the sincerity employed may be judged of from the following passage. Dr. Rogers is pleased to approve of the homœopathic principle of the necessity of proving medicines on the healthy body. The reviewer deigns also to "say nothing but good" of such a recommendation. But, at the same time, he apologises to the customers. "Some people think the practice is part and parcel of homœopathy; it is nothing of the kind. Baron Stoerk was the first to introduce that method of ascertaining the action of remedies, and from him Hahnemann borrowed it." To us who know the facts such a statement implies either wilful misrepresentation, or an ignorance only less culpable. How sad it is that we cannot see a single medical periodical

conducted on high-minded and philosophical principles, where truth alone, from whatever quarter it come, is the object aimed at. We have, to a great extent, lived down personal prejudice. Our brethren of the old school no longer treat us as if the only alternative for us were fool or knave. But against the *system* we profess the antagonism is as fierce as ever. We wish we could think that the fierceness savoured of the holy indignation of truth against error. It reminds one rather of a certain uproar, wherein men cried "Great is Diana of the Ephesians," because their craft was in danger. We wonder who will be the judicious "town clerk" who shall appease this clamour, ere yet the public call its promoters in question for it, as being alike causeless and injurious.

It is time that this should be done, if their credit is to be saved. For side by side with the contemptuous denunciations lavished upon the homœopathic system, there is going on a silent appropriation of homœopathic ideas and practices, which cannot fail ere long to become manifest to others if not to themselves. The *Practitioner*, as a journal especially of therapeutics, is obviously the best register of this process. We have called attention from time to time to its approximations to our position. The article on *Mercury* by Dr. Ross in the number for October, 1870, is another instance to the point. It might almost have been written by one of ourselves *incognito*. The profession will wake up one morning, and find itself homœopathic in everything but name. We can only pray, with the historian of John Gilpin,

"May we be there to see!"

ON THE SPECIFIC CORRELATIONS OF *FERRUM*.

By Dr. HEINIGKE, of Leipzig.*

THE reference in No. 59 of the *Neue Zeitschrift* to the toxical symptoms observed by Warburton after the ingestion

* *Neue Zeitschrift für Hom. Klinik*, vol. xv, No. 11.

of *Liq. ferri sesquichlorati* brought me a confirmation I had long wished for of the reliableness of the principle of similarity in regard to a cure I effected several years ago, to which I had never up to that time found a precedent either in practice or in the writings of others. The case occurred in the winter of 1861, at a time when I practised according to Rademacher's system. For the benefit of those of my colleagues who are not familiar with this method of treatment, I may be permitted to make the following brief remarks respecting it. *Ferrum*, together with *Cuprum* and *Natrum nitricum*, belongs to the constitutional remedies, and constitutes a panacea, having a manifold application. The homœopathic practitioner, possessing such a wealth and multifariousness in his treasury of medicines, can get on very well in practice without *Ferrum*, but the Rademacherian practitioner, with his scanty supply of medicines, proved *ex usu in morbis*, cannot dispense with it. Hence it happens that the followers of Rademacher prescribe *Ferrum* in a multitude of forms of disease with direct curative action in which it would never enter into the head of a homœopathist to think of giving it. The preparations I chiefly employed at that time were the *Tinct. ferri æth. acet.* and the *Liq. ferri sesquichlorati*, at first in larger doses, similar to those used by Rademacher; but subsequently I was convinced that it was not the quantity, but rather the quality of the remedy which was mainly instrumental in the cures, and I gradually diminished my doses. According to my observations it is in most cases a matter of indifference which preparation of *Iron* we employ, as long as the specific *chemical* action is not required. *Chlorate* or *Chloride of iron*, given in sufficiently diluted form, follows the same specific lines of direction as other mild preparations of iron, with the same difference we observe in regard to other drugs, *e.g.* *Chloride of mercury*, *Chloride of barium*, &c., the combinations with chlorine modifying the organic functions more profoundly and intensely. The case in question was of the following description :

A stout blacksmith, in consequence of a severe chill, observed that the mobility of his lower jaw became gradually

diminished and more difficult, and at the same time he felt a painful spasmodic sensation extending from his temples to his lower jaw. Various domestic purgatives and vapour baths were employed for several days without effect, and as the malady increased I was sent for. On my arrival I found as well marked a case of trismus as any to be read of in books; the patient could scarcely open the mouth wide enough to admit of his sucking in liquids from a spoon. The genius epidemicus then prevailing led me to select *Ferrum*; I prescribed a solution containing *Tinct. ferri acet. æth.* In twenty-four hours there was decided amelioration; in three days there only remained traces of the obstruction to the free movements of the jaw. Under the use of the *Iron* a perfect cure was effected in five days; that is to say, the whole treatment was concluded within five days without the aid of any external appliance whatever. This enlivening of the scenery of the sick-bed, this scientific display of all sorts of apparatus and machinery for the application of water, steam, and what not, I have long abandoned, where I could dispense with it, to the gentlemen of the physiological school and to the midwives who know no better. Warburton's case gives me the desired confirmation that this cure by *Iron* also falls within the law of similars.

As appertaining to this subject, and as doubtless interesting to the reader, I will now give a brief *résumé* of the lines of direction of *Iron* in its action on the human organism, as far as these have been revealed by physiological provings and confirmed by observation on patients.

CORRELATIONS.

I. *Blood and lymphatic system.*—One of the most constant observations is, that the watery constituents and the white corpuscles of the blood are increased, whilst there is a slight diminution of the blood-fibrine; the red corpuscles become of a darker colour; the velocity of the pulse sinks from five to twenty beats per minute (according to Löffler's experiments on five persons). The abnormal distribution of

the blood in the vascular region of the centre and of the periphery is caused by the affection of the central organ of the nervous system (of which more in the appropriate place). According to Symp. 56 (*R. A. M. L.*, 3rd edit.) the lymphatic glandular system is directly affected by *Iron*; the results of physiological provings are deficient on this point, but there is no doubt that we are justified in believing it to be affected, from the observed increase of the white corpuscles of the blood, and from the records of the curative effects of *Iron* in glandular affections and in leucæmic constitutions. In the winter of 1862 I had an opportunity of making some interesting observations bearing on this subject. There occurred at that time several cases of illness among adults, where, after a chill, there appeared whole rows of swollen subcutaneous lymphatic glands in the neck, arms, and groins, with febrile symptoms and extreme debility. *Iron* effected a rapid cure; after each dose of the medicine slight perspiration ensued. The remedial power of *Iron* in certain forms of hydræmia and leucæmia is so well known that it needs but to be mentioned.

II. *Brain and cerebral nerves*.—Unmistakable hyperæmia and orgasm of the blood-vessels of the meninges and convexity of the brain (Symp. 1—2 *R. A. M. L.*, loss of consciousness in Warburton's case); the base of the brain seems not to be always affected, or at most only small circumscribed spots of the pons Varolii and medulla oblongata (tonic spasm in Warburton's case, neuroses of the nerv. vagus). The optic and olfactory nerves seem to be unaffected; so also the trochlearis and motor oculi; the diminished irritability of the iris (Symp. 41) may be ascribed to the consequence of the cerebral hyperæmia in the sympathetic root and the rad. long. nerv. naso-ciliar. in the ciliary ganglion. On the other hand, the nerv. trigeminus is considerably affected. The sensory portion seems to be implicated (Symp. 26) in the symptom, hyperæsthesia of the scalp, in the feeling of pain and burning in the conjunctiva and meatus auditorius (Symp. 35—38, 44 and 45); in the sensation of pain and numbness in parts supplied by the nerv. lingualis (Symp. 49). The motor portion of the

upper branches of the ramus maxill. infer. supplying the temporal and masseter muscles (Warburton's case of trismus). The facial nerve seems not to be affected, the auditory nerve only secondarily, in consequence of the congested condition of the brain (Symp. 46 and 47). The glosso-pharyngeal nerve reacts in the production of various alterations of the sense of taste (Symp. 94—100); the hypoglossal nerve seems to be affected, though it may be in a secondary manner, in Warburton's case (inability to speak). The vagus and spinal accessory nerves are markedly inflamed. We recognise three principal directions in which they are affected :

a. Neuroses in the sphere of the circulatory system :—Quick and small pulse (Warburton); slow pulse (Löffler); fainting (Symp. 246 and 247); ischæmia in the extremities (Symp. 206 and 232); ebullitions of blood (Symp. 286 and 287); scarcely perceptible pulse (Symp. 289).

b. Neuroses in the sphere of the respiratory system :—Laryngeal-bronchial catarrh and congested state of lungs; cough; expectoration of mucus, pus, blood (Symp. 151—190), with and without asthma and pains in various parts of the thorax.

c. Neuroses in the region of the fauces, œsophagus, and stomach :—Sensation of excoriation; pain and pressure in swallowing (Symp. 51—55); spasm of œsophagus (Symp. 161), with simultaneous affection of the motor fibres of the glosso-pharyngeal nerve; angina tonsillaris and pharyngeal catarrh; vomiting of acid mucus or of food, nausea and eructation (Symp. 57—73); stomach-ache, loss of appetite, distension of epigastric region, spasmodic pain in stomach (Symp. 74—89).

Sensorium :—Disinclination for thought; confusion of head (Symp. 14); weariness and sleepiness by day; difficulty of falling asleep, restless broken sleep at night, with vivid dreams (Symp. 255—271).

The local affections belonging to this sphere—blepharitis and conjunctivitis palpebr. et bulbi (Symp. 33—40).

Curative effects of *Iron* belonging to this rubric were observed in—

1. Hyperæmic states of the brain, meningitis convexitatis of children and adults, with an acute character, idiopathic and as a consequence of scarlatina, treated by myself and by other practitioners on Rademacher's plan.

2. Ophthalmia scrofulosa (Rademacher's school).

3. Delirium potatorum (by myself and other practitioners of Rademacher's school).

4. Trismus idiopath. (by myself in the case referred to).

5. Angina tonsillaris (by myself and Rademacher's school).

6. Laryngitis catarrhalis and pseudo-croup (by myself and Rademacher's school).

7. Pneumonia, pleurisy, hæmoptysis (by myself, Rademacher's school, the last also by some homœopathic practitioners).

8. Asthma (by homœopathic practitioners and Rademacher's school).

9. *Æstivatio sanguinis nocturna* (by homœopathic practitioners).

10. Gastrodynia, dyspepsia (by homœopathic and allopathic practitioners).

11. Chorea S. Viti (by allopathic practitioners).

III. *Spinal nervous system*.—*a.* Neuroses of the brachial plexus:—Pains in the muscular parts of the shoulder, along the arm, especially at night; arm as if paralysed; cramp and jerking in the hands (Symp. 196—208); swelling of hands.

b. Neuroses of anterior and posterior thoracic nerves:—Pains and feeling of constriction in muscles of trunk (Symp. 164, 172, 173, 191, 192, 196—204).

c. Neuroses of sacral nerves:—*a.* Plex. ischiaticus.—Sciatica; pains in muscles of thigh, leg, and sole of foot, with and without cramps; swelling of lower extremities, heaviness and stiffness of lower limbs (Symp. 209—234). *β.* Plexus pudendalis, uniting with the like-named branches of the sympathetic, as nn. anales, vesicales inferiores, vaginales.—Of the affections coming under this category I may mention proctitis catarrhalis, proctalgia, pains and cramp in the rectum, discharge of mucus and hæmorrhoidal blood from

the anus (Symp. 113—123); discharge of oxyurides (Symp. 114, 115). The other neuroses belonging to this division and those of the plex. pudend. communis will be mentioned under the affections of the organs in which these nerves are distributed.

The following curative results belonging to this rubric should be mentioned. Cure of—

1. Rheumatalgia of shoulder-joint and upper arm (by homœopathic practitioners).

2. Sciatica (especially of Rademacher's school, but also observed by myself).

3. Sudamina hæmorrhoidalia (by myself and Rademacher's school).

4. Œdema pedum (by homœopathic and allopathic practitioners).

5. Formation of oxyurides (by homœopathic practitioners).

IV. *Uropoietic and sexual organs*.—After the use of *Iron* the urine not unfrequently has a neutral reaction, sometimes also alkaline, and it then contains basic phosphates of lime, triple phosphates and mucus-corpuscles, perhaps also albumen (?) (Löffler and Kissel). Symptoms of catarrhal affections of the kidneys (?), of the mucous membrane of bladder and urethra, with affections of the sphincter vesicæ, were observed (Symp. 129, 133, 134) in the shape of urinary difficulties, blennorrhœa, fluor albus, hyperæsthesia of the mucous membrane of the vagina (Symp. 135—138).

Excited states of the sexual organs show themselves as persistent erection and nocturnal pollutions in males (Symp. 130—132); and metrorrhagia (rare), uterine blennorrhœa and hyperæmia, amenorrhœa, abortion, and sterility in females (Symp. 137—150).

Curative results from *Iron* in this sphere have been observed in—

1. Chronic vesical and urethral catarrh (by Rademacher's school and allopathic practitioners).

2. Pollutions (by homœopathic and allopathic practitioners).

3. Leucorrhœa, metrorrhagia, amenorrhœa (by homœopathic and allopathic practitioners).

4. Hydrocele (by Rademacher's school).

V. *Intestinal canal*.—The oxides, salts, and chlorine combinations of *Iron* taken in substantial doses form albuminates with the contents of the stomach and alimentary canal; these coagulated masses, or only to a small extent redissolved by digestion, reach the liver through the portal system; a portion of the iron is passed off through the bile, the superfluous iron still circulating in the blood is excreted by the kidneys, mammary glands (in the milk) and epidermis. The undissolved iron remaining in the intestines passes off with the fæces, without affecting the economy of the organism, and colours them reddish-brown when there is great excess of oxide, or black (chiefly in thin stools) by combining with sulphurous molecules in the bile to form simple *Sulphuret of iron* (Lehmann). Whether the symptoms it excites are caused by direct action on the sympathetic nerves, or only arise indirectly by the changes they effect in the quality of the blood, is not known. The following results of physiological provings should be noted here:—shootings in the bowels, meteorism, rumbling, colicky pains, painful heaviness in the abdomen when walking (Symp. 101—111); urging to stool, frequent motions, diarrhœa and burning in the anus (Symp. 121—128).

The curative results referable to this subject are:—

1. Circumscribed peritonitis, limited to small spots (by Rademacher's school and myself).

2. Diarrhœas (by homœopathic practitioners and Rademacher's school).

VI. Finally, as pertaining to the correlations of *Ferrum* to liver, spleen, and kidneys, it cannot be denied that the relations of the nutrition and functions of these organs are modified by it; but it has not yet been satisfactorily made out, either by physiological provings or by the results of pathological observation, in what manner the functions of these organs are influenced by iron. It may only be surmised that the alterations in the vital processes of these organs may be indirectly caused by the altered quality of the blood constituents.

From this explanation of the specific correlations of iron to certain structures of the organism, based on the results of physiological provings and pathological observations, we may infer that it was always the principle of similarity that ruled in all the cases of cures, even in those where this principle was not and will not be recognised. This fact should warn us to be cautious in our conclusions, so that in cases in which a specific cure takes place by means of a remedy with respect to which, in consequence of the paucity of the results of physiological provings compared with the infinity of morbid states, we cannot clearly see the concordance of the phenomena with the law of similarity, we may not at once conclude that we have here an exception to this law. There are in nature no exceptions to the prevalence of eternal laws; it is only man who imagines such when the causal nexus in the sequence of phenomena remains inscrutable to him.

EXPERIMENTAL RESEARCHES ON THE NATURE AND CAUSES OF CATARRHUS ÆSTIVUS (HAY- FEVER, OR HAY-ASTHMA).

By MR. CHARLES H. BLACKLEY, M.R.C.S. Eng.

CHAP. I.—INTRODUCTION.

§ 1. AT no period in the history of medicine has the investigation of the causes of disease been carried on more assiduously than it is at the present day. Such is the magnitude of the work, however, and so great are the difficulties which are inseparable from it, that comparatively little has yet been accomplished, and it is still, practically, one of the widest and also one of the least exhausted fields of inquiry in the whole domain of science.

It is not that it can furnish occupation to the thoughtful and curious mind merely, or that it offers an almost un-

bounded *terra incognita* to the scientific explorer who desires to traverse unknown regions, that it ought to invite our attention and make us desirous of laying bare its secrets. It has wider and more direct and intimate relations to the physical well-being of mankind than any question of mere abstract science can give, and it will be readily admitted that the successful elucidation of the etiology of disease is fraught with consequences the value of which it is scarcely possible to estimate. It is, moreover, probable that in this field of research there are to be found some of the brightest laurels which can be won by the scientific discoverer.

§ 2. Whether we accept the germ theory of disease, and regard these germs as portions of organic matter, which have a separate and independent existence and as capable of growth and multiplication, within the organism, to an extent at present undetermined, or accept what has lately been designated the "physical theory," and maintain that the causes of communicable diseases do not possess any of the characteristics of germs, we shall still have to admit that disease is, in a large number of instances, the result of the operation of two principal factors:—The one being a condition of the animal body which permits the development of disease in that organism—the other that of some agent external to the body, which becomes operative whenever there is present the condition above named, or whenever this external agent is in sufficient quantity to overcome the resistance which every organ is, in a state of health, able to offer to that which is inimical to the due performance of its functions.

§ 3. The first condition named, though often varying, is probably always present, in a greater or lesser degree, even in the most healthy subject. The second, in like manner, is probably as variable as the first, and, though possibly always present in the larger centres of modern civilisation, is ever changing in its quantity or its power.

§ 4. The essential nature of that state of the organism which gives a proclivity to disease, and permits external agents to act upon the animal body, so as to produce morbid

conditions, may never be fully known to us ; and it may be that, with our present means of research, the active causes of many of our most formidable maladies will elude our grasp for long years to come. There are, however, some of the more simple and less fatal diseases in which, by an improved method of study, we may hope to obtain a better knowledge than we now possess of the nature and *modus operandi* of those external agents which act as exciting causes ; and it is, perhaps, in searching for such that we shall most easily learn very important lessons in studying the etiology of disease.

§ 5. The importance of this study has been noticed by some authors who have long since passed away. One of these, who wrote in the early part of the present century, in speaking of the study and practice of the art of medicine, very forcibly says :—

“ As it is physical influences with which we have chiefly to do in medicine, the main and ultimate object in cultivating this art must consist in ascertaining the agency of external objects, whether salutary or noxious, on the living body, and in applying these, or avoiding them, so as to obtain the desired result, either of preventing the occurrence of disease or of converting the state of disease into that of health. It is to the extent and correctness of our knowledge of these agencies that the perfection of the art of physic must consist.”*

The remarks made by the illustrious author just quoted apply with considerable force to the study of the malady which is the subject of the inquiries and experiments described in the following pages.

§ 6. This disorder is an example of those which are mild and non-fatal in their character, and on this account offers opportunities for investigating it with comparative safety.

Many able authors have written upon hay-fever, and considerable effort has been made to explain and to account for its phenomena, but, so far, there has been but little progress made towards a complete understanding of these ; nor have we yet made much, if any, advance towards

* *Medical Logick*, by Sir Gilbert Blane. London, 1819.

obtaining a remedy on which we can depend as an effectual means of cure.

§ 7. Hay-fever or hay-asthma was first known in this country, and may indeed be said to have had its birth-place in England. It was first described by Bostock in the year 1819. In a paper read before the Medico-Chirurgical Society of London he gave an account of a case which was termed "A case of the Periodical Affection of the Eyes and Chest,"* and which was in reality a description of his own case. Heberden had previously mentioned† such a catarrh as is understood to have been what is now termed hay-fever, but he does not seem to have known anything of its real nature. Cullen also remarks that in some persons asthmatic fits are more frequent in summer, and more particularly during the dog-days, than at other colder seasons of the year. In 1828 Dr. Bostock read a second paper‡ on this subject before the above-named society, and gave a more lengthy and exact account of the symptoms of the disease.

§ 8. In the time which had elapsed between the reading of his first and second papers Bostock had either seen or had received "distinct accounts of eighteen cases," besides about ten others in which "the accounts were less perfect." In the latter communication the disorder was designated "Catarrhus æstivus" or "Summer catarrh." It is by this name it has since been more or less known among medical men, but from the circumstance of its having been noticed that it commonly came on during the hay-season it has obtained, among the laity, the name of hay-fever or hay-asthma, and I am inclined to believe that these names will be found to be the most appropriate of any yet used.

§ 9. The literature of the disease was, up to a com-

* "Case of a Periodical Affection of the Eyes and Chest," by John Bostock, M.D. *Medico-Chirurgical Transactions*, London, 1819, vol. x, part i, pp. 161—165.

† *Commentary on the History and Cure of Diseases.* 4th edition. London, 1816. Chap. "Destillatio," p. 113.

‡ "On Catarrhus Æstivus, or Summer Catarrh," by Thos. Bostock, M.D. *Medico-Chirurgical Transactions*, 1828, vol. xii, p. 437—446.

paratively recent period, very scanty. Bostock's first paper was published, as stated above, in 1819, and between this and his second communication in 1828 there was an interval of nine years, in which no other notice of the disease appeared in a printed form.

§ 10. In 1828 Dr. Macculloch* mentioned the disease, and in speaking of its causes says, "It is produced by hot-houses or green-houses, and in the public estimation it is particularly caused by hay-fields."

§ 11. In 1829 Mr. W. Gordon published his "Observations on the Nature, Cause, and Treatment of Hay-Asthma."†

§ 12. In 1830 Mr. Augustus Praeter published a short notice of a case he had seen in Paris some years before.

§ 13. In 1831 Dr. Elliotson noticed the disease in his lectures, and again in 1833 he gave a more lengthened account of the malady.

§ 14. From this latter period there was again an interval of ten years during which there was not anything of a special character published on hay-fever.

§ 15. Since the time just mentioned the subject has gradually excited increased interest amongst the laity and also amongst the members of the medical profession; and it would seem that there are now a greater number of cases to be met with than there were formerly. Perhaps this may, in part, be due to the increased attention, directed to the disease, having had the effect of bringing the cases, which do occur, much more distinctly under the notice of medical men than they have been brought at any previous period; but it may also be, in part, accounted for by the greater prevalence of those conditions which act as pre-disposing and exciting causes.

§ 16. This increased interest in the disease has shown itself in the publication of numerous articles on it in the

* *An Essay on the Remittent and Intermittent Diseases*, by Dr. John Macculloch, London, 1828, vol. i, pp. 394—397.

† "Observations on the Nature, Cause, and Treatment of Hay Asthma," by Wm. Gordon, M.R.C.S. Edin. *London Med. Gazette*, 1829, vol. iv, pp. 266—269.

periodical medical literature, and in the works of various writers on systematic medicine, both on the continent and in this country. There have also been several treatises published in a separate form during the last few years. Among the principal of those which have been published in this country may be mentioned those of Dr. Abbotts Smith,* Dr. Pirrie,† and Dr. G. Moore.‡ Although the disease is more prevalent in England than in any other part of the world, it is to a German author (Dr. Phœbus, professor of medicine at Giessen) that we are indebted for the best monograph § that has yet appeared on hay-fever.||

§ 17. In speaking of the disease having been brought so recently under notice Bostock says:—

“ One of the most remarkable circumstances respecting the complaint is its not having been noticed as a special affection until the last ten or twelve years. Except a single observation of Heberden’s I have not met with anything that can be said to refer to it in any author, either ancient or modern.”¶

As far as the researches into the literature of the disease up to the present time have shown, there does not appear to have been any notice of it previous to that which is here mentioned by Bostock.

§ 18. Considering the high character which the physicians of that day bore as acute observers, it seems strange, at first sight, if the disease did exist, that it should not have been recognised and described by them. Whether the malady had been only recently developed when Heberden first

* *Observations on Hay Fever, Hay Asthma, or Summer Catarrh*, by Wm. Abbotts Smith, M.D., M.R.C.P.Lond. London, 1865. 2nd edition.

† *On Hay Asthma, or the Affection termed Hay Fever*, by Wm. Pirrie, M.D., &c. &c. London, 1867.

‡ *Hay Fever, or Summer Catarrh: its Causes, Symptoms, Prevention, and Treatment*, by George Moore, M.D. London, 1869.

§ *Der Typische Frühsommer Katarrh, oder das sogenannte Heufieber, Hay Asthma*, von Philipp Phœbus, M.D., &c. &c. Giessen, 1862.

|| It is to this work that I am indebted for the names of all the foreign authors, and for those of several English authors, who have written on hay fever.

¶ This remark occurs in Bostock’s second paper, published in 1828.

mentioned it, or whether it was then of much older standing, we may not now be able to ascertain.

§ 19. Dr. Copland tells us that, up to the time of Sydenham, rheumatism and gout had been regarded as one and the same disorder. These forms of disease have less similarity and are more distinct in their characters than are the catarrhal form of hay-fever and common coryza; whilst in the asthmatic form of hay-fever it is impossible to distinguish between the latter and common asthma except by determining what the exciting cause is.* It may not, therefore, seem so remarkable that hay-fever may have been long mistaken for the ordinary form of coryza or of asthma; and when we consider that there are not wanting instances of even medical men having been affected by this malady, and who have not become fully aware of its nature until they had suffered from it for some time, it may be considered highly probable that it had occurred in isolated cases long prior to the time when it was first noticed by medical authors.

§ 20. Hay-fever is said to be an aristocratic disease, and there can be no doubt that, if it is not almost wholly confined to the upper classes of society, it is rarely, if ever, met with but among the educated. Dr. Phœbus and other writers speak of cases which have occurred among the working part of the population. I have myself never seen or had any such cases brought under my notice; and I think it is tolerably certain that, if they occur at all, they do so but very seldom. I have met with cases of chronic catarrh among the working classes which seemed at first sight to resemble hay-fever, but when tested in the manner described in my experiments, the patients were found not to be amenable to the same influences as are those individuals who suffer from the genuine disease.

§ 21. In perusing the literature of hay-fever one is struck with the great variety of opinion which prevails upon its causes, and also, in some degree, upon its nature, but more especially with the small amount of success which has apparently attended its treatment. This want of unanimity of

* This remark applies only to the purely asthmatic form of the disorder.

opinion on its causes, and the want of success in its treatment, have perhaps been the most marked where medical men themselves have been the subjects of the malady; whilst, on the other hand, singular as it may appear, those who seem to have been the most fortunate in treating others have themselves never suffered from the disorder.

This diversity of opinion and the failure which has attended the efforts to cure the disorder may be, and probably are, to some extent, due to the absence of any attempt at proving by inductive methods of experimentation the precise nature of its causes, and also in not endeavouring to ascertain by the same means the relative value of the remedies used in its treatment; and, to use the words of a recent writer, it is "a matter of astonishment that greater efforts have not been made to elucidate the doubtful points relating to its history, causes, and treatment, and thus to obtain a more certain guide to the relief or cure of the disease."*

§ 22. In the course of my reading on this subject I became convinced that something more needed to be done, than had already been accomplished, before we could say that we had got hold of the "sum of the facts" to which the disease owes its existence. It seemed as if we had hitherto failed to grasp the idea that "the cause cannot be anything which is present in other cases where the given effect is not produced, unless the presence of some counter-acting cause shall appear to account for its non-production."†

§ 23. In regard also to the mass of evidence already collected there seemed to be a great need of this being sifted and re-arranged on a more logical method; and, more than all, there appeared to be a necessity for a collection of additional facts in order that we might fill up the missing links in the chain of evidence, so as to give us the means of a closer and more correct generalisation.

The annual recurrence of the malady at a given period of the year, the almost certain departure of it after a given

* Preface to Dr. Abbotts Smith's first edition of his work on *Hay-Fever*.

† Archbishop Thomson's *Outline of the Laws of Thought*.

time, the entire freedom from it which most patients enjoy for the greater part of the year, the absence of any dangerous symptoms, except in rare cases, as well as the non-occurrence of sequelæ of a serious character, seem to offer opportunities for safe experimentation such as are rarely found in any other complaint, whilst the presumed exciting causes are such as to present no great obstacle to their being fairly tested during the intervals of freedom from the disorder.

§ 24. If any reason needed to be given for these investigations having been undertaken by me, it is partly furnished in the remarks made above, but principally by the circumstance that I have myself been a sufferer from this curious malady for more than twenty years.

Although I had in the earlier years of my attacks of hay-fever carefully read all the then very scanty literature of the disease, I had not been able to form any very definite and settled notion of the nature of the cause. I was inclined to regard heat as the principal exciting cause, but my experience did not quite coincide with the opinions of those who had written on the disorder, and this experience had, unfortunately, compelled me to come to the conclusion that, until something more was known than I had learned from the writings of others or from my own previous observations, there was no chance of escape from the annual torment. I had thus a personal interest in getting a more thorough knowledge than I then possessed of all the phenomena of hay-fever; and whilst I was in this way furnished with a good and sufficient reason for commencing the investigations, the annoyance caused by the annual attacks acted as a powerful stimulus to exertion in making these as complete as my somewhat limited time and opportunities would permit.

§ 25. The experiments, which I have to describe in a succeeding chapter, were commenced in the year 1859, but owing to various circumstances, which were not controllable at the time, proceeded very slowly for some years. This delay was in some measure owing to the difficulty there was in sparing sufficient time for a lengthened and uninterrupted

course of observations during some of the spring and summer months. For the purposes I had in view, it was absolutely necessary that a number of observations should be made each day during the period named, and that these should commence some time before the grass came into flower, and be continued until it had been all gathered in. It was also necessary that some person who suffered from hay-fever should be exposed during a portion of the day, at least, to the influences which might prevail in the district in which the observations were taken, and that the symptoms generated by such exposure should be registered daily. It would have been more satisfactory, and would have brought out more exact results, if the patient to be experimented upon could have been kept within a short radius of the spot selected for the first series of experiments; but as this would have involved a sort of imprisonment for at least ten weeks, it was impossible for me to follow out the investigation in this very precise manner. What, however, the observations have lacked in this respect, I have endeavoured to some extent to make up for by following out the inquiry under varying circumstances and in different districts. It would also have been well if I could have had the co-operation of some other individual who, like myself, was a sufferer from hay-fever. In two attempts which I made to induce others to undertake experiments upon themselves I failed, although these were of a very limited and simple character.

§ 26. There is one part of the subject which would well repay careful investigation, viz., the chemical constitution of the pollen of various plants, and especially those of the order Graminaceæ. Into this I have not been able to enter, but I cannot doubt that it would be well worth the time and trouble which would have to be expended on such an inquiry, if some able chemist would take up the matter and ascertain all that can be known of the constituents of pollen.

§ 27. In the first years of the disease, in my own case, it had more than once happened that, when some particular plan of treatment was being pursued, this would seem for a time to be successful in mitigating the severity of the

symptoms, and, in some instances, in apparently curing the malady; but suddenly, and without any change in the treatment, there would be a complete relapse, and the condition would become as bad as before. Subsequent experience, in seasons when the disease was present, and when no treatment of any kind was being used, convinced me that these variations were not due to, or indeed, in any degree, influenced by the treatment adopted, but that they were probably the result of a variation in the *quantity* or *quality* of the causes of the malady, whatever these might be.

§ 28. It seemed, therefore, to be highly important that not only the *nature* of the cause of this disease should be discovered, but that we should have the means of measuring its variations in *quantity* as well as *quality*; and that we should particularly study those circumstances which lead to these variations. There was also another question to which it was important to obtain an answer if possible, viz., whether in any individual case the disease owned only one exciting cause, or whether it had a plurality of causes. Until the latter questions were satisfactorily settled it was clear that any effort to gain relief, by avoiding the one supposed cause, might be completely frustrated by the patient unwittingly putting himself under the influence of other hostile circumstances. But once accomplish these objects, and we should not only be better able to gain relief by avoiding the cause, but we should have the means of erecting a standard by which we should be able to estimate the value of the remedies used in the cure of the disorder. So long, however, as we were ignorant of the real nature of the influences which give rise to the morbid conditions and of the possible rates of their variation, we were constantly liable to be misled in our estimate of the efficacy of the remedies used.

§ 29. It was with the hope of accomplishing something in the direction indicated that these experiments were commenced. The main object was to find out what were the exciting causes of the attacks in my own case; but as an examination of the records of other cases showed me that in its symptoms, and in the conditions which seemed to give

rise to these, my own case was almost identical with a very large majority of those I had investigated, it appeared to be highly probable that if I succeeded in ascertaining the cause in my own case, I should also be doing the same thing for a large number of other patients. If, after making the attempt, I found that I was not able to do satisfactorily all that I have indicated above, I still hoped to glean some fresh facts in this field of inquiry, which might serve as stepping-stones for the further progress of those who might follow me.

CHAP. II.—A REVIEW OF THE OPINIONS HELD ON THE CAUSES OF HAY-FEVER.

§ 30. In the last chapter I have mentioned incidentally that in its two principal phases hay-fever resembles common catarrh and ordinary asthma. Although this description is very partial and imperfect, it will suffice for our present purpose. When we come to consider the symptoms of the disorder in detail, I shall then be able to note their peculiarities, and to give to each its due significance.

In the present chapter I propose to pass under review the leading opinions which have been held on the causes of the ailment. We shall by this means have a better idea of their variations and peculiarities than would be had if they were introduced in a fragmentary manner in the course of remarks on other parts of the subject, and we shall to some extent have the ground mapped out which I have had to occupy in my experiments.

§ 31. Not only have the opinions which have been entertained on the causes been very varied, but, as I have before hinted, these have in some cases been very conflicting. The most opposite conditions have, by some writers and also patients, been thought to be capable of producing the disorder.

In some cases high temperature with dryness of the atmosphere have been held to be sufficient to produce the symptoms of hay-fever in persons who are liable to it. Some patients

have thought that excess of moisture with high temperature has brought on the disorder in their cases. By some authors ozone is named as a possible exciting cause of the disease, and by others odours of various kinds, especially those given off by plants, have been accused of being the causes. In some cases common dust has been thought to have a considerable share in bringing on the disorder, whilst in a comparatively large number of instances the agent which has given the popular name to the malady has been taken to be the principal if not the only exciting cause. In an equally large number of instances, however, the pollen of grass, and other flowering plants, has been held to be the most active and efficient of all causes, but among these the pollen of grass is by some writers held to be the most powerful.

§ 32. Upon one point, however, all authors are agreed, viz., on the existence of some peculiarity of the constitution which predisposes to the disease. Whether this peculiarity should, however, be regarded as simply a local one, to be found in a particular vascular, or other, condition of the mucous membranes affected, or whether it is to be sought for in the periphery of the nerve, or in some part of the nerve tract supplying these membranes, or whether we must go to the sympathetic system, or to the larger nerve-centres, and seek there for the predisposing cause of the malady, is not yet decided. Curious and unaccountable as the predisposition seems to be, we shall, when we know more of its nature than we now do, probably find that, in the modes of its manifestation, and in the laws which govern these, it does not differ very widely from the peculiarities which give a proclivity to other and essentially different forms of disease.

§ 33. Bostock, who was the first writer who gave a full description of the ailment, believed that in his case it was not caused by the effluvium of grass or hay. He thought that heat had the greatest share of influence in producing the attacks. After the attention of the public had been drawn to the existence of the disease, probably by the publication of his first paper, the idea prevailed that it was

caused by the effluvium of hay recently made. Bostock was desirous of testing the accuracy of this opinion, and made it the subject of study and close observation so that he might be able to determine the cause of the attacks in his own case.

In speaking of this part of the subject, after he had carefully studied it and watched the effects of variations of temperature and other conditions; and especially after he had, as he thought, watched the effects which grass in flower, and also when turned into hay, had in producing the attacks, he says :—*

“I think myself fully warranted in asserting that, in my own case, the effluvium from hay has no connection with the disease. The observations will, I think, be sufficient to prove this position.

“In consequence of the benefit which I always experience from cool fresh air I made choice of Ramsgate as my residence during the summers of 1824, 1825, and 1826. The last two of these years will be long remembered for their excessive heat; but by procuring a house on the cliff exposed to the German Ocean, and commanding complete ventilation, by avoiding bodily exercise, and, indeed, seldom leaving the house until evening; during the year 1825 I nearly escaped the disease. In the year 1826 I have reason to believe that the disease was much mitigated by the comparative coolness of the situation, but still I had many decided and some severe paroxysms.

“Now it is well known that there is not an acre of meadow-ground in the whole of the Isle of Thanet, and in the year 1826, in consequence of the great drought, all the little patches of grass which may be supposed to exist on the road sides, or elsewhere, were completely burnt up.

“Nor is this all, during many of the hottest days the wind blew steadily from the south-east, so that the nearest land to windward of the house which I occupied was on the French coast a little to the north of Calais. Yet, during this time, whenever I relaxed my plan of discipline, and

* *Medico-Chirurgical Transactions*, 1828, vol. xii, pp. 437—446.

exposed myself to the sun's rays or by any means quickened the circulation, the symptoms recurred in full force.

"The last year, 1827, with the exception of a short period in July, was cold. I could not conveniently remove to any great distance from London, and spent the summer in Kew. This situation might have been chosen for the purpose of experiment, for almost the whole of that part of the country consists of hay-grass, which was cut whilst I was in the neighbourhood. In consequence of the coolness of the season I did not confine myself to the house, but walked out daily, occasionally in Kew Gardens, and was surrounded by many hundreds of acres of hay-grass, in all different states, yet except during the few hot days, when I suffered as usual, my complaint was in a much less degree than the average. But although I think the evidence, so far as respects myself, to be quite decisive, I acknowledge that I have received accounts from various quarters of individuals who have felt no doubt, that the complaint was brought on by the effluvium from hay, and was relieved or prevented by avoiding this effluvium. I will not venture to assert that this opinion is incorrect, but I believe that in most cases we may explain the facts more naturally by supposing that the patients, at the time when they conceived themselves to be inhaling the effluvium from hay, were also exposed to heated air or sunshine, or had been using bodily exercise. Experience, however, must decide the question, and when the subject is once fairly brought into view it will not be difficult to collect a sufficient number of facts to enable us to form an opinion."

There is, in these observations of Bostock's, an evident effort to ascertain by careful experiment the real cause of hay-fever. It is this honesty of purpose which has probably led subsequent writers to accept his conclusions without submitting them to a searching examination such as their importance demanded; and his statements have also probably given the cue to some authors who have written on the disease without having had any personal experience of it. How far these statements of Bostock's are borne

out by the results of actual experiments I shall have occasion to notice further on.

§ 34. Gordon, who was a contemporary of Bostock's, took a different view of the cause of hay-fever. Whilst he recognized the circumstance that many cases were on record which showed that severe derangement of the function of respiration was sometimes "occasioned by the odour exhaled by aromatic pungent bodies," he thought there could be no doubt that hay-fever was caused by the "aroma emitted by the flowers of grass, particularly from those of the *Anthoxanthum odoratum*."

He had been inclined to adopt this view by noticing that "whenever the patient remained closely shut up in a house, even though this was situated in the midst of the richest grass, he suffered considerably less than if he walked abroad into the fields; and if he removed from the country to the centre of a large town he was never at all affected; but the moment he came into or approached a meadow he immediately began to sneeze and returned home with wheezing and difficult respiration."

In summing up his observations Gordon says:—*

"I have said that the *Anthoxanthum odoratum* seems to be the principal exciting cause of hay-asthma, and I am induced to come to this conclusion—*first*, because this plant is one of the most strong-scented of the grasses; and *secondly*, because so soon as it begins to flower, and not until then, the asthma commences; as the flowers arrive at perfection the disease increases, and after they have died away I have remarked that patients could pass through the most luxuriant meadow with total impunity. The disease then should rather be denominated grass-asthma than hay-asthma, since hay seems incapable of producing it."

§ 35. Elliotson, who was also a contemporary of Bostock's, argues forcibly against the views entertained by the latter, and reviews his opinions at considerable length.† He, however, agrees with Bostock in believing that the disorder is not caused by hay, but, contrary to what the

* *London Medical Gazette*, vol. iv, 1829, pp. 266—269.

† *London Medical Gazette*, vol. viii, 1831, pp. 411—413.

latter affirms, he believes it to "depend upon the flower of grass, and probably upon the pollen."

Elliotson also contends that this view of the case is supported by the circumstances that the disease does not usually appear "till the grass comes into flower; and as long as there is any flower remaining on the grass the disease continues."

In speaking of Bostock's account of his experience in the Isle of Thanet, when he suffered very little from the disease and when the heat was so great that nearly all the grass was dried up, Elliotson remarks :—*

"I can conceive that a minute quantity of the emanations from the flower of grass is sufficient to produce it—so minute that you can be in few parts of the country at all, without the chance of its reaching you through the atmosphere, emanating from some grass or hay."

And again, in speaking of Bostock's experience whilst staying at Kew, he says :—†

"Dr. Bostock mentions, as another argument, that he was at Kew one summer where there was a great deal of grass growing, but he did not then have the affection severely. He mentions, however, that it was a cold season, and in a cold season you are aware that exhalations do not take place to anything like the extent that they do in hot seasons. That, I think, would account for the difference. But what makes me believe that it does depend upon the flower is, that a lady has lately given me an account of her own case, in which the symptoms appear and gradually increase as the grass comes, more and more, into flower, till at last they arrive at such an intensity that she is obliged to leave home and go to the sea-side, and she is always relieved by shutting herself up in a room."

Dr. Elliotson also mentions that he had heard a paper read at the Royal College of Physicians, on one occasion, where the patient described suffered from hay-fever, and was always seized with the symptoms of this disease when-

* *London Medical Gazette*, vol. viii, 1831, pp. 411—413.

† *Ibid.*

ever she approached a field of sweet-scented grass : In reference to this case he says :—

“ I do not know what kind of grass produces hay-fever, but this would make it appear that it arises from an emanation, for whenever the lady approached a field of sweet-scented grass she was always seized in this way.”

§ 36. Macculloch, who wrote in 1828, briefly refers to hay-fever as a possible form of intermittent.* He speaks of it as a well known disease, and as I have before stated, says “ It is produced by hot-houses and green-houses, and in the public estimation particularly by hay fields.” He does not, from the fact of hay-fever being a periodical catarrh, mean to say that it must be, therefore, a mode of intermittent disease, but he thinks, from the circumstance of its “ having a quotidian period and being the produce of heat and vegetation, it at least presents features of analogy which render it worthy of being noticed in his work, and also of being more minutely studied ; as far, at least, as we can investigate a disorder generally too trifling to attract much notice.”

§ 37. A paper published by Dr. T. Wilkinson King† is chiefly remarkable for the manner in which the author mistakes the true character of hay-fever and for the way in which it helps to confuse, rather than to clear up, the subject in the mind of the reader. Dr. King had suffered from the disease, he says, for about fourteen years, and thought it worth while “ to set down his own conclusions, together with an additional fact or two,” so that he might “ point to and confirm more general views of disease.” The various causes of the local irritations as given in books Dr. King thinks are more curious than instructive, and, after noticing the various conditions which affect asthmatic patients favorably and unfavorably, he goes on to say :—

“ The preceding and countless vagaries are to be accounted for by simple principles. (Vide a series of papers on

* *An Essay on the Remittent and Intermittent Diseases*, by Dr. John Macculloch, London, 1828, pp. 394—397.

† “ On Summer Asthma, Catarrhus Æstivus, or Hay-Fever : its Causes and Treatment,” by T. Wilkinson King. *London Med. Gazette*, 1842—1843, vol. ii, pp. 671—675.

"Angina," *Medical Gazette*, 1841.) We have to calculate that according to circumstances, a certain number of hours having elapsed after exposure (specific or general), eating or lying down, capillary excitement or distension is to be evinced by uneasiness, obstruction, and various forms and quantities of secretion. Beyond this, we believe very few of the phenomena of hay-fever or asthma will remain unresolved. The time at which the affections prevail is the time of diminishing our clothing..

"It seems to me, in the first place, not unreasonable to compare these affections with summer eruptions I think I have, at different times, experienced most of the symptoms described by Dr. Bostock, *but not only in summer*. I am subject to slight attacks of dyspnœa, especially on lying down, attended by a very slight ropy and clear secretion in the trachea. I make very little doubt, also, that these same catarrhal disturbances of summer are of more frequent occurrence under a less distinct form, namely, that of aggravation of affections which, in some degree, the sufferer considers as habitual, and almost natural to him. One cannot travel without incurring ophthalmia, another asthma. Many suffer in particular localities or seem to require peculiar circumstances to secure tolerable ease. The above considerations, and my own experience, make me conclude that none of the affections are positively and necessarily confined to any season, or such a specific cause as hay or ipecacuanha."

This writer evidently confounds ordinary catarrh and asthma with hay-fever, and if he suffered at all from the latter disorder he was labouring under a mistake in supposing that it could come on at any time of the year independent of any specific influence such as he names. There is one point however which Dr. King notices in his remarks, in another part of his paper which I have not quoted, in which, although contrary to the opinion generally entertained, his observations will, I think, be found to be very correct, so far, at any rate, as regards hay-fever. In speaking of the structures affected in this disease, he says:—

"I make some exception to the conclusions of Dr. Bos-

tock, that the air cells of the lungs are especially affected; and I prefer to set down the dyspnoea as the result of turgescence in the lining membranes of the air tubes."

So far as hay-asthma is concerned I hope to be able to show, further on, that this view of the case is more in accordance with facts than that which is generally held by the writers on hay-fever at the present time.

§ 38. Dr. G. T. Gream, who was also affected with the disorder, did not believe that it was owing to any strictly specific cause, and held that the farina of grass had "no more influence in causing the disease than that of any other flowers." He says:—

"The dust from beaten carpets, from the roads, and from other sources, produces the same distressing symptoms." And he further remarks, "I am led to think that in the middle of summer, from the end of May to the end of July, at which time hay-fever generally ceases, a quantity of fine dust floats in the atmosphere, finer than any which is in the air at other seasons, increased, probably, by the farina of the mass of flowers at that period in bloom, but that during the later and earlier months, the more frequent rains, and the dews at night, prevent these particles from leaving the ground, and I have been induced to suppose that this reasoning is correct by finding that however distressing the symptoms have been during the day, they are all entirely removed by a shower of rain: the face becomes cool; the irritation of the nostrils and the eyes ceases, and does not return until the heated atmosphere has again evaporated the fallen rain."*

Here, again, we shall find that although, in supporting his views, Dr. Gream is somewhat wanting in that exactness and precision which should characterise inquiries of this kind, he has hit upon one very important feature in the phenomena of hay-fever, namely, the influence which rain has in diminishing the intensity of the symptoms.

§ 39. Dr. Ramadge speaks of hay-asthma as a variety of ordinary asthma; but, in this form of the complaint, he

* "On the use of *Nux Vomica* as a Remedy in Hay Fever," by Dr. G. T. Gream. *Lancet*, 1850, vol. i, pp. 692—693.

regards the exciting cause as being more obvious than that of other forms of the disease, "inasmuch as we can refer to a sensible material object, the presence of which is known to produce it, and the removal of which is sure to be followed by its subsidence." After detailing the symptoms of the disease and giving examples of it, Dr. Ramadge goes on to say :—

"When we take these facts into consideration we need not feel sceptical as to effluvia from the flowers of grass, the odour of the bean flower, &c., being the occasional cause of the variety of asthma of which we are now treating."*

§ 40. Dr. Hastings, in referring to hay-fever, says :—
"The disease, in its severest form, is common in June about the period of hay-making, and there can be no doubt but that in some peculiarly constituted persons, an emanation from hay, the exact nature of which is unknown, occasions an attack of the disorder."†

§ 41. Dr. W. P. Kirkman seems, so far as I can learn, to have been the first patient who has tested, by an experiment upon himself, one of the supposed causes of hay-fever—the pollen of grass.‡ He tells us that a day or two before Christmas he noticed, in his hot-house for flowers, one single plant of the *Anthoxanthum odoratum* in blossom, loaded well with pollen. He thought it would be a capital opportunity for trying this particular grass, so he plucked it, rubbed the pollen with his hand and sniffed it up his nose; almost immediately it brought on sneezing, &c., and all the symptoms of hay-fever, which continued for an hour and then left him.

§ 42. Dr. (now Sir Thomas) Watson mentions this disorder in his lectures,§ and quotes the testimony of Bostock, Gordon, and Elliotson. He also gives accounts of some

* *Asthma, its Varieties and Complications, or Researches into the Pathology of Disordered Respiration; with Remarks on the Treatment Applicable to each Variety*, by F. H. Ramadge, M.D. London, 1847.

† *Treatise on Diseases of the Larynx and Trachea, &c.*, by John Hastings, M.D. Edin.

‡ Quoted from Dr. Phœbus' *Typische Frühsommer-Katarrh*, p. 137.

§ *Lectures on the Principles and Practice of Physic*, by Dr. Thomas Watson. London, 1857, vol. ii, pp. 52—56.

interesting cases of hay-fever which have come under his own notice, but does not enter into any lengthened consideration of the causes of the disease. In his concluding remarks, however, after mentioning that the powder of *Ipecacuanha* produces similar symptoms to those of hay-fever in some persons, he says, "these effects of a powdered root and certain emanations from grass or hay lend weight to the hypothesis which ascribes the influenza to subtle vegetable matter floating in the atmosphere."

§ 43. Dr. Walshe, in speaking of hay-fever, says :

"A singular variety of naso-pulmonary catarrh, which has been supposed to follow the inhalation of the aroma of the sweet-smelling spring grass and hay (*Anthoxanthum odoratum*), is known under the name of hay-asthma, hay-fever, or summer catarrh. The complaint occurs only at the periods of hay-making, or when the odour of grass is powerful, and is of exceedingly rare occurrence. The susceptibility to these emanations, indeed, constitutes a very remarkable example of unalterable idiosyncrasy. Persons who have once suffered invariably have a return of the disease, if exposed even in a slight degree to the specific cause.

. The most effectual means the habitual sufferer can command of preventing the attack, is by removing at the season to the sea-side, by getting out of the reach of the odours of grass and hay. But so exquisitely sensitive to such sensations are some individuals that a land wind, blowing for a few hours only, will bring on an attack even at the sea-shore. Once the complaint is established, total abstraction of the exciting cause will not put an immediate termination to the seizure. I have had a very precise narrative of a case in which the patient retained his symptoms during a passage across the Atlantic."*

§ 44. Dr. Hyde Salter, when treating of the annual periodicity of asthma,† remarks that asthma occurring once a year is almost always winter asthma, "There

* *A Practical Treatise on the Diseases of the Lungs, Heart, and Aorta*, by W. H. Walshe, M.D. London, 1854.

† *On Asthma: its Pathology and Treatment*, by Henry Hyde Salter, M.D., F.R.S. London, 1859.

is, however," he says, "one kind of annual asthma that is not a winter asthma, but a summer asthma; and that is that curious disease called hay-fever or hay-asthma. This begins and ends with the hay-season, and varies in the time of the year, according as the hay season is early or late. As long as the grass is in flower it persists, with that it ceases. Its visits are, therefore, restricted to about a month or six weeks in the early summer. It is not constant throughout this time as one attack, but comes and goes with those other symptoms of irritation of the respiratory mucous membrane, of which it is a part. The neighbourhood of hay, bright, hot, dusty sunshine, a full meal, laughter, &c., suffice at any time to bring it on. It often affects a sort of diurnal rhythm, being generally worse at night. While this condition lasts the asthma is often so severe as to deprive the sufferer of sleep for nights together, and he leaves his bed in the morning, pallid, blear-eyed, and worn out. When the hay season is over every symptom vanishes, and for ten or eleven months the patient may calculate on a perfect immunity from even the slightest asthmatic sensation."

Amongst the examples which Dr. Salter gives are the narratives of two cases which are interesting as well as instructive. The patients each tell their own tale, and this they do, in each case, in a very clear and graphic manner.

§ 45. The first case is what we should call a typical case of hay-fever, or what Dr. Phœbus would say, was an example of the "whole disease;" that is to say, of both the catarrhal and asthmatic form of the disorder. This case is instructive in the way in which it shows us that the catarrhal form of the disease may set in and continue to attack the patient for some years, and afterwards be followed by the more troublesome phase of the malady in the shape of asthma.

In this case the patient tells us that the attacks first came on when he was about eight years of age, and in speaking of their commencement he says:

"I well remember the first attack of those symptoms which, now more developed and more regular in their

appearance, I recognise as my annual hay-fever torment.
. I was at the play-work of haymaking with my young companions, surrounded by newly mown grass, when I was suddenly seized with all the eye and nose symptoms of hay-fever—profuse lachrymation, swelling of the conjunctivæ and lids, with intense ecchymosis, well nigh blinding me, and ceaseless sneezing.

“I recollect that I was taken into the house by my elder companions, and speedily recovered.

“It was, however, about the fifteenth year of my age before I was conscious of my annual infirmity—before I understood that at every early summer I was liable to sneezing fits if I ventured into the country; but from that time to the present this tendency has been abiding, has manifested itself every year, and has always governed my habits and residence during the month of June, and part of May and July. I am now usually first attacked by sneezing and lachrymation about the middle of May, though this is much determined by the nature of the season; the warmer the weather, and the more advanced the vegetation, the earlier does my malady show itself. It usually lasts till the end of the first week in July (when it leaves me very suddenly), though this also is determined by the rapidity and shortness of the haymaking season; for in a hot, dry season, in which the hay is rapidly made and carried, my immunity from trouble occurs a week or ten days earlier.”

§ 46. The second case shows that hay-fever came on after the patient had suffered from ordinary asthma for many years, or that it must have been an accompaniment of the latter disease, without the patient having been aware of it. In commencing to describe the hay-fever symptoms the patient says:

“It seems reasonable to suppose that I must have been liable to hay-fever, at the ordinary season, during the whole course of my life, but till within the last few years I was never aware of its presence, or of the existence of such a malady. From the frequency of my asthma, and common colds in early life, it is probable that the recurrence of asthma at a particular season, and the other symptoms of

hay-fever, were overlooked; and that when I became less generally subject to asthma, the tendency to hay-fever remaining, that complaint more distinctly declared itself; or it may be that of late years I have become constitutionally liable to hay-fever—either more susceptible of the influence, whatever it may be, or have acquired a constitution capable of evolving its symptoms. . . . I have suffered most from paroxysms whilst taking country walks, walking through grass meadows, and especially in one particular garden surrounded by fields. The prevalence of the influence in this locality is very remarkable, as there is nothing peculiar in the neighbouring soil or its products. I know one other locality where the influence is still more excessive; here there is an abundance of flowering grass and rushes, the region is flat, the soil marshy, and in the neighbourhood there is a great variety of indigenous vegetation. If the influence arises from the grass, it is not necessary it should be cut and dried, that is to say, the presence of hay is not essential.”

§ 47. In the early part of the year 1859 Dr. Phœbus (Professor of Medicine at the University of Giessen), who had previously taken up the special study of hay-fever, sent out a circular, which was published in various medical journals in this country and on the continent. The object of the author, in sending out this circular, was to obtain contributions on the pathology and therapeutics of the disease, and to extend the knowledge of its literature. Information was also sought on the following subjects:

1st. On the geographical distribution of the disease.

2nd. On the ethnographical distribution of the malady (that is to say, as to whether it affected natives or foreigners most in countries where it prevails).

3rd. On the influence of sex in predisposing to attacks of the disorder.

4th. On the effect of social position and education in producing a liability to the malady, and on the frequency or non-frequency of its occurrence among the working classes.

5th. As to whether the persons predisposed to attacks of this disease are distinguished by any marked peculiarities

which make this predisposition at once manifest ; and, in such a case, whether any such escape the attacks at any time.

6th. Whether any difference, such as is named above, is to be found in members of the same family, where one of the family may be predisposed to attacks of hay-fever, and other members not predisposed.

7th. Whether it always occurs, in those who suffer from it, at one particular time of the year, or at various periods of the year.

In the preliminary remarks to the questions, of which only a summary is given above, Dr. Phœbus draws attention to the leading features of the disease, and particularly to the circumstance of its showing itself *exactly* with the first heats of summer. In this latter observation there is, if not a little tendency to prejudge the case, at least an evident leaning to the theory of causation which the learned author has since adopted. Of this I shall have more to say hereafter.

The circular of Dr. Phœbus called forth a number of responses, and resulted in the accumulation of a mass of very valuable information on the subject. Several medical men on the continent, as well as several in America and in this country, sent contributions to the various medical journals, and to Dr. Phœbus, in answer to the inquiries made in the circular.

§ 48. Dr. Cornaz, of Neuchatel, Switzerland, has published an interesting paper* on hay-fever, in which he records the histories of six cases with which he had become acquainted. After giving the particulars of these, the author discusses the question of nomenclature for this disease. On the whole he prefers the name "catarrh," because in all the cases which he describes there was coryza, accompanied by catarrhal symptoms. The author also prefers the denomination "de foin" (of hay) to that of "d'été" (of summer), and the reason given for this preference is, that in each of the six cases, the flower of grass seems to be the agent

* "De l'Existence du Catarrhe des foin en Suisse," *L'Echo Medical*, No. 7, July, 1860.

which brings on the attacks. At the same time, however, Dr. Cornaz affirms that hay, when being gathered, will bring on the disorder, although to a less degree than the flower of grass.

The last-mentioned of these terms (d'été) Dr. Cornaz thinks might, in some respects, be considered an appropriate name, inasmuch as it has the very important advantage of not appearing to prejudge the cause of the ailment; but this advantage is, to some extent, counterbalanced by the circumstance that the name "summer catarrh" (catarrh d'été) is given to a disease which often sets in before summer has fairly commenced. On the other hand it is admitted that the term "de foin" is also open to objection, inasmuch as it is applied to a disorder which is known to lessen in severity, if not altogether to disappear, when grass has been converted into hay; and also because it is well known that, in some cases at least, the pollen of cereals has the same influence in bringing on the symptoms.

§ 49. Dr Longueville,* who wrote an account of his own case in answer to the inquiries of Dr. Phœbus, tells us that he suffered from attacks of ordinary asthma, and says he also found that proximity to hay was sure to bring on similar attacks, but he did not consider that in the symptoms which, in his own case, followed those attacks which were caused by hay, there was anything which would warrant the designation "fever."

§ 50. Dr. Labosse, of Nitry, writes an article† in reply to the observations of Dr. Longueville, and takes the latter somewhat sharply to task for his statement "that there is not anything which should be called 'fever' in the symptoms brought on by hay." The former gentleman says we may have the feverish symptoms well characterised in hay-asthma, and he believes that the attacks of asthma from which Dr. Longueville suffered were widely different, in their

* Of France, but of what city or town I have not been able to ascertain.—
C. H. B.

† "Nouvelle Observation de Catarrhe de Foin."—*L'Abeille Médicale*,
August 20, 1860, p. 270.

symptoms, from true hay-fever or hay-asthma. Dr. Labosse had seen three persons who were annually attacked by this disorder, and in each case the symptoms were cough, dyspnœa, coryza, and injection of the conjunctivæ ; and these began to come on precisely at the period when the natural or artificial meadows were in blossom, lasted as long as the time of blossoming, and returned periodically, every year, in those individuals who were subject to the disease.

In these observations we have the influence of the period of blossoming clearly pointed out, and it seems that in the case of the patient (a farmer) whose symptoms are here given, the influence of heat had never been supposed by him to have much to do with the occurrence of the attacks. It is also especially worthy of notice that whenever the *saint-foin* (the herb with which he fed his sheep) had gone beyond the period of flowering, to the ripening of the seed, the patient could handle it without bringing on any unpleasant feelings, or any of the symptoms of hay-fever.

§ 51. Dr. Laforgue, of Toulouse, in a paper which was also published in answer to the inquiries contained in the circular of Dr. Phœbus, gives an account of two cases of hay-fever which had come under his own notice.† In one of these cases the patient is said to have always enjoyed excellent health during the cold weather, with the exception of an occasional attack of coryza, which, however, was never attended with any difficulty of breathing, or any of the symptoms of asthma. As soon, however, as the warm weather set in, she always began to suffer from coryza, and after a short time her breathing would become impeded, and as long as the warm weather continued she suffered severely from both the catarrhal and asthmatic form of hay-fever ; and, in spite of all the means used to prevent these attacks, or to moderate their severity when once developed, the malady returned regularly every summer.

Dr. Laforgue thought that in this case heat was the exciting cause, and in concluding his remarks on this patient, he says :

* "Observation de Catarrhe d'été," par M. le Docteur Laforgue de Toulouse. *L'Union Médicale*, No. 149, 17th December, 1859.

“The great heat of the last summer strongly affected Mdle. X—; beginning with coryza, the cold (rhumes) has become such intense spasmodic bronchitis, that the dyspnœa has for several times shown a threatening character.

. This observation, methinks, enters well into the category of the facts collected by Dr. Phœbus, of Giessen. It furnishes a good example of those catarrhal affections which, developed under the acting influence of heat, present all the symptomological characters of asthma.”

§ 52. In an anonymous paper published in one of the French medical journals* the author of the article, in describing his own case, expresses a very decided opinion on the cause of the attacks. In speaking of the effects of heat he says:—

“The heat has no *extraordinary* effect upon me; I suffer from it as anybody else does, but I do not feel any symptoms which may remind me of hay-fever. The hay being safely stored away, my pocket-handkerchiefs remain undisturbed until the following year; they only serve me for wiping my forehead during the heat of summer. After the hay is gathered in, the heat or atmospheric changes do not bring on, afterwards, any attack of coryza. Having the head only very lightly covered, the heat, however excessive it may be, does not trouble me any more than it does other people. But when hay-making is going on, at a time when it is much less warm than in August, I suffer in the most severe manner.

“I do not believe in an atmospheric cause or coincidence in regard to hay-fever attacks. I have collected many proofs respecting the effect of hay on my head in different countries which I have visited.”

§ 53. Dr. Phœbus, to whom we are indebted for bringing together and putting into an available form all that was known of hay-fever up to the time he wrote, has pursued the inquiry into the causes of the disease with a care and minuteness which is quite characteristic of the mode in which the German mind deals with obscure and little known

* “Un dernier mot sur la Fièvre de Foin,” *L'Abeille Médicale*, May 21st, 1860.

subjects. No phase of this curious disorder has escaped his scrutiny, and such has been the result of his labours that we may say with justice that we owe to him the creation of a considerable portion of the literature of hay-fever.

This question of cause is confessedly one of the most difficult parts of the subject with which we have to deal, but whilst it is desirable that we should enter into every phase of it with the greatest care and circumspection, it is nevertheless true that the process of refinement and subdivision may be carried too far, and in our anxiety to get hold of and to examine minutely every possible cause of the disorder we may fail to recognise the true cause, and may spend our time in the examination of those things which have no relation whatever to the malady, and which a more simple and logical method of viewing would have put out of court altogether. If we have any fault to find with the way in which Dr. Phœbus has done his work, it is in the circumstance of the over-refinement which characterises that work, and the want of a more rigid process of elimination in considering its causes. An example of this latter fault is to be found in the reference which has been made to ozone as a cause of hay-fever, by Dr. Phœbus. Although this substance has been supposed to be a possible cause of the malady, it is well known to all who have paid any attention to its treatment that no method of cure or prevention is more successful than that of sending the patient to the very spot where ozone is most abundant and most constantly to be found, namely, to the sea-shore.* The great amount of time and labour, however, which Professor Phœbus has expended upon the investigation of this disease entitles his opinions to be received with respectful attention. At the same time, however, the conclusions he has arrived at

* Lest it may be thought that I have dismissed this part of the subject in a somewhat summary manner, I may here say that I have not neglected to make a short course of experiments with ozone which will, I think, show conclusively—if any proofs were needed—that this body has no unfavourable action on hay-fever patients; or, at least, that it does not produce any of the symptoms of hay-fever. It is, however, only right to say that I should not, for the reasons I have given above, have thought it needful to enter into any experiments with ozone if I had not had other investigations in hand respecting it.

demand a careful examination, because as he has embraced a wider range of causes of the malady than other authors who had written before him, he has, as a consequence of this, increased in a corresponding ratio the possible sources of error; and more especially because subsequent writers seem to have been largely influenced by the views he holds.*

§ 54. Like all other writers, Dr. Phœbus makes predisposition to be the starting point of the disorder, but in what part of the system this predisposition has its seat he does not attempt to decide; and he thinks that between those who exhibit a liability to its attacks and those who are free from such liability, no well-marked line of separation exists. Almost all temperaments and all states of the system, from feeble to robust health, are to be found amongst hay-fever patients. Curiously enough, however, Dr. Phœbus suggests that the predisposition may not be present during the whole year, but may "repeat itself annually at the season of the attack," and that it may possibly be produced in some unknown manner by the action of one or more of the exciting causes. He also thinks it possible that the disease may be present in a latent state before it first shows itself, and consequently there is no necessity for considering the first attack as the commencement of "the whole disorder." For the same reason we may imagine the disease to be present in a latent state during the intervals in which the patient is free from any visible signs of it; and it may also be further supposed that variations in the conditions of the separate organs may possibly be a cause of the attacks having a somewhat intermittent character—occurring on one day and not on another—during the season when the disease prevails.

These views imply a possibility of the predisposition having been in some cases essentially of a temporary nature, but this is believed to be warranted by the analogy which exists in other diseases of a nervous character; but, as the author (Dr. Phœbus) remarks, "in no other disease in so constant and logical a form as in hay-fever." One thing,

* See his *Typische Frühsommer Katarrh*.

however, is considered certain, namely, that when once a patient has had an attack, this leaves a condition of the system which, to a certainty, leads to others.

It will be seen that these opinions of the modes in which the predisposition may vary in its manifestations are somewhat complicated, and also a little conflicting, but as some of the facts I shall have to communicate have an important bearing upon the opinions here advanced, I shall not enter into any lengthened consideration of them at present.

§ 55. Circumstances of a geographical and ethnographical nature seem to have a considerable influence in determining the relative number of cases which may be met with in natives of different countries. In some countries the disease is never seen among the natives; in others it is very rare. In respect to the comparative frequency with which the disorder is met with in the different countries of Europe, England stands at the head of the list. Germany comes next; having less than half the number as compared with England. France, Belgium, Switzerland, Scotland, Italy, Russia, and Ireland, follow in the order in which they stand. North America is said to have very few cases occurring in it, but this will probably be found to be an error. The disease is said by some to occur frequently in that country under the name of "rose cold" or "rose-fever." According to a table which Dr. Phœbus has prepared, those who are of Anglo-Saxon parentage form by far the largest proportion of the whole number affected, whilst those who stand next on the list are the most nearly related to the former ethnologically. Out of a total number of 152 patients whose parentage was ascertained, 81 were found to have been born of English parents (natives of England proper), whilst of the remainder, 36 were found to have been born of German parents, leaving only 35 whose parents were natives of other countries. It is a curious fact, too, that we have only one patient who was born of Irish parents; thus showing that race seems to have a more potent influence in producing a predisposition to the disease than mere geographical position. But it will probably be found, when the geographical distribution of the disease comes to be

more fully investigated, that a change may be produced which will somewhat alter the proportions here given, but not to such an extent as to affect the principle here enunciated.

§ 56. Professor Phœbus places the exciting causes under three heads : viz., 1st. Causes of each single attack. 2nd. Causes of exacerbations. 3rd. Causes of the variations of the groups and of other variations.*

It is with the first and second of these I shall have principally to deal, and if we succeed in demonstrating the cause of each single attack, in any one year, we shall have made some advance towards discovering the second cause named, and probably there will not be much difficulty in showing that the causes of each single attack and of the exacerbations are, with very rare exceptions, one and the same.

The third cause is more complicated and will not be so easily cleared up until more research has been devoted to it than has yet been expended upon it ; indeed, in the present state of our knowledge, it is almost waste of time to speculate on the *causes* of these differences. Our first effort in studying this and also other diseases, of a more complicated character, should be to get a knowledge of the nature of the primary exciting cause, and of all the modes in which it may affect the organs upon which it acts.

The *causes* of the mode in which the agent acts, and the reason why it sometimes acts upon one organ and sometimes upon another, may for ever remain hidden from us.

In considering the evidence which has been brought in favour of the various substances which have been already noticed as being the active agents in producing hay-fever, Dr. Phœbus draws attention to the fact that some authors and some patients accuse one agent and some another ; but no one, he says, " has accused these agents *en masse* ;" and, although some experiments have been tried with one or

* Dr. Phœbus has also divided the symptoms of the disease into groups, according to the part principally affected, viz. into Nose Group, Eye Group, Throat Group, Chest Group, &c. ; of these divisions I shall have something to say further on.

another of these, no one has yet pursued a systematic course of experiments which, on the one hand, would prove that the presence of any given noxious agent (or supposed exciting cause) was always followed by an attack of the disorder; and, on the other hand, that the absence of this same agent has always given complete freedom from the attacks. Another source of error, Dr. Phœbus thinks, lies in the circumstance that "as soon as one of these noxious agents has caused its effect, the symptoms of the intensity of the disorder have shown themselves immediately," and it has almost always been overlooked that the symptoms of "the stage of development" had already preceded these, and the patients have thus taken the more complete form of the disease for the beginning of it. "If people in future," he says, "will be more attentive and especially observe more than one patient carefully, the author does not doubt that the accusation against these noxious agents will, in greater part, fall to the ground."

§ 57. Whilst recognising the fact that the attacks of hay-fever occur during the time that the greatest number of the natural and artificial grasses are in bloom, and that the duration of the disease is synchronous with the period of flowering, Dr. Phœbus thinks that the proofs which some authors have brought have shown "that this or that noxious agent of the grasses is not the only cause of the access, obvious in all cases," and with regard to these he says:—

"Such proofs were formerly very desirable but have now lost much in value, for we now know that more than one of these noxious agents is accused, with apparently good reasons, whilst *the first heat of summer*, however, is a stronger cause than all the grass emanations put together." He then goes on to say:—"I have already shown that the supposition that the attack begins with the first heat of summer, and that its occurrence depends on the latter, is supported by numerous and important authorities far more than the assertions which give the following as the causes: A, the common grass blossom; B, the beginning of grass being generally in flower; C, any blossoming grass not in

too small a quantity; D, the first blossom of the sweet-scented spring or vernal grass; E, the blossom of rye; F, hay; G, roses in bloom; H, pollen of all blossoms; I, dust in general."

In concluding his observations on this part of the subject Dr. Phœbus says:—

"From what is said in the preceding paragraphs, I think myself justified in drawing the following conclusions: 1st. Nobody has yet succeeded in showing with certainty the exciting causes of each single attack. 2nd. With probability the following circumstances (momenta) may be accepted as such causes: A, the first heat of summer (which, however, only acts in an indirect manner as an exciting cause); B, the longer days (which act, perhaps, through the stronger influence of light, or, perhaps, also through ozone); C, the same (or nearly the same) odours and different kinds of dust which we positively know as causes of exacerbations of the attacks. Amongst these hay and the blossom of rye have the greatest probability in their favour. 3rd. It may be *possible* that one of these causes is the only true one, and that the facts in regard to the other causes have been falsely understood. It is more probable, however, and the after access especially speaks in favour of the supposition, that all these influences act as exciting causes of the attacks.

"Some patients might be susceptible to only a part of these influences.

"I do not doubt that future and more accurate observations, especially comparisons of the phenomena of the disease with the meteorology and vegetable phenomena, will bring certainty instead of probability and possibility merely."

§ 58. The English medical authors, who have written on hay-fever since the work of Dr. Phœbus was published, have, to a considerable degree, followed his teaching and, with some modifications, have arrived at much the same conclusions with regard to the nature of the causes; but, in some cases, they have gone farther, and have, perhaps, expressed

the opinions he holds in a much more decided manner than he himself has expressed them.

§ 59. Dr. Abbott Smith, whose work* has passed through four editions, gives numerous cases in which the exciting cause of the attack seems to have been the emanations from grass and other flowering plants.

He also states "that strong light as well as great heat will induce or aggravate the symptoms," but he does not believe that the ozone theory of Dr. Phœbus is sufficient to account for the attacks, or the exacerbations of hay-fever.

Dr. Smith mentions that M. Vogel, many years since, ascertained that benzoic acid exists in two of the grasses, which, by some authors, are considered to be the most important agents in the production of hay-fever, namely, the *Anthoxanthum odoratum* and the *Holcus odoratus*, and he suggests the question "whether hay-fever may not, in some degree, especially when it arises in persons who are affected by the aroma of grass or hay, be attributed to the irritating effects of benzoic acid, which is liberated from the above-named grasses by the agency of summer heat."

In support of this idea he says, "it may be observed that the attacks of hay-fever are almost invariably worse during the continuance of hot, dry weather, while they generally assume a milder character in wet weather, or when the temperature is much reduced; at these latter periods the sublimation of the benzoic acid, contained in the flowers, would be less than in hot weather."

Dr. Smith states that he has been informed by Messrs. Davis, Macmurdo & Co., manufacturing chemists, "that the inhalation of the vapour which incidentally escapes during the sublimation of benzoic acid, causes considerable irritation of the throat and violent paroxysms of sneezing and coughing."

One case is given by Dr. Smith, which he thinks "bears upon the fact that the smell of decomposing vegetable matter is sometimes the cause of this affection." The patient says "I was occupied in adding fresh water to some

* *On Hay-Fever, Hay-Asthma, or Summer Catarrh*, by Wm. Abbotts Smith, M.D. 4th edition. London, 1866.

flowers in a vase in which the water had been standing several days, and was foul, and as I poured it away my annual visitor came on."

In reference to this case I have to remark that it is surprising how easily facts may be misunderstood and wrong inferences drawn from them. It was a similar incident which occurred to myself, some years ago, which first drew my attention to the real nature of the cause of the malady. Of this I shall have to speak further on.

Another case is given by Dr. Smith as an example of the effects of heat. The patient had resided for seven years at the sea-side and, from his experience whilst residing there, considered that his case quite agreed with that of Dr. Bostock, in not being dependent upon the smell of hay "but merely on the approach of really hot weather. This year (1865)" he says, "the disease first came on whilst I was on the sea yachting with a friend. It was a hot day in May, with the wind from the south-west, the nearest land to windward being nine miles distant. I felt myself, after some exertion in assisting to hoist the sails, suddenly seized with sneezing and I have had it ever since." (The date of the letter was June 13th.)

After noticing the causes spoken of by Dr. Phœbus, Dr. Smith says, "each of the principal causes just enumerated has, doubtless, much to do with the causation of summer catarrh; and, as in all other affections, sometimes one, sometimes another, cause may preponderate;" but he afterwards adds, "the majority of sufferers from this disorder attribute their illness to the presence of ripe grass or hay in their immediate neighbourhood."

§ 60. Dr. Pirrie* thinks that there are two distinct forms of the disease. He notices the circumstance that the emanations from ripe grass and some other plants, in flower, are recognised as the grand causes of both the catarrhal and the asthmatic form of the disease, but he says that cases have come under his own notice where the sufferers have attributed their indisposition "to solar heat

* *On Hay-Asthma and the Affection termed Hay-Fever*, by Wm. Pirrie, M.D. London, 1867.

and intensity of light” and his own observations lead “him to conclude that sufficient importance has not been attached to their opinions on this point.” One form of the disorder Dr. Pirrie regards as spasmodic in character, exhibiting its effects on the mucous membranes of the respiratory tract, and as being caused by subtle volatile emanations from flowering plants acting upon the nervous filaments distributed to these membranes. In some cases, he thinks the disorder may also arise from irritation of the filaments of other nerves, or it may be caused by a “primarily tumid and swollen state of the lining membranes” (of the air-passages) such as heralds in attacks of measles or common catarrh. In the other form of the illness it is thought that the cause does not operate directly upon the mucous membranes, or upon the nervous filaments distributed to these, “but on certain nerve centres of the cerebro-spinal and sympathetic system.”

In the first form of the disease, Dr. Pirrie says, a change of residence and the use of other remedial measures are mostly followed by speedy relief. In both forms of the illness we may have severe catarrhal and pectoral symptoms, but in the latter “the vascular relaxation and the associated nervous paresis are the results of the debilitating effects of great solar heat, assisted in many cases by intense light, on the cerebro-spinal and sympathetic systems, of certain peculiarly constituted people.”

Dr. Pirrie gives cases which seem to favour the idea that in these the ailment was due to the action of solar heat and light. Into the consideration of these I shall not enter at present.

§ 61. Dr. George Moore, whose monograph* on hay-fever is the latest treatise with which I am acquainted, holds much the same opinions of the symptoms and causes of the disorder as those held by the two last writers I have mentioned. He, however, expresses his opinions in a little more decided manner than these writers have expressed theirs; or, to put it in a more definite form, everything, or almost every-

* *Hay-Fever, or Summer Catarrh : its Causes, Symptoms, Prevention, and Treatment*, by George, Moore, M.D. London, 1870.

thing in relation to this disorder is a settled thing. The causes are with him clear and definite, and the symptoms are placed before the reader in a precise and exact manner.

Like the writers above mentioned, Dr. Moore believes the disorder is, in some cases, caused by solar heat and light ; in others by the effluvia from hay or by the emanations from other flowering plants and from those of decaying vegetable substances.

Dr. Moore quotes Dr. Bostock's case as an example of that form of the disease which is caused by great heat and strong light, and he tells us that, in these cases, " sea-voyaging, out of the reach of putrescent and other effluvia, affords no protection or exemption." He, however, does not give us the particulars of any cases of the disorder which he has himself had the opportunity of watching, or of which he has obtained the histories. It is to be hoped that in a future edition of his brochure Dr. Moore will be able to supply the details of cases with which he has become acquainted, and particularly of such as have any bearing upon the question of cause.

§ 62. Another supposed cause of hay-fever is alluded to by Dr. Gull in his Harveian oration delivered at the Royal College of Physicians, June 24th, 1870. It was stated by a writer* on hay-fever that vibriones had been found in the nasal mucus of patients suffering from this disease. In reference to this supposed action of vibriones Dr. Gull says :—

" No new fact bearing on the propagation of contagious disease has been reached by the recent investigations on dust ; nor can we infer the nature of summer catarrh because the nasal mucus, under such circumstances, and at no other time, was found peopled by vibriones, since decomposing mucus is always populous with this common race of infusoria."†

* I cannot now call to mind the name of this author. [Helmholtz.—Eds.]

† I have examined the nasal mucus frequently in the early stages of hay-fever, but have never seen any sign of vibriones or any other infusoria. I have frequently seen minute bodies having distinct molecular motion, but this motion is not so vigorous or so extensive as that of most of the infusoria. I shall be able to show further on that some of these molecules are probably derived from the atmosphere.—(C. H. B.)

§ 63. I shall now give extracts from the history of cases I have myself seen or which I have had communicated to me. One of these patients I have had under my care annually, since the commencement of the disorder—four years ago; and another of the patients who has suffered for about twenty-four years I have had under observation for about ten years. The other patients named have not been under my care at any time.

§ 64. Patient 1.—A military officer who has spent some years in India. In answer to my inquiries in reference to his case he says:—

“I was in India for some years, and during that time I had no hay-fever whilst in the plains; but one season I took an excursion into the Himalaya Mountains, about the month of June, and I found that on many days when I was in parts of the hills, which from their elevation correspond with the heat and climate of England, and where crops were growing somewhat of the same nature as European cereals, I had violent attacks of hay-fever, although no grass was out for hay-making and the grain crops were nearly ripe. The cultivation, however, was very scanty and partial—small patches levelled in the hills about the native villages—so that I was more inclined to attribute the attack to the temperature than to the cultivation. Long grass, however, was growing in places on the hills.”

§ 65. Patient 2.—The wife of a military officer, residing in the South of England. In this case the patient says:—

“The attacks always begin some time in May and occasionally continue until September, but in London they have ceased about the middle or end of August, and they certainly seem to follow the growth of the grass, but roses affect me so severely that if I gather them a very severe attack instantly supervenes, worse than from any other flower. The attacks are very severe in a hay-field during haymaking, and the illness does not seem to cease with the haymaking season, but the climax of suffering hitherto, has been from the middle of June to about the middle of July.”

§ 66. Patient 3.—Sir ——, Bart., in speaking of the exciting causes of the attacks in his case, says :—

“The attacks generally begin about the 4th of June and cease about the second week in July. In wet weather I seldom or never suffer. The hotter the weather (particularly if there is no wind) the worse I am. I am quite certain that, in my case, hay-fever is caused by the minute particles which come from, not only grass but, flowers and trees of all sorts.”

§ 67. Patient 4.—In this case the patient is a medical man holding the rank of Surgeon-Major in the British Army. Having spent many years in India and being well acquainted with the climate, his testimony is, on this account, very valuable. In answer to my inquiries about his experience of the disease in India and in England, he says :—

“I have suffered from hay-fever for about thirty-five years. I have had it both in India and in England. The period at which the attacks come on is not fixed, the date of the attack depending more on the grass ripening late or early than on any other circumstance. They always begin towards the end of the hay season when the grass is fully in flower, and cease slowly and gradually—not directly—on gathering in the grass. In India the attacks come on after the rains, about August or September.

“Changes of atmospheric temperature do not increase or decrease the severity of the symptoms ; I have been attacked as severely in the cool climate of Simla as in the heat of the plains. At sea I have escaped the attacks, and also at some northern stations in India—at Kurrachee for instance.”

§ 68. Patient 5.—A lady residing in one of the midland counties sends me the following particulars of her case :—

“I have suffered from hay-fever twelve or fourteen years. The attacks generally commence some time in May, but they come on earlier in a warm season than a cold one, and they are sure to come on with the first scent of spring flowers. May blossom, or a bean field in bloom, is as trying as a hay field, and the Elder flower is the worst of all. The attacks sometimes cease before the hay is all gathered in ; a

cool grey day restores to temporary health, whilst heat and sunshine cause great suffering, but the symptoms are less severe after rain unless the weather is very close."

§ 69. Patient 6.—A young lady, æt. 22, residing in a suburb of Manchester. In this case the disease first came on four years ago. As far as she could observe there had been no change in the constitutional tendencies or in the habits. Each year the attack has commenced at the time the grass has begun to come fully into flower, and as long as the patient has remained under the influence of the emanations from flowering grass the attacks have continued. Each year, however, since the disease commenced the patient has, a few days after the attack has shown itself, removed to the sea-side. On every occasion this change of locality has brought relief in a few hours, and in the course of twenty-four or thirty-six hours the patient has described herself to be, what she considered, almost well.

On one occasion Blackpool, on the Lancashire coast, was selected as a place of residence during the usual period of the attack. On two other occasions, I believe Llandudno in North Wales was the place resorted to.

§ 70. Patient 7.—A lady, æt. 53, sister to a clergyman of the Church of England. This patient has suffered from the disease for about twenty-four years. When the attacks first came on she resided near Sheffield, but during the last ten years she has resided near Manchester. As far as the patient can now remember she suffered from both the catarrhal and asthmatic form of the disease at the commencement, but of late years the asthmatic form of the complaint has been the most marked.

The attack generally begins, in a mild way, about the latter end of May. This goes on increasing in severity up to the middle or latter end of June, and from this time up to the middle of July the symptoms are somewhat severe; they then gradually decline and by the time the second or third week in August has arrived she is free from the disease. The disorder has attained its maximum severity generally about the middle of July—sometimes earlier and occasionally later.

Although grass in flower appears to be the most frequent cause of the attacks the patient has thought that flowers having a strong odour have brought on the symptoms.

On one occasion, when on a visit to a relative who resided near Bradford, in Yorkshire, she was out walking in a meadow where grass in flower was being mown, about the latter end of the month of May. She had not proceeded far when an asthmatic attack came on and she found it necessary to leave the neighbourhood as soon as possible.

Atmospheric changes of temperature do not increase or decrease the severity of the symptoms. She has sometimes felt as well in the latter part of August, when the weather has been excessively hot, as she has been at any part of the year (so far as hay-fever is concerned). She has also often found that a room with the windows closed, and the heat greatly increased by this means, has been much less injurious than it has been when the windows have been opened and the room cooled.

Rain always mitigates the severity of the attacks.

§ 71. Patient 8 (the author's own case).—I have, as I have previously said, suffered from hay-fever for more than twenty years, but the exact time at which the disorder first commenced I cannot now remember. The attacks at first lasted only a few days, and then declined rapidly; and they seemed then, to me, to be in some way dependent upon the commencement of warm weather. For several of the earlier years the attacks came on about the middle or latter end of June, but I noticed that a cold season would delay the time for a week or ten days. Up to the present time the disease has only taken on the catarrhal form with me, but I have once or twice artificially brought on slight asthmatic attacks. From the circumstance of my noticing that the advent of the disorder seemed always to occur when the heat began to be such as warranted the designation "summer weather," and particularly from the fact that a walk into the country on a hot sunny day, whilst the attack of hay-fever was on me, was invariably attended by a great increase of the severity of the symptoms, I was inclined to regard heat as the principal cause of the disease. After hearing of Bostock's

case I was still more inclined to take this view of the cause, but circumstances which occurred subsequently considerably altered my opinions.

In the year 1857 I had occasion to go down to the sea-side* for a day or two. The hay had been nearly all gathered in in the neighbourhood of Manchester, and I was, as a consequence, just beginning to feel free from my usual summer illness. When I had got within the distance of six or eight miles from the sea shore, I felt that my old enemy was coming on again, and before three hours had elapsed I was suffering as severely as I had done during any part of the attack I was just recovering from. The disorder did not at all abate for the time I then remained (two days). The heat was certainly not greater than it had been in Manchester.† I returned home at the end of the time named, and was not a little surprised to find that, from the time I reached Manchester, my hay-fever rapidly disappeared.

In about five days I made another journey to the same part of the sea coast, and when about the same distance from the sea shore, that I was when the attacks came on on the former journey, I began again to have all the characteristic symptoms of hay-fever; but, strange to say, when I got to my journey's end these again quickly disappeared, and I was not troubled again during my stay of seven or eight days.

I was considerably puzzled with the very erratic manner in which the disease had come and gone after the usual period of the attack was over; but in thinking the matter over, I remembered noticing that, at my first journey, the hay grass for some miles inland was uncut, and also that much of it was in flower. Another concurrence of circumstances also impressed me much at the time, and helped very greatly to alter my views with regard to the action of

* Blackpool, on the Lancashire coast.

† I made memoranda of the heat at the time, but these I have unfortunately mislaid, and cannot now find them. I have, however, a distinct recollection of the fact that the heat was slightly less than it was on my leaving Manchester.

heat, namely, that during my first stay there was a land wind blowing, and that during my second stay the wind was from the sea nearly all the time, whilst the heat was somewhat in excess of what it had been during my first visit.

Another circumstance, which occurred in 1859, helped still further to cause me to doubt whether heat had any direct influence in producing the symptoms in my own case. A bunch of one of the grasses (I think it was the *Poa nemoralis*) had been gathered by one of my children and placed in a vase in one of the rooms at home which I seldom entered. I happened, however, to notice the vase in going into the room a few days after the grass had been placed there, and on disturbing it to examine it, a small cloud of pollen was detached and came in close proximity to my face. I commenced sneezing violently in the course of two or three minutes, and had what I considered a rather smart, though short, attack of my usual early summer disorder. As this grass flowers much earlier than the majority of the grasses cultivated for hay-making, and as there was little or no grass in flower in the meadows at the time, I was satisfied that the symptoms were due to the pollen which had escaped accidentally during the examination. From this time my experiments, I may say, commenced, and these have been carried on at intervals as opportunity has offered. With what result I must leave the reader to judge.

§ 72. The extracts I have just given of cases which have come more or less directly under my own notice, have been taken from the answers obtained to a set of questions, a copy of which was sent to each patient. These were framed so as to obtain as much information upon the causes of the disorder as it was possible to get without appearing to have a leaning to any theory which might bias the mind of the patient in giving the answers. To save repetition I have dispensed with the questions in each case, and have thrown the answers into a connected form.

§ 73. In the foregoing quotations I have endeavoured fairly to represent every variety of opinion held on the causes of hay-fever, and although I have done this at some length, and consequently at the risk of being somewhat

tedious, I have by no means exhausted the matter I had at hand.

§ 74. It will have been seen that, even before the time of Bostock, the popular idea was that hay or grass in flower was the exciting cause of this disease. Bostock, however, by his observations upon his own case, laid the foundation of the theory that heat was a much more active cause of the disorder than the emanations from grass or hay. His experiments seem, at first sight, to be tolerably conclusive, but when we come to examine them carefully, and to compare them with the observations of other patients and with the results of other carefully conducted experiments, we shall see that his reasoning was based upon the results of a mode of observation in which there were several sources of error which he did not discover.

With an acuteness which was quite characteristic, Elliotson not only took a comprehensive view of the phenomena of hay-fever, but at the same time did not fail to notice some of its important, though less prominent, features ; and, as the reader will have seen, he pointed out some of the probable causes of fallacy in the conclusions which Bostock had arrived at. Although the opinions of the latter have had great weight with most of the authors who have studied the subject since his time, it may be said that opinions have been pretty equally divided between the two theories. Like many questions, however, which have remained in an unsettled state for a length of time, this question of the cause of hay-fever has given rise to speculation ; and causes have been named which could only have been thought of in obedience to a strong impulse to catch at anything which seemed at all likely to have any share in the production of the disease, but which the simplest crucial experiment would have shown to have no such power as that which has been claimed for it. The question has in the last ten years so expanded itself that, from being, as it was at first, confined to the consideration of the two conditions named by Bostock, we have now at least half a dozen of these supposed causes presented for examination..

§ 75. When Dr. Phœbus took up the study of hay-fever, comparatively little had been done which could furnish sufficient data for accurate conclusions; and considering the state in which he found the subject, he has, perhaps, accomplished as much as any one man could have accomplished in the study of a disease in which there is so little chance of continued clinical observation. Not being himself a sufferer from the disorder, he had no opportunity of observing its peculiarities, or of making any experiments upon himself; and from the circumstance of the disorder being comparatively rare in Germany, he had, as he tells us, only the opportunity of observing one patient. If Dr. Phœbus had been himself the subject of hay-fever, and at the same time had had an opportunity of observing a greater number of patients, it is possible he might have arrived at somewhat different conclusions. As it is, however, he has become a warm advocate of Bostock's theory, but, unlike the latter, he claims for the *first heats of summer* a power which does not belong to the later heats of summer, and seems to infer that the former have some specific character which the latter do not possess. Of what this consists, however, he does not satisfactorily explain; nor, so far as I am aware, do any of the authors who have adopted the opinions of Dr. Phœbus on this point, make any successful attempt at explaining them.

§ 76. The necessity for believing that heat has different qualities at different times, and, as a consequence of this, has the power *per se* of effecting at one time of the year what it cannot accomplish at another, is one of the weakest points in the theory which claims that heat is one of the most efficient causes of this malady. Before we can accept this theory as the true one, it should be shown at what temperature, or between what ranges of temperature a patient, who is amenable to this influence of heat, will have the symptoms of the disease developed in him; and at what precise point he can depend upon being free from them. It should also be shown that at any time and in any place, where a patient happens to be who is known to be the sub-

ject of this form of hay-fever, when the temperature rises to the point indicated, an attack is sure to come on.*

On the other hand, in claiming for the agents named in the foregoing pages the power of producing the morbid conditions which characterise the disease, we should be equally exact in our requirements. It should not only be shown, in any individual case, that the attacks come on at a certain time of the year, when the substance, which is the supposed cause of the malady, is generated in the largest quantity, but it should be shown that at any time when a patient, who is presumed to be susceptible to the action of this substance, is brought in contact with it, it will to a certainty bring on the attacks. The cause of the malady should, in fact, be as capable of being proved to be so by repeated experiment as any chemical reaction is capable of being again and again demonstrated by the ordinary processes of chemical manipulation. In no case has this hitherto been done.

§ 77. In Dr. Smith's treatment of the subject, there is an evident wish to fall in as much as possible with the views of Dr. Phœbus, but at the same time to recognise and to give fair prominence to the views of English observers. There is, however, no attempt at recording anything more than what may be fitly termed fragmentary observations on the influence of heat, and of the other causes named.

Dr. Pirrie, as will have been seen from the quotations already given, has gone a step further, and has distinguished the attacks which are said to be caused by heat from those which are thought to be due to other and very different influences. He, however, does not give us the history of any case where the symptoms and the apparent cause have been observed, season after season, and where the dates and localities are given in the manner in which Bostock has given them.

In Dr. Moore's pamphlet we have the symptoms, causes, and treatment of the malady given in a most orderly and concise manner, but although the author has evidently devoted a great amount of attention to the disease, he does not give us the history of even a single case.

* Unless it can be shown that other causes are in operation which prevent the attack coming on.

It is a larger amount of this kind of evidence which is most urgently needed to enable us to arrive at sound conclusions, and the absence or comparative smallness of which has hitherto constituted one of the great difficulties in the study of the disorder; whilst this has at the same time been an almost insuperable barrier to the attainment of correct notions of the value of remedies.

It was in order to supply my own mind with additional testimony of the kind I have alluded to above that the experiments, which will be described in a succeeding chapter, were commenced.

It would be unreasonable to suppose that the experience of one individual could furnish an amount of evidence large enough to permit us to hold the question of cause, in any disease, as being a settled point, but whilst I cannot claim to have determined this for all cases of hay-fever, I believe that in doing so for my own case I shall at least have shown the way in which this may be done in a very large majority, if not in all, of the cases of the same character; and I am convinced that the more closely we study the malady, the more we shall find that, to a much larger extent than has hitherto been supposed, it is unique in the nature of its causes and in its character also.

(*To be continued.*)

TYPHUS ABDOMINALIS.

By Dr. TRINKS.*

[THE *Neue Zeitschrift für Homöopathische Klinik* has lately given to the world a long and exhaustive work on typhus, by the late Dr. Trinks, of Dresden. It is altogether too long for insertion in these columns, but we are sure that our readers will be pleased to peruse the thera-

* The *typhus abdominalis* of German authors corresponds to the *typhoid with ulceration of the bowels* or *enteric typhoid* of English systematists, and not to *typhus* properly so called. It seems, also, occasionally to include our *gastric typhoid*.

peutic portion of this excellent essay, as the results of the observations and reflections of such an eminent adherent of the homœopathic school, respecting the treatment of this formidable disease, cannot fail to be both interesting and instructive. The work is probably not precisely what Dr. Trinks would have given to the world had he been alive to superintend its publication, for there are evidently some lacunæ which its author could easily have filled up, and which deprive it of that completeness we would have expected it to possess ; but, such as it is, it is too valuable a contribution to our therapeutics to make us regret that Dr. Hirschel has published it, imperfect though it undoubtedly is in some few particulars.]

Before proceeding to consider the characteristics of the several remedies we may give a general outline of the treatment of typhus.

1. *Simple Abdominal Typhus*

requires the employment of but few remedies. *Aconite*, occasionally *Bellad.* and *Ipec.*, in the first five days of the first week, and in the last days of that week *Bry.*, whose sphere of action extends to the end of the week, and then *Phosphoric acid*. A rapid change of the medicines is seldom indicated, and they must be allowed full time for their action. The typhous process must go through its various stages and cannot be cut short. Moderate epistaxis in the first week must not be regarded ; but violent and frequently recurring epistaxis in the later periods must be checked by *Acid. sulph.*, as it diminishes the mass of the blood. The typhous stools, when they occur not oftener than two or three times a day, must not be checked before the tenth or twelfth day, otherwise the disease has a tendency to fly to the brain. The constipation that sets in must not be interfered with, otherwise diarrhœas, difficult to master, are apt to occur, and the cicatrization of the intestinal ulcers is delayed and forcibly interrupted, and adhesions causing constriction of the intestinal canal may be produced.

2. *Typhus biliosus*

is generally, at least in the cases we have seen, the product of external influences, which cause a hyperæmia, or even an inflammatory irritation of the covering of the liver and of the biliary passages which mask the typhus; hence there is first, yellow, furred tongue, bitter taste, nausea, even bilious vomiting, painful sensibility and swelling of the liver, icteric coloration of the skin and urine, ash-grey fæces, pressive headache, febrile symptoms, and increase of temperature. In such cases, also, we first gave *Aconite*, if there had been any previous violent emotion *Chamomilla* also; when the liver was very painful *Bellad.*, and when the icteric complication was very prominent *Merc. sol.* By this method this complication was removed by the end of the first week, and the typhous process continued its course on the mucous membrane of the bowels, requiring *Bryon.* or *Acid. phos.*

3. *Typhus pituitosus.*

This form, which is characterised by a very intense affection of the whole mucous membrane of stomach and bowels, by a very slow lingering course with but moderate implication of the nervous system and circulation, and is generally observed in persons of middle age, especially males of lymphatic-phlegmatic constitution addicted to the free use of spirituous liquors, when it is of moderate intensity requires *Pulsatilla*, and if this does not suffice *Merc. sol.*, when there are thick greyish white coating on the tongue, very bad metallic smell from the mouth, disgusting, clammy, insipid, soapy taste, thin, very fetid stools with much mucus of a glassy appearance, combined sometimes with dysenteric tenesmus in rectum, urine with mucous sediment, clammy, fetid perspiration; *Rhus tox.*, when along with this intense affection of the intestinal mucous membrane the brain is seriously affected with stupor, sopor, muttering delirium, automatic muscular movements in the hands and feet.

4. *Typhus putridus*

is distinguished by great disposition to hæmorrhage in all the cavities, from all outlets of the body and in the subcutaneous cellular substance, from ruptured congested capillaries and larger blood-vessels. This tendency shows itself either in the first days of the disease or in its later course, and is usually accompanied by greater or less derangement of the brain, spinal cord, and intestinal mucous membrane. Hence there are copious bleedings from the nose and buccal cavity, from the bowels, kidneys, bladder, uterus, and lungs, extravasations of blood in the subcutaneous cellular substance in all forms and degrees, rapid and gangrenous decubitus, noma, &c, and, as a consequence, great and dangerous anæmia and collapse, rapid sinking of the forces, death by paralysis of the heart, &c.

We have generally observed these states in the second, third, and fourth weeks in severe cases of typhus in anæmic, ill-fed individuals, who had previously suffered from exhausting ailments.

For these septic conditions we first gave *Acid. sulphuricum* in large and oft-repeated doses, with the best and most rapid results. The hæmorrhages from nose, mouth, lungs, bowels, kidneys, and bladder were arrested and did not recur. Even the subcutaneous extravasations of blood did not increase. We have often seen these, as well as the hæmorrhage from the bowels, yield to the energetic employment of *Arsenic*. Our experience does not allow us to recommend *Acid. muriat.*, but we should not forget *Acid. nitr.* in many cases. We have no experience of our own with respect to *Phosphorus* or *Carb. veg.*, which latter, however, is of great use externally in cases of gangrenous decubitus. In noma the external and internal use of *Kreasote* was of no use, but we are inclined to put great reliance on *Camphor*. The anæmia that remains yields to *Quinine* and the exhaustion of the fever to strong wine and perhaps also to *Moschus*. On the occurrence of collapse with gradual lowering of the temperature *Camphor* always proves useful.

5. *Typhus with predominant cerebral affection.*a. *Typhus apoplecticus.*

This is a form which we have met with twice in the early days of the disease, viz. on the fourth and seventh days in a male and a female of the age of thirty. In both the typhus commenced with extreme congestion of head and face, very rapid, hard and full pulse, and had caused a great over-filling of the cerebral vessels. Suddenly an apoplectic fit came on with complete loss of consciousness and hemiplegia. Death ensued in a few hours. In such cases I would not hesitate, on the occurrence of the threatening prodromata, to open a vein, to apply ice to the head, and to give *Bellad.* in repeated doses, until the congestion of blood in the cerebral vessels has diminished and the danger has been arrested. If the apoplectic fit has taken place we would pin our faith to the energetic action of *Arnica*.

b. *Typhus cum statu versatili; febris nervosa versatilis; sive cum erethismo cerebri et nervorum.* A form always difficult to cure, which in its severer development allows of only an unfavourable prognosis, as it often ends in cerebral paralysis.

The medicines that have proved of use in it are—*Bryon.*, followed by *Acid. muriat.*, then *Hyos.*, *Opium*, *Bellad.*, *Stramonium* and *Zincum*.

When this status versatilis is developed in the commencement of the disease, its further progress may, in many cases, be stayed by the administration of *Bryon*.

If this result is not obtained after it has been taken for three or four days, if, on the contrary, the erethism of the brain and of the arterial system become increased in the morning and evening hours, then *Acid. mur.* is indicated, and this frequently has a soothing effect on the brain and circulation.

In not a few cases we have been able to observe this very beneficial action of *Acid. mur.* on the brain and blood life, and have seen the excitement of the vascular system very rapidly allayed, and return to the normal condition, the temperature of the skin also becoming lower. The typhous

process thereafter ran its course without further interruption. But we have never observed that it exercised any great influence over hæmorrhage—as, for example, subcutaneous extravasation of blood; hence we are inclined to doubt the antiseptic powers attributed to it by the old school. Nor did it perceptibly have any effect on the typhous evacuations. Its action did not appear to us to be powerful enough in the status stupidus. Still, we must reckon it as one of the indispensable remedies in typhus. Hufeland himself knew, and lauded its effects in the status versatilis, and we freely confess that we acted on his recommendation, and can only confirm his opinion.

Hyos. is useful in the status versatilis with great delirium, the subjects of which change frequently.

Opium is often very efficacious in the status versatilis, with very severe persistent delirium, caused by very intense cerebral irritation, and attended by considerable congestion of blood towards the head and face. But we have also seen good effects when the delirium was purely nervous, and not owing to hyperæmia of the brain.

Stramonium we gave with good effect when the delirium assumed the form of visions and monomanias, and was attended by hallucinations of the senses of seeing, hearing, and smelling; also in furious delirium, in which we have also seen excellent effects from *Bellad.* In the last-named state *Zinc* deserves attention; it is also said to be of use in threatening cerebral paralysis.

c. Typhus cum statu stupido; typhus cum torpore; febris nervosa stupida.

This form of typhous cerebral affection allows of the most favourable prognosis, even if the state should go on to actual paralysis, and the patient fall into a condition of acute idiocy; the senses, too, are involved in this paralytic-like depression. The patient sees nothing with his eyes open, and the hardness of hearing increases to complete deafness; voluntary muscular movements cease, the limbs are moved about mechanically, and, if the spinal cord is implicated, then even automatic muscular movements are discontinued.

The circulation of the blood and the temperature are not

much affected, nor is the mucous membrane of the lower bowels. This kind of typhous cerebral affection usually appears in latter days of the first, or in the course of the second week, seldom later, and it may last into the fourth, fifth, and sixth week. The medicines particularly serviceable are—*Acid. phos.*, *Rhus tox.*, *Cocculus*, *Helleb. nig.*, and *Phosphorus*. *Phosphoric acid* is suitable for the milder cases, in which the patient passes the most of his time in sleep, when awakened immediately regains consciousness, but is very little disposed to talk, and is very apathetic when awake. Any questions put to him he answers very curtly with a “yes” or a “no,” and when left alone he immediately falls asleep again. There is some hardness of hearing; he has dry lips and tongue, the latter often being covered with brown cracked sordes. Rumbling and gurgling before the typhous motions, which are of a very moderate character; clear acid urine; febrile exacerbations and temperature very moderately increased. Such cases occur very frequently, and generally terminate favourably at the end of the third or fourth week, without requiring the employment of any other medicine.

Rhus tox. has a more intense action in the grave cases of this sort in which the brain is seriously implicated from the beginning. The stupor and sopor, interrupted by delirium of greater or less severity, often make their appearance on the fifth or sixth day of the first week, accompanied by increasing febrile exacerbations and elevation of temperature; the affection of the intestinal mucous membrane is not very considerable; the typhous evacuations cease, or are limited in number. On the other hand, the patient loses consciousness more and more, no longer recognises those about him, and falls into even deeper sopor, from which he can only be roused for instants at a time. Involuntary evacuations of fæces and urine, together with automatic movements of the muscles and tendons of the hands and feet, set in; the weakness and prostration increase. We have often been astonished by the rapid change to the better through the action of the medicine; in two or three days the patient's consciousness returned, and the profound and

heavy sopor gradually became natural sleep; the fever and the temperature fell in a few days to the normal point. In other more severe cases the change for the better occurred more slowly, but then it continued to progress steadily. In yet other cases, after the cerebral symptoms subsided, the affection of the mucous membrane of the bowels became more prominent, and called for the employment of other more suitable remedies.

Any way *Rhus tox.* must be reckoned one of the most efficacious remedies in cerebral typhus with stupor and sopor, and all who have witnessed its energetic action at the bedside will join me in singing its praises.

Cocculus we have found serviceable in a few cases, in which there had come on in the latter course of the disease deep sopor, with pinched, pale features and sharp nose, very quick pulse and heart's beat, rather lowered temperature of the skin, automatic movements of the muscles and tendons; but in which the mucous membrane of the bowels was but slightly affected.

We have administered *Helleb. nigr.* with great effect in a few cases, which presented the perfect features of acute idiocy; the patients had not the slightest consciousness, all impressions on the senses and all expressions of the will were wanting, the heart's beats and the pulse were very low, the skin only moderately warm, the bowels quite inactive. Urine passed involuntarily; swallowing difficult.

We shall now pass in review the remedies indicated in typhus.

Acidum muriaticum.

I am surprised that there is so seldom mention in our homœopathic literature of this drug, and I can only occasionally find a trace of its employment in typhus fever. It has been recommended (Hartmann, *Therapie acut. Krankh.*, I, p. 325) in febris nervosa stupida, and especially when the patient has a tendency to get down to the foot of the bed, groans and sighs, with constant mutterings and unconsciousness when awake (!), with paralytic state of the tongue, and

great dryness of fauces and mouth; the patient is, though quite conscious, unable to move the tongue as he wishes (consequently, when there is complete glossoplegia, which I have never seen in typhus, Tr.); the tongue appears to him to be too long and heavy, so that he can scarcely elevate it. Conclusive for the choice of this remedy are said to be—intermittent pulse at every third beat, and excessively copious secretion of watery urine, symptoms that are but seldom met with in typhous affections.

My own experience of the employment of *Acid. mur.* in the nervosa stupida and putrida has furnished me with unfavorable results, and has convinced me that it is of no use either in the higher grades of the nervosa stupida or in the last named form. On the other hand, it is of great use in the opposite form of febris nervosa versatilis, in the higher grades of which it surpasses *Bryon.* itself in curative efficacy.

I have not been able to find in the very imperfect register of symptoms of its physiological action, any hint as to its use in this form of typhus. I was guided by the recommendations of the older physicians, which are often worthy of regard, and specially of Hufeland, and I was not disappointed. A far from inconsiderable array of observations has taught me that *Acid. mur.* is an extremely powerful agent in the higher grades of febris nervosa versatilis, and it is especially indicated when *Bryon.* has already been given for several days without effect, when the disease and its symptoms became always more severe, and when the excitement of the arterial system became always more and more intense, states that are characterised by the following symptoms:

Persistent delirium, now more now less marked, which allows the patient neither rest nor sleep; his attention is constantly engaged with ever varying pictures from the past and present, and he is kept in constant excitement, so that he forgets time, place, and every thing he has just been speaking about. The activity of his senses is quickened in equal degree to the extent of the most vivid hallucinations; his eye shuns the light, his ear is sensitive

to the slightest sound, he hears sounds that have no existence, *e. g.* rain pouring down, and music; his sense of smell is much more acute, so also is his sense of taste; the eyes sparkle, the pupils are contracted, the cheeks have a circumscribed patch of red on them, the nose, lips, and tongue are dry, but the tongue is not furred, or only slightly so; the thirst is, however, great; the intestinal canal and its mucous membrane is either not at all or very slightly affected; typhous stools either absent or very rare; urine clear, acid; heart's beats and pulse very quick, irritated, deficient in energy, 110—130; respiration hurried; skin generally dry, its temperature increased. Great desire for sleep, without being able to sleep; muscular power not much diminished, slight feeling of weariness and relaxation.

The beneficial action of *Acid. mur.* displays itself first in the gradual lowering and diminishing in frequency of the excessive action of heart and blood-vessels; the pulse sinks in the course of 48—60 hours to 90—80, and this is accompanied, *pari passu*, by the soothing of the excited cerebral arteries, and of the erethism of the nerves of the senses and others; the patient begins to slumber and sleep; the extreme thirst diminishes; lips, tongue, and buccal cavity again become moist, and convalescence at once sets in, or the typhus gradually runs its course as simple abdominal typhus, with the ordinary symptoms of complication of the mucous membrane, without further interruption. In all cases of this sort I had generally given *Bryon.* for four or five days previously, without effecting any improvement of the morbid condition. The great increase of the severity of the symptoms then compelled me to look around me for a more energetic medicine, and I had always the pleasure of finding *Acid. mur.* answer my expectations.

I always prescribe *Acid. mur.* in the 1st dilution, two or three drops in water every two, three, or four hours, and I have always seen the best effects from these doses.

Acidum Phosphoricum

α;

has a sharply defined sphere of action in typhus entericus, within which sphere it is of remarkable utility. But the

plan adopted by the allopathic practitioners of giving it in almost every case of this disease, is irrational and objectionable, and is often followed by disastrous consequences.

On carefully comparing the physiological effects of *Phosphorus* with those of its acid, we find a great similarity between them, and we must regard the latter as a sort of depotentized *Phosphorus*. The physiological effects of *Phosphoric acid* differ from those of *Phosphorus* chiefly by their greater mildness and inferior intensity, and this view is borne out by the results of treatment. By bearing this steadily in view, we shall find that we cannot see in *Phosphoric acid* a remedy for the typhous processes comparable to *Rhus tox.*, and this view is justified in practice. At the same time we are far from estimating lightly its curative powers in this disease, but we have found no reason to place it on a parallel line with *Rhus tox.* in respect to the intensity and range of its action. The sphere of action of the latter in the typhous processes is much more extensive and more profound. We shall seldom have occasion to resort to the administration of *Phosphoric acid* before the seventh or eighth day, hardly ever before the tenth or twelfth day of the development of the typhous process. During this period the erethic character of the typhous phenomena generally declines spontaneously, or is extinguished by the use of other medicines (*e. g.* *Bryon.* and *Acid. mur.*), and gives place to the torpid character, which can either attain a moderate height, or may in a short time rise to a dangerous degree. In the former case the disease will have localized itself in the mucous membrane of the large intestines, as shown by the persistent, moderate, thin, very fetid, pea-soup-like evacuations, attended by constant rumbling and gurgling, combined with, in many, dryness of the mucous membrane of the mouth and fauces, the tongue covered with thick greyish-white phlegm, the greater or less tenderness of the ilio-cæcal region, and the urine with acid reaction. The patient begins to slumber even in the daytime, has low muttering delirium, but is easily roused from this slumber, when he immediately regains full consciousness; the eyes have a dull, sleepy

lustre, the countenance assumes a stupid, apathetic, indifferent expression ; speech is difficult, the pulse 100 and upwards ; the temperature of the skin evenly elevated, the skin itself dry, or covered with clammy moisture ; a miliary rash breaks out first on the neck, chest, and forehead, gradually spreading over all the surface of the body, and at last extending to the feet.

Phosphoric acid is suitable for this moderate height of the torpid condition until the fifteenth or twenty-first day, in which period the brain first of all becomes freer, the slumbering intellectual powers more active, and the senses more acute ; the dryness of the mucous membranes of the nose, lips, and mouth diminishes ; the tongue becomes moister and cleaner ; the rumbling and gurgling in the bowels rarer ; the thin, pea-coloured stools less frequent, finally ceasing altogether ; the pulse falls to 80, and even much lower, the temperature of the skin sinking in equal degree. The patient turns again upon his side, and his sleep is longer and more refreshing, and he gradually passes into convalescence, during which the vegetative processes become stronger and more predominant.

To resume. The indications for the employment of *Phosphoric acid* are strictly based on its physiological effects, which bear the impress of torpid character, defective reaction in the nervous and circulatory systems, and decline of the organic vital power. Hence the typhous processes, in which it is curative, are of the torpid character, in fact, the typhus cum torpore of the old school.

I have always given *Phosphoric acid* with good results to adults in the dose of two drops of the 1st dilution, to children the 2nd dilution, every three or four hours.

Aconitum napellus.

Drs. Wurmb and Caspar, in their joint treatise, denounce the employment of this drug in typhus abdominalis, because it does not belong to the class of sthenic diseases in which alone *Acon.* can be curative. Hence it is always to be feared that by its administration a sthenic condition may be produced.

In the first stage of the development of enteric typhus, before its localization on the mucous membrane of the bowels, all its symptoms are those not of an asthenic, but in most cases of a so-called sthenic character, and demand the employment of *Aconite*. Among these symptoms we count in particular the febrile phenomena, with their evening exacerbations and increasing temperature, the quality and quickness of the pulse, the rush of blood to head and face, the injected albuginea of the eye, &c.

Notwithstanding the denunciations of the above-named gentlemen I have always given *Acon.* in typhus when its employment was indicated by the phenomena present, the synochal fever with its evening exacerbations and increase of temperature, its quick hard pulse, its rush of blood to head, face and eyes, &c., and I have attained the desired result completely in all cases of typhous disease that did not commence too stormily and malignantly; viz., diminution of the fever and of the temperature, even in the evening, diminished congestion of head and face. Nothing more can be effected by *Acon.* in this disorder. I have never, in a single instance, seen any harm result from its use, and, assuredly, never the ill-consequences observed by those gentlemen. In several cases it seemed to me as if, by the diminution of the fever effected, the localization of the disease on the intestinal mucous membrane was promoted and brought about more rapidly.

The action of *Aconite*, in mild cases, is very soon evidenced by the diminution of the fever and heat, by the lowering of the pulse by ten to twelve beats, by lessening the rush of blood to the head and face and the heat of the head and the throat, and by the production of more or less perspiration. In some cases this result is only obtained after giving *Acon.* for two or three days.

It is most appropriate in the first three or four days of the disease; after this period it is only exceptionally that its employment is advisable. For all cases of typhus commencing with great intensity, *Aconite* will assuredly not suffice to produce permanent diminution of the febrile

disturbances of the circulation; for this much more energetic medicines are required.

It is gratifying to us to find that Dr. Kafka corroborates our experience of the suitability of *Aconite* in typhous affections. We have always given with the best results the 2nd and to children the 3rd decimal dilution in doses of two or three drops in water every two or three hours. From these doses we have seen nothing but good effects.

Apis mellifica.

This insect poison, which has only been lately introduced into our pharmacopœia, is always receiving an increased amount of clinical application; even if it does not quite come up to the extensive usefulness in diseases attributed to it by Wolf, it cannot be denied that it has a great pharmacodynamic power which makes it an efficacious remedy for many acute and chronic diseases.

Dr. C. W. Wolf, in his *Homöop. Erfahrungen*, Heft. I, first recommended it in typhus entericus, when the disease comes on after intermittent and the following symptoms are present: apathetic, unconscious, soporous state, with muttering delirium, hardness of hearing, inability to put out the tongue or to speak, dryness of the tongue, which is fissured, sore, and ulcerated, difficulty of swallowing, painful tension and distension of the belly, which is tender to touch and pressure, long-continued constipation, or frequent, painful, fetid, bloody, involuntary diarrhoea, urine jumentous and passed involuntarily; skin sometimes partially dry, burning hot, sometimes partially covered with clammy sweat and cool, trembling and jerking of limbs, white miliary rash on chest and abdomen, extreme weakness and working down to the foot of the bed; changeable pulse, sometimes slow sometimes quick, weak and intermitting, and with simultaneous affection of the mucous membrane of the bronchial tubes. This is the picture of typhous disease with special soporous and comatose affection of brain and spinal cord, which we have found to recover under the use of *Rhus tox.*, *Phosph.*, and *Arsen.* In the case of persons affected with tubercles in the lungs and abdomen a

dose of *Sulphur* should be administered before the *Apis*, and when the smallpox virus is present (?) *Antim. tart.*; in cases with great disposition to paralysis *Apis* should be given alternately with *Moschus*.

As we have no clinical experience of our own as regards the curative power of *Apis* in such serious cases of typhus, seeing that we could always rely on the other medicines we have mentioned in such cases, we content ourselves with referring to Wolf's experience, which future clinical trials may confirm. In any case it deserves the best attention of homœopathic practitioners. Wolf advises the 3rd dilution of *Apis* as the most appropriate dose, one drop in water, repeated as often as needful.

Argentum nitricum.

This medicine, which is so little known to practitioners of the physiological school, but is so often misused in its external employment as a caustic, has been employed by them in typhus entericus in order to check the too copious typhous evacuations. Espanet mentions its employment by Italian practitioners in certain stages of typhus. As far as we know it has not yet been used in typhus abdominalis by homœopathic practitioners, nor have we ourselves made any clinical trials of it.

We have no doubt, however, that owing to its specific action on the mucous membrane of the intestinal canal it may be useful in the too frequent typhous stools even when accompanied by pain and hæmorrhage, when other remedies fail us.

We would recommend it to be given in the 3rd, 4th, or 6th dilution, two or three drops in water, every three or four hours.

Arnica montana.

We are well aware that in ancient physic this plant played a principal part in the treatment of so-called stupid nervous fever. Its pharmacodynamic effects justify us in supposing that it may be successfully employed in the soporous and comatose states of typhus abdominalis, in

which, indeed, it is recommended by Hartmann (loc. cit.). It seems to have met with but little attention from homœopathic practitioners, and when we have had occasion to give it in such states we have seen hardly any favourable effects. But these negative clinical results should not induce us to neglect it.

Arnica is only likely to be useful in stupid, soporous, and comatose cerebral affections in typhus entericus, with muttering delirium, paralysis of the sphincters, great prostration of strength, very low, weak, rapid pulse, &c.

In such cases we should give the 1st dilution, two or three drops in water, every three or four hours.

Belladonna.

The great value of this polychrest, which is applicable to many diseases and indispensable in many acute maladies, is ever more and more acknowledged the oftener it is employed. So of late years its sphere of action in enteric typhus has been considerably enlarged, and the indications for its appropriate employment more sharply and accurately defined by accumulated experience.

The earlier observations and experiences of its employment in typhous affections may be read in Rückert's *Klin. Erfahr.*, Bd. IV, p. 671, where they are all arranged in a very convenient manner.

Bähr (*Therapie*, II, p. 632) seems to form an insufficient estimate of its value in typhus entericus, and only speaks of its employment in the first week to allay the violent fever and the inordinate action of the circulation in the brain, and in violent bronchitic complications.

Kafka (*Therapie*, II, p. 571) must have drawn his indications for the employment of *Bellad.* in typhus entericus from a more fruitful source and from more numerous observations and experiences, as they coincide perfectly with those we have ourselves laid down. He says:—When after the employment of *Aconite* no improvement takes place we may be sure we have to do with a serious illness. In such a case the phenomena of flow of blood to the brain are predominant. The patient throws himself about in the greatest restlessness, complains of

pains in forehead and temples, is now noisily, now quietly delirious, the cheeks are red, the eyes sparkling and injected, the head burning hot, the carotids beating violently, the heat of the skin is pungent, the pulse very frequent, the thirst much increased, the tongue dry, the urine deep-coloured and scanty. When symptoms like these present themselves we administer for the hyperæmia of the brain, *Bellad.*, *Atropin*, *Apis*, *Stramon.*, *Hyoscyam.*, or *Glonoïn*. In the generality of cases we obtain favorable results from these medicines, the cerebral congestive symptoms, the delirium, the excessive heat, become alleviated, the typhus becomes milder and pursues its further course.

According to my observations and experiences, *Bellad.* showed its curative effects in typhous disorders: 1st. Which from their commencement are ushered in with very intense fever, violent evening exacerbations and increase of heat, great rush of blood to head, eyes, and face, with deep red often dark-blue coloured cheeks, bloated face, much vertigo on movement, great heaviness and confusion of head, various kinds of pains, mostly shooting, pressive or compressive in the whole head or in some portions of it; great inclination for sleep, without being able to sleep; slumber with awful and frightful dreams, which sometimes bring on violent delirium; great thirst for cold water, with dry lips and bright red tongue; great restlessness, &c.—against which symptoms *Aconite* has been ineffectually employed. In such severe forms, in which, in addition to the intense fever, the typhous virus makes violent attacks on the brain, we have seen in most of the cases, after from three to four days of the energetic use of *Bellad.*, considerable decrease of the fever, considerable diminution of the rush of blood, and cessation of the very violent and agonizing pains in the brain, and a great tranquillization of the excited state set in.

If *Belladonna* is successful in warding off from the brain these violent assaults of the typhous poison, the disease localizes itself on the intestinal mucous membrane, and then runs its usual course. But if it fails to do this, the cerebral affection takes on other forms, such as the nervosa versatilis and stupida, or it passes into the apoplectic

form, as we have had occasion to observe. Other observers say they have seen it pass into meningitis spinalis.

2. In the maniacal form of enteric typhus, when this is particularly associated with stormy disturbances of the circulation. Such acute manias often come on after very trifling external causes, in otherwise mild cases of typhus, even in the course of the second and third week, as we have several times seen. In those cases that are ushered in and attended by great disturbance of the circulation, *Belladonna* always proved useful; in two other cases, females, which had more the character of nervous erethism, *Stramonium* was efficacious.

3. In bronchitic affections, which come on pretty severely in the commencement of the disease, especially in children, and often mask the development of the typhus, *Belladonna* is of great use, both for allaying the febrile disturbance and for removing the local bronchitic disorder. In less severe cases *Bryonia* is usually sufficient.

4. In the course of even a mild attack of the typhous process, there often occurs, in consequence of external exciting causes, local peritoneal inflammation in limited portions of the bowels, not caused by perforation of the bowel. For these unwelcome accidents we have often found *Belladonna* in oft repeated doses afford speedy relief.

Bryonia.

Drs. Wurmb and Caspar do not seem to have thoroughly appreciated the great importance of this medicine in enteric typhus, although Rau (*über Erkenntniss und Heilung des Nervenfiebers*, p. 499), mentions it approvingly, and recommends it in versatile nervous fever (*febris nervosa versatilis*), especially in the earlier stages of irritation with congestion of the brain and organs of the chest.

Bryonia is of very frequent applicability in typhus, for most cases assume such a character as energetically to demand its employment, and its action in them is almost always extremely beneficial and curative. We have found it much more frequently indicated than any other medicine, not excepting *Rhus tox.* and *Arsenic*. As in so many other common diseases, so in typhus; it is a sort of

polychrest. By far the greater number of typhous disorders commence with symptoms of erethism in the nervous and vascular systems, which afterwards give place to others of a different stamp, and *Bryonia*, as its physiological effects show, corresponds most accurately to such states.

The late Dr. P. Wolf* has furnished some excellent preliminary studies of the characteristics of this medicine as well as of the general and special indications for its use, which we cannot pass over unnoticed. Dr. Wolf claims for it a high rank in the therapeutics of typhous affections, and says of it that it is the remedy for febrile diseases that most frequently gives a favorable turn to impending development of a nervous state, and though it chiefly corresponds to the first period of typhus, it is also often indispensable in the later stages of both cerebral and abdominal typhus.

From the observations of a large number of cases in the well-marked phenomena of which no sensible practitioner could fail to diagnose the commencement of typhus, we go still further than our esteemed colleague, and give it as our firm opinion that in many cases *Bryonia* is capable of cutting short the further development of typhus, for we have frequently observed that after its administration all signs of the commencing typhus would suddenly cease, often in one night, and complete recovery set in.

We are well aware that other practitioners also (*e. g.* Lebert, of Zürich) have frequently observed typhus cut short. We are naturally of a very sceptical turn of mind, and would not venture to express this opinion were it not that we have observed the fact so frequently, and under such circumstances as to admit of no other explanation. The change usually took place too suddenly and too unexpectedly to allow us to attribute it to what is called a natural cure; it did not take place on the fifth, seventh, thirteenth, or fifteenth day, but usually very soon after the exhibition of *Bryonia*.

In the later stages of both cerebral and abdominal typhus we have seldomer found *Bryonia* indicated.

“That particular form,” says our colleague, “which is

* *Hygea*, Neue Folge, vol. i, translated in *B. J. of H.*, vol. viii, p. 433.

termed *nervosa versatilis*, with excitement of the imagination and of the emotions, over-excitation of the senses, rapid change of the symptoms and their increase and decrease, as also the numerous other phenomena of excessive activity of the nervous and vascular systems, whose strength bears no proportion to their liveliness, gives us a general picture of the state to which *Bryonia* corresponds in nervous fevers, for which it is also suitable when there is a considerable development of the nervous phenomena, and for which its lengthened employment—often throughout the whole of the disease—is advisable, as long as the vitality is not greatly lowered.”

Dr. Wolf considers the selection of this remedy within the limits of its sphere of action as one of the easiest and most certain for practitioners at all conversant with their profession. The only difficulty he sees is the determination of the boundary between *Bryonia* and *Belladonna*, a distinction all the more important because the frequent catarrhal and inflammatory affections of the serous or mucous membranes in every part of the body—especially in the first stage—may according to circumstances demand the employment of one or other remedy for the gastric conditions; and with this determination the peculiar pains experienced by the patient have much to do.

After what we have said respecting the indications for *Belladonna* in our disease, this difficulty will not appear so great, even in reference to the local and inflammatory affections of single organs in the course of typhus. And after the characteristics we have given of both these remedies, it will not be so hard to discover the moment when the administration of *Belladonna* should be abandoned and that of *Bryonia* resorted to.

Wolf lays down the following as the particular phenomena of typhus that call for the use of *Bryonia*.

Irascible, irritable, quarrelsome, lachrymose humour, anxiety.

Delirium at first only when asleep or on first waking; as the disease advances, when awake, and then very constant. The delirium is generally in reference to the daily occupa-

tions of the patient, which he thinks he ought to set about, and when it is accompanied by violent efforts to get out of bed. The patient attempts to put on his clothes. Children speak about their school, or they express a wish to go home; they do not believe they are in bed. Some questions are answered rationally, and then the delirium returns.

Visions: Round the bed, and especially at the foot of it, people are seen, even when the eyes are shut. This kind of vision Wolf thinks is a decisive indication for *Bryonia*; we cannot agree with him in this, as it may also point to *Hyoscyam.* or *Stramon.* when the state is otherwise adapted for these remedies (Trinks).

Restlessness, constant desire to change the position, sleeplessness from anxiety, from fear of visions (the patients do not dare to close their eyes), with great desire for sleep. Sleep with tossing about, speaking, groaning, starting, anxious dreams, night-mare, unrefreshing slumber. (This is seldom refreshing before the fifteenth day. Trinks.)

Vertigo worse on rising up, confusion and heaviness of the head, staggering gait, hot feeling in the head; shooting, throbbing, tearing, constrictive (as from a ligature) headaches. (These symptoms are by no means exclusively indications for *Bryonia*, as they also indicate *Belladonna*, Trinks.)

Look dull, sometimes penetrating, eyes mobile, glassy (?). Intolerance of light and of all strong impressions on the senses. The photophobia and the over-sensitiveness of the senses are in this case more of the nervous character, in *Bellad.* they are more of the hyperæmic congestive nature (Trinks).

Slight redness of the conjunctiva; *Belladonna* has great injection of some of the vessels, which spread over the eye in an aborescent manner (Trinks).

Catarrhal symptoms (hyperæmia of the mucous membranes is characteristic of *Bryonia*. Trinks).

ringing in ears, chirping, roaring, too acute hearing, hardness of hearing (when these phenomena present the more purely nervous character. Trinks).

Nose generally dry.

Face red (but not so dark red as in *Belladonna*, rather pale red. Trinks).

Tongue clean (?), often covered with yellow phlegm (or also white), afterwards dry, brown, fissured. (These three last appearances on the tongue belong to quite a different category. Trinks.)

Skin of lips brown, as if burnt. (This symptom also belong to a different class. Trinks.)

Tongue rather trembling. (May also point to other medicines. Trinks.)

Taste slimy, bitter, sour, as long as the tongue is dry (seldom! Trinks).

Feeling of dryness of the tongue.

Thirst.

Retching, nausea (seldom! Trinks).

Tenderness of scrobiculus cordis.

Constipation. But the opposite state need not be minded, if *Bryonia* is otherwise suitable. Even moderate abdominal typhous diarrhoea is no counter-indication when the strength is satisfactory. (Quite right! for such diarrhoeic states are met with in the physiological action of this drug. Trinks.)

Pale, crude urine; brownish, without sediment. (In typhus the urine can furnish no indication; in the first period of the fever it is crude, afterwards clear, with acid reaction, and it remains so even when there is a high degree of putrid complication. Trinks.)

Urging to urinate (seldom met with at the beginning and cannot point to *Bryonia*. Trinks).

Typhous processes of the mucous membrane of the organs of respiration (when these manifest themselves as hyperæmias and irritated conditions of the mucous membrane of the larynx and bronchial tubes. Trinks).

Gelatinous, viscid mucous secretion (does not point to *Bryonia*. Trinks).

Hypostatic pneumonia occurs generally in the later stages and does not indicate *Bryonia*, as Wolf justly remarks.

In genuine (?) typhous bronchitis, both primary and secondary (?), Wolf saw much good effected by this drug

(when that affection had not attained any great height; more fully developed cases require *Merc.* and *Phos. Trinks*).

The symptoms preceding an outbreak of perspiration and those that accompany the miliary eruption, such as increased febrile movement, great discomfort, restlessness, tossing about, feeling of heat, desire to lie without clothes, dry skin (?), rapid, soft, or irritated, rather jerky (very quick, *Trinks*) pulse, palpitation of heart, rapid, hurried breathing, oppression, feeling of anxiety, referred to the substernal region or heart, sometimes decided painful sensation only on pressure in the former, stitches also in the latter region. Palpable alleviation of the symptoms by the outbreak of perspiration. (These states, as Wolf truly remarks, occur too late in the disease for the employment of *Aconite*; they have also too much the character of nervous and arterial erethism to indicate the exhibition of *Aconite*. *Trinks*.)

As regards the miliary eruption, the symptoms, especially the nervous ones, are often still more stormy; the sufferings referred to the cardiac region (especially the intolerable anxiety, *Trinks*), groaning, sighing, very sour, mouldy smell, itching of the skin not much relieved by perspiration.

Wolf rightly remarks that here *Bryonia* agrees with *Ipec.*, also with *Carb. veg.* and *Kali carb.* (In such cases I prefer *Ipec.* on account of its greater similarity and the rapidity of its action. I have no experience on this subject with regard to *Carb. veg.* and *Kali carb.* *Trinks*.) Wolf gives *Ipec.* when the febrile state is not well marked, when there is a feeling of tension and constriction in the chest, slight heat, even partial chilliness, frequent sighing, and greater expansion of the chest; cramp-like accessory symptoms increase the indication for *Ipec.*

Certain pains (of what kind?) in the serous membranes.

This is a symptom which he has no cause to remember with pleasure: in weakly, unenergetic patients there often occur unexpectedly extremely violent shooting or cutting pains, apparently seated in the pleura or peritoneum. As external contact is very painful, we might be disposed to consider them inflammatory, but their sudden appearance,

disappearance, and recurrence in the same or other parts, show them to be of a different nature; the patient earnestly begs for relief, and *Bryonia* often assuages them, but without doing any further good, for these pains are a very ominous precursor of an unfavourable termination. (This I can unfortunately confirm: I have met with these pains in the peritoneum only, never in the pleura, and they point to that fatal peritoneal inflammation that occurs in consequence of the escape of gaseous or other fluids from the intestinal canal into the abdominal cavity, owing to perforation of an intestinal ulcer. Trinks.)

Rheumatic pains. The febrile states have often a predominant rheumatic character, in which the pains completely cease on the ninth, sometimes even on the seventh, day, and a complete nervous or abdominal typhous process commences. The symptoms of the first period are frequently of a kind that indicate *Bryonia*, especially when irritated states of the serous membranes are present, so, also, when various signs of the subsequent nervous condition are detectable. (No doubt such rheumatic catarrhal states sometimes are seen; to a certain extent they mask the typhus that has simultaneously come on; but they do not belong to the typhus, but must always be regarded and treated as independent morbid states. Thus also I have seen that a peritonitis contracted by a chill masked and concealed the typhus, which became obvious when they were cured. Trinks.)

Fainting. (I have never observed this during the period of typhus when *Bryonia* was suitable, nor for other reasons can I look upon it as an indication for this drug; especially where it is only a sign of transient weakness. Fainting depending on direct and persistent great weakness requires other remedies. Trinks.)

Involuntary, frequently recurring movements of periodical occurrence, which have the appearance of spontaneity, such as sitting up and lying down. They are disagreeable to the patient himself, as they fatigue and exhaust him.

Quickened, irritable pulse, influenced by speaking, moving, and other things, not very strong, but still not too

depressed pulse, though the latter is not worth attending to when other signs show the strength to be tolerably great. (The pulse in typhus is much affected and increased in rapidity by speaking and moving, especially by rising up and lying down. Trink.)

Warm, dry, or perspiring skin. *Bryonia* is not indicated in very copious, weakening perspirations.

Drying of the formerly moist skin with increased discomfort and gloomy disposition. At the commencement of the disease rigor, sometimes also at later periods.

These observations of our lamented colleague are well worthy our attention, for he was the first who, on rational empirical grounds, spoke in favour of the employment of *Bellad.* and *Bryon.*, and we must always regard them as a valuable contribution to a monographical nosology and therapy of typhus, although they are deficient in the requisite precision and distinctness, on which account we have made free to add to the special indications given by him our own remarks derived from our own observations.

We will now attempt to lay down the general and special indications for *Bryonia* in typhus, founded on numerous observations and extensive experience. In doing so we will naturally always bear in mind its peculiar physiological effects, whose general character, as Wolf rightly recognised and pointed out, manifests itself in the simultaneous excitement of the nervous and vascular systems, with greater or less participation of the serous and mucous membranes.

In accordance with this peculiar character of its physiological effects, it is indicated in the following principal forms of typhus.

1. In the form *febris nervosa versatilis*, with little or no participation of the mucous membrane of the intestinal canal, the *febris nervosa cum erethismo* of the older writers.

2. In *typhus abdominalis*, in the narrower acceptance of the term, in the *febris gastrica* and *biliosa* of the older writers, a denomination which has come more into use of late; a form in which the typhous process is first localized on the mucous membrane of the bowels, and only at a later period

attacks the nervous system, when this is not prevented by the resources of our art.

The febris nervosa versatilis, or cum erethismo, either comes on at first as such, or is developed from the so-called febris nervosa inflammatoria, and that after the expiry of the first period of seven days; in both cases the phenomena were the same, and gave tokens of the simultaneous increasing excitement in the brain and arterial system.

The particular symptoms that distinguish this condition of irritation, and at the same time indicate the use of *Bryonia*, are the following :

After a longer or shorter period of great weakness, bruised feeling and repeated chilliness, the disease commences usually with moderate rigor, soon followed by increased temperature and perspiration. Along with the vascular irritation there occur vertigo, heaviness and compression of head, aching and shooting pains in sinciput and occiput, irritable, irascible, quarrelsome humour, often shown in outbursts of rage, restlessness of body, jactitation of the limbs (in some cases there are no headaches); the impressionability of the nerves is obvious; the hearing is more acute; the eyes very sensitive to light, the pupils contracted; sleeplessness, with great desire for sleep, comes on; the patient repeatedly falls into a slumber, but soon wakes up, in consequence of visions and hallucinations of the senses, which persist even when he is quite awake, and increase to delirium, which generally has a reference to the ordinary occupation of the patient, which he thinks must be attended to; the white of the eye is, at the same time, moderately injected, the face red, the mucous membrane of the nose dry; the tongue clean, bright red, the desire for refreshing drink is augmented by the increasing dryness of the lips, mouth, and fauces; the taste is not materially altered, the operations of the stomach and bowels as it were suspended. The urine, in the beginning crude, depositing a sediment after standing, subsequently remaining clear and with a sour reaction; the respiration not obviously accelerated; sometimes dry tus-siculation from irritation of the mucous membrane of the larynx; the pulse, not hard but rapid, 100 to 120 in the

minute; the muscular power energetic, not diminished, the skin more dry than moist, its temperature rises correspondently to the increased pulse.

In these irritated erethic states *Bryonia* often acts in a wonderfully curative manner, indeed truly miraculously in the excited activity of the brain and arterial vascular system. The heart's beats and pulse fall (from 120 to 100, 90, and 80 in the minute) and the flow of ideas and pictures of the phantasy cease during the waking state, and only reappear in the dreams of the now quieter and longer continued sleep. The attack of the disease on the brain is beaten back, and the typhus is either extinguished or runs its course quite mildly as enteric fever with its well known symptoms; or in the other case this erethic condition develops itself out of the febris nervosa inflammatoria in which form it originally appeared, and that without the aid of art, or when by the employment of *Aconite* and *Belladonna* the inflammatory condition can be removed and the purely nervous element comes into play. The symptoms naturally are and remain the same, and demand the energetic employment of the now indicated *Bryonia*.

I must allude to two other conditions which arise during the first stages of typhus and, to a certain extent, mask and conceal the latter, which are no doubt regarded by the physiological school as typhous forms or processes, but which I have not yet been able to recognise as such; I mean the bronchitis and the rheumatic pains in muscles and joints.

I have observed bronchitic phenomena very often in the first, more rarely in the second stage of typhus, and where they were present, their exciting causes, such as chill when the body was heated, or the influence of a sudden change of temperature on the patient, were ascertainable. They never reached any great height, and were always removed by the employment of a suitable remedy, after which the typhus continued its further development, either on the brain or on the intestinal mucous membrane. In the cases that fell under my own observation, I noticed more or less violent, dry cough, the irritation causing which was felt low down in the chest, fine dry râles, whistling and

wheezing, feeling of weight under the sternum, difficult respiration, &c., symptoms that are also found in ordinary inflammatory catarrh of the air passages.

Bronchitic affections of moderate degree always lie within the sphere of action of *Bryonia*, whereas those of greater severity only yield to the employment of *Merc. sol.* or of the more powerful *Phosphorus*. I always found *Bryonia* sufficient in all cases where those affections occurred alone with the development of typhus; and I noticed that the further development of the typhus was at the same time immediately affected, or was so changed as to demand the use of other remedies.

So also I have seen cases of typhus which came on as so-called rheumatic fever, with wandering, shooting, drawing, and tearing pains in the muscles and joints, and thus rendered the diagnosis of the typhus difficult as long as they masked its essential symptoms. I here distinguish the true rheumatic affections from the painful feeling of being heated, the painful weariness, which precede by a longer or shorter time the outburst of typhus or accompany it, and as the disease progresses, give place to the peculiar weariness and debility.

These more distinct rheumatic pains in muscles and joints were, in all the cases I observed, produced by specific causes, like the bronchitic affections, after a chill to the heated body, after a sudden wetting, &c., and yielded either to *Aconite* or, when they were obstinate, to *Bryonia*.

I have not deemed it advisable to give *Bryonia* in the majority of the cases of miliary eruption that ensues, because neither the time nor the conditions of its appearance appeared to me suitable for its employment. In Dresden the eruption seldom occurs before the tenth, generally not till after the fourteenth or fifteenth day of the disease, and then either without any symptoms or with symptoms of such violence as to require the use of other remedies. In the former case, when the eruption occurs with only slight transient vascular disturbance, it is scarcely necessary to attach any great value to its appearance. But when its outburst occurs amid greatly increased vascular commotion,

great restlessness, constant tossing about, much disagreeable sensation of heat with profuse perspiration, very much accelerated heart's beats and pulse, hurried respiration, great oppression of chest and precordial anxiety, groaning and sighing, debility and prostration of strength, &c., I then generally have recourse to *Ipec.*, *Carbo veg.*, or *Arsenic*, whereby the threatening paralysis of the heart is warded off in many cases, a catastrophe that not unfrequently occurs under such circumstances.

I cannot attach any importance to the eruption in the point of view of prognosis; this is influenced more by the circumstances under which it occurs and by the symptoms that attend its outbreak. Even repeated outbreaks of the eruption, as I have often witnessed, are frequently without any appreciable effect on the further progress of the disease.

In most cases *Bryonia* shows itself of marvellous efficacy; it brings down the rate of the pulse with great certainty after the first twenty-four hours, diminishing the tenderness of the bowels and the frequency of the peasoup-coloured stools mixed with shreds of mucus; in the course of forty-eight hours the attack on the sensorium is often beaten off, the delirium with increased sensitiveness of the organs of sense as if charmed away, quiet sleep sets in, the circulation loses its rapidity, and the typhous process becomes limited to the mucous membrane of the bowels, in which it runs its course of longer or shorter duration without danger and without further disturbance of the cerebro-spinal nervous system.

In certain rare cases *Bryonia*, like other remedies, as *Hyos.*, *Stram.*, *Opium*, &c., is of no avail; the delirium increases to a furious extent, whilst the circulation rises to the highest degree of quickness, and the typhus then proves fatal by the advent of paralysis of the brain, just as happens in malignant scarlatina.

These are mostly cases in which the typhus makes its chief onslaught on the sensorium, and by the employment of this remedy the development of the typhus cerebri can in many cases be arrested, and its localization in the intestinal mucous membrane be determined and promoted.

Calcareo carbonica

is a remedy to be borne in mind on the occurrence of the miliary rash. Wolf says he has seen it of use in threatened pulmonary paralysis during the eruption.

Camphora.

In typhous diseases we should remember this remedy when it becomes requisite to strengthen and raise without loss of time the rapidly sinking organic vital force. These are extreme cases distinguished by excessive weakness, cold sweat over the whole surface of the skin, rapid sinking of the cutaneous temperature, especially of the limbs, with small, scarcely perceptible, uncountable pulse, great nervous restlessness of the body and limbs, pinched features, coldness and sharpness of nose, coldness of mouth, automatic motions of the muscles, confused fancies or attacks of faintness, constant longing for sleep, from which the patient soon awakes, great thirst with red dry tongue, frequent involuntary stools, with loud borborygmus in the bowels. In a few cases of such a state I have seen great curative effects from rapidly repeated doses of *Camphor* (1st dilution); it allayed the storm in the nervous and vascular system in a short time, and raised the sinking and exhausted vital force, which then could be beneficially acted on by restoratives of longer action, as *China* and *Arsenic*, which changed the course of the typhous process so that it was unattended with danger. In such threatening conditions it resembles *Carbo veg.* in its effects, only those of *Camphor* are incomparably quicker than those of *Carbo*. I have also seen it equally serviceable in similar cases of collapse occurring in the course of malignant scarlatina. The rapidly increasing coldness of the body always indicates *Camphor*, whereas burning heat of the skin under similar circumstances points to the administration of *Moschus*.

China

can only be regarded as an intercurrent remedy in typhus abdominalis, in order to strengthen the reaction in the

sinking forces and to raise the vital powers (especially in cases of excessive loss of fluids, copious diarrhœas, hæmorrhages, &c.), and to quicken the sensibility for the action of the medicines.

Digitalis,

in doses of two and a half grains, brings down the temperature and diminishes the rapidity of the pulse even to below the normal standard. This depressing effect is first shown in the pulse; it comes on in from half a day to several days, or after leaving off the drug. The duration of the effect is various, sometimes a few days, sometimes weeks. We are further told that "100 to 200 grains in from two to four days rapidly effect a change of temperature. It is dangerous to give more than 200 grains in two days, at subsequent periods from forty to sixty grains suffice. There often occur nausea and scraping in the throat, and an exanthema (of what kind?) breaks out. It stimulates the action of the urinary, cutaneous, and intestinal glands, by the excretions from which the temperature is still further lowered." (Ferber and Virchow, *Arch.*, 1864, Pts. 3, 4, p. 290.) Traube was the first to establish the power of *Digitalis* to lower the temperature. Wunderlich and Thierfelder tried it in typhus.

Employment.—Mucous fever (Hartmann).

Febris gastrico-venosa (Rückert).

Febris nervosa, symptomat. ptyalism during a nervous fever (Lobethal).

Typhus, with bad smell from the nose and hardness of hearing (Kammerer).

Gelsemium nitidum

was employed by Dr. Kersten in typhus (gtt. x in aq. destill. ʒij), with success against the headache and delirium, which were much alleviated by it (*Allg. Hom. Ztg.*, B. 65, No. 22).

Glonoïn

was used by Dr. Mahir, of Munich (*Allg. Hom. Ztg.*, B. 60, No. 12).

Helleborus niger.

In my case-books of the last decennium I find some cases of typhus recorded, in which this drug was rapidly and decidedly curative; their symptoms precisely resembled the picture Hahnemann draws with such a masterly hand of the physiological action of this plant, and which he characterises as paralysis of the intellectual organs. A. Kölliker also (*Phys. Winke über die Wirk. einiger Gifte*, in Virchow's *Archiv*, x, 1856, p. 235) observed paralysis of the brain produced by *Helleborus niger*.

The typhus was of the following description:—Facies quadrata, stupid, dull expression of the face, which was not fallen in, inexpressive stare of the eyes, with dilated pupils, constant somnolence, from which the patients could be awakened, but when awake they had not their full consciousness, stared at the doctor with widely opened eyes, without answering his questions correctly and rapidly, but they must think for a time; the faculties were only available slowly or not at all; the patients manifested no desire for anything, but when left alone fell into a slumber, in which they lay on their back, drew their thighs up to their abdomen, and worked down to the bottom of the bed. At the same time the affections of the mucous membrane were very moderate or quite slight, the abdomen not distended, free from pain, no diarrhoea. In some the urine was passed involuntarily; in all, however, there was no increase of the activity of heart and arteries; the pulse was generally as low as 80, respiration correspondingly slow, temperature of skin nearly normal, no miliaria, no sign of putrid decomposition of the blood, emaciation very slight; only the vitality of the brain seemed to be affected, and that without the vitality of the blood having anything to do with the production of this state.

By the steady use of this remedy (two drops of the 2nd dil. every three hours) there occurred, after from five to seven days, a freer use of the mental powers; it was as if they had been freed from a great burden that had been weighing on them. The mental faculties became capable of

being exercised, the patient was able to think more rapidly, the consciousness became clearer and freer, and he rapidly passed into convalescence. Whether serous exudation had occurred in these cases could not be ascertained, as they terminated in recovery. I find it noted that in these cases there were no previous violent pains in the head; they were young men of between twenty and thirty years of age, of good constitution and well nourished.

Helleborus also showed itself very useful in the œdema of the feet so often accompanying the convalescence of typhus, as also in the cutaneous dropsy, which I have two or three times met with in the convalescent stage.

Iodine.

Welbrand was the first to recommend it in typhus; it is said to have caused considerable lowering of the temperature and to have prevented the formation of sordes on the lips and tongue. According to Zorn's experience this pretended action on the morbid process is without foundation. (*Petersb. Med. Zeitsch.*, 1867, pp. 247-282.)

Kreosote.

Experience has taught us that this substance is a valuable hæmostatic in profuse passive hæmorrhages, epistaxis, hæmoptysis, and hæmaturia; and we have also used it in profuse hæmorrhages from the typhous intestinal ulcers with good results. When the thin, or it may be more consistent, evacuations from the bowels only show a bloody discolouration, we find *Arsenic* suffice to remove this symptom; but we have only used *Kreosote* in two or three cases, where much blood, often in clots, was found in the diarrhœic, thin, very fetid stools, and where the evacuation was always followed by great prostration.

In such cases we give, every hour, two drops of the 3rd dilution of *Kreosote* with the most rapid benefit.

Lachesis.

The want of a good and sure preparation is probably the reason that this powerful agent is not so often employed as

it, no doubt, deserves to be. Another reason undoubtedly is the imperfect and defective knowledge of its physiological effects. The effects observed from the bite of the reptile must still be regarded as the only positive knowledge we have of them. The provings made by Hering and other American physicians furnish results of such a character as no conscientious practitioner could make use of as the basis of an indication.

If we are not mistaken, *Lachesis* should be of great use when the typhous poison threatens with paralysis the brain and spinal chord; therefore, in the so-called status nervosus stupidus, with sopor, coma, dorsal decubitus of the patient, difficult recovery to consciousness and then only for a moment, difficult speech, commencing paralysis of tongue and jaw, collapse, facies hippocratica, thin, thready, very quick pulse, paralysis of the sphincters, &c.

2. In typhous states with rapid chemical dissolution of the fluid parts, that is to say with septic character. In such cases it is after *Arsenic* one of the most powerful agents.

3. In those cases, when along with an apparently mild and normal course, violent symptoms without any premonitory signs set in, the patient is suddenly attacked with delirium, which, in a short time, assumes a furious maniacal character, and is speedily followed by death in the midst of convulsions and paralytic symptoms. In such cases it may be recommended on account of the rapidity of its action.

Laurocerasus,

I have only used this remedy in a few cases of typhus. In those cases there was marked affection of the spinal chord in the form of clonic convulsions of the upper and lower limbs, which lasted sometimes long, sometimes but a short time, but recurred frequently and led one to apprehend the occurrence of a paralysis of the spinal cord. During the convulsive fit the consciousness usually remained undisturbed, and the patients only complained of an excessively annoying paralytic weakness of the limbs. They had no pains in the spine nor were the vertebræ

painful on the strongest pressure being applied to them. This paralytic feeling in the limbs drew my attention to *Laurocerasus*, this symptom being one of its most peculiar effects, otherwise I would have selected *Stramon.*, which, like *Laurocer.*, causes clonic convulsions of the muscles, which receive their innervation from the spinal cord.

In a young girl, aged 18 years, paralysis of the lower extremities had already set in, whilst the upper extremities and the muscles of the trunk were still affected with convulsions; at the same time she lay in profound stupefaction. In this case even *Laurocerasus* was ineffectual. The paralysis quickly spread upwards, the arms also became paralysed, œdema of the lungs came on, and she died.

In two boys, between the ages of ten and twelve, and in three women of twenty years old, these clonic convulsions disappeared after the administration of *Laurocerasus* (three drops of the 2nd dil. in water every hour) in the course of from six to ten hours, and the disease pursued its quiet untroubled course.

Mercurius solubilis.

Fortunately the time seems to have passed when, according to Lesser, the allopathic practitioners thought they had discovered in *Mercury* a panacea for typhus, and in allopathic periodicals we much oftener read warnings against than recommendations of this metallic drug in typhous ailments.

What I have witnessed of the employment of *Calomel* in typhus, was certainly not favourable to it; in almost all cases I noticed great and rapid sinking of the strength, no diminution of the affection of the intestinal mucous membrane, or of the fever and the delirium. On the whole, I know of no drug that so permanently damages the digestion and assimilation as does *Calomel*, which can be best seen in children to whom *Calomel* is given for local inflammation of the mucous membrane of the bowels or respiratory organs.

I must confess that I was long deterred from the employment of *Mercury* by such observations. But remembering

the old maxim, that the abuse of a medicine should not deter us from its judicious use, I recommenced to prescribe it in those cases to which it seemed to correspond in its physiological effects, and I have reason to be satisfied with the results.

The effects of *Mercury* on the whole mucous membrane of the bowels are so strongly marked that they cannot be overlooked.

Mercury is indicated in the following symptoms:—Dulness of the intellectual faculties, disposition to sleep, weight and confusion of the head; tongue thickly covered with a dirty-white fur; sickly, slimy, or nasty taste (desire for strong, piquant, or sour things, beer, wine, fruit, &c.); considerable thirst; painful sensitiveness of the precordial, hepatic, umbilical and ilio-cæcal regions; bilious, slimy, or watery diarrhœa or absence of stools; prostration of the strength; sometimes copious, debilitating perspirations; paleness of face and pinched features, sunken eyes, also dirty-yellow complexion. All signs of well-marked status pituitosus.

Typhus with predominant affection of the liver, which is painful to the touch, tender and swollen, with inflammatory irritation of the peritoneal covering of the bowels, and hence great tenderness of the abdomen to the touch, where this does not yield to *Bryonia*. Occurrence of jaundice in the second or third stage, or jaundice as the after-effect of typhus.

Mercury must not be administered in typhus too long or in too large doses, for it then tends to favour the supervention of the septic conditions.

Opium

is indicated in typhus:

1. On the occurrence of maniacal states and furious delirium, with violence, striking about, blows and cries; with great rush of blood to head and face, throbbing of the carotid and temporal arteries, redness of forehead and face, hard full pulse of 100 and upwards, restlessness and sleeplessness, and over-sensitiveness of the organs of the senses, injected conjunctiva, contracted pupils.

2. In profound sopor, as Hahnemann has pointed out.

Dr. Flamm, of Vienna, gave it in typhus in order to induce sleep, and observed that this end was attained by small doses. Latterly he only gave it at night to obtain a good night's rest, also for catarrh, diarrhoea, and hæmorrhage from the bowels. Herzfelder gave small doses of *Morphia* for sleeplessness in typhus. (*Wien. med. W.-schrift*, 1865, No. 46.)

Phosphorus.

We cannot omit saying a few words about this hero of our medicinal treasury, which deserves special consideration, not only for the pneumo-typhus in typhous ailments, for which it has long been celebrated. Our experience is not sufficiently extensive to allow us to point out all the phases of the typhous process for which it must be useful. But we are indebted to it for the cure of some of the worst and most desperate cases, in which we had vainly given *Arsenic*.

It is not capable of warding off the cerebral, pulmonary and cardiac paralyses, ushered in by severe miliary eruptions or extremely furious delirium, but it proved highly successful in some cases in which the vital energy of the animal and vegetative nervous system and of the arterial vascular system sank rapidly, the rapidity and weakness of the pulse was much increased, in which meteorism, involuntary passage of fæces and urine supervened, subsultus tendinum and floccilegium made their appearance, and the spinal cord seemed much affected.

The special indications are—

Rapid and great sinking of the strength; very weak, small, thready pulse; stupor, unconsciousness, hypnotism, profound sleep and stupefaction, delirium and floccilegium, hardness of hearing, from which the patient can scarcely be roused; dull, lustreless, half-closed eyes; sunken hippocratic countenance, dorsal decubitus; dry, immovable tongue, covered with black crust; abdomen very painful and tender to touch, borborygmus in the bowels during and after drinking; diarrhoea, involuntary evacuations of stools and urine, rattling in the trachea, threatening paralysis of the

brain, with commencing collapse, burning pains in the brain, hurried pulse.

Secale cornutum.

When the spinal cord is seriously affected, great automatic movements of the voluntary muscles, diarrhoea, &c.

Staphisagria.

Hartmann (loc. cit., p. 228) recommends this remedy in the first stage of typhus under circumstances that I have never met with in that period, and some of which do not occur in any stage of typhus, such as rapid destruction of the teeth, &c.

I have never had occasion to give it in typhus, but I am far from asserting that it can never be of use in this disease when conditions occur corresponding to its physiological effects; but it seems to me that *Staphisagria* is more suitable for chronic than for acute morbid states.

Stramonium.

Hahnemann asserts, apparently from his own experience, the curative virtues of this plant in epidemic fevers with symptoms similar to those it is capable of producing in the mind and body. But as we know that fevers with local inflammation seldom attain an epidemic extension, and then do not come within the sphere of action of *Stramonium*, those could only have been so-called nervous fevers in which Hahnemann found it useful.

We can point to several descriptions of typhous affections in which we have given this plant with good effect, such as—
1. The febris nervosa versatilis with fever of almost synochal type; quick strong pulse; high temperature of the skin; strong congestion of blood to the head; very red face, brilliant eyes; great thirst; clean dry tongue. 2. Typhus chiefly affecting the spinal chord, and giving rise to clonic convulsions, as I observed in two cases occurring in children near the period of puberty, and who in their infancy had suffered from convulsive fits. *Stramonium* acts chiefly on

the spinal chord, and is an excellent remedy in all forms of convulsions which arise from affections of the spinal chord.

The delirium in which *Stramonium* is indicated was either (1) such as had the character of monomania, and in which the patients were, so to speak, under the influence of one fixed idea, clung to it obstinately, pursued it constantly, and when for a time they were diverted from it always returned to it again ; or (2) delirium in which the patients repeated poems or single verses of poems, or sang operatic melodies the musical accompaniment of which they heard, &c. ; or, finally (3) delirium in the form of visions, in which they saw persons they knew, and conversed with them, or strange figures that inspired them with fear and horror, from which they sought to escape by creeping under the bedclothes, as in delirium tremens, with hallucinations of the senses to a greater or less degree. This latter kind of delirium in the form of visions and hallucinations often only came on in the later stages of typhus, when the circulation no longer showed signs of febrile excitement, and the intestinal ulcers were nearly healed.

Tartarus stibiatus.

This remedy I found very useful when in the first, second, or third week of the disease a severe bronchial catarrh came on, indicating its presence by moist large râles in the large and small bronchial tubes. The cough, which was more or less frequent, brought up transparent and afterwards yellow-coloured sputa, sometimes with, sometimes without, disturbance of the respiration. I did not consider the fever and the typhous phenomena in the brain and mucous membrane of the bowels as contra-indications, nor were these affected by the administration of this drug.

This bronchial catarrh threatens to produce acute œdema of the lungs, and hence demands our earnest attention in order to its speedy removal. *Tartar emetic* effected this desired object quickly and certainly, and always removed the affection in three or four days.

The medicine was given in the dose of one grain of the

2nd trit. in water, every three or four hours, and, as the moist râles diminished, every five or six hours.

Valeriana.

This is a medicine whose sphere of action must first be determined by extensive provings on the healthy, and by clinical observations.

It will then undoubtedly occupy a very important place in the treatment of typhus. Judging from some clinical observations, it was suitable in the versatile form where *Bryon.* and *Stramon.* failed to do good, but, on the contrary, the delirium became more violent, went on to constant talkativeness, to which were added subsultus tendinum and other automatic muscular movements; in some of these cases *Valerian* produced a permanent tranquillizing and ameliorating effect.

Veratrum album.

In my case-books I find but one case recorded in which, at the very commencement of the typhous disorder, the symptoms were of such a kind as to determine my choice of this medicine. On the third day after the certain appearance of the typhus the patient was seized with an attack of such copious, thin, watery evacuations, preceded by pains in the bowels, that there was an urgent call to diminish and curtail them, as they caused a very rapid sinking of the strength. This object was attained in forty-eight hours, and after that the disease, under the use of *Phosphoric acid*, ran a very mild course. This case occurred when there was no cholera in the neighbourhood, and the patient had not been anywhere in which it prevailed. Drs. Wurmb and Caspar have rightly drawn attention to this remedy in typhus occurring under the influence of Oriental cholera.

Zincum.

In recent times *Zinc*, and especially the *Acetate of Zinc*, which is regarded by Rademacher and his followers as a

first-rate brain medicine, has got the reputation of being a sort of metallic *Opium*. It is said to have a wonderfully soothing effect in states of extreme excitement of the brain in acute and chronic diseases. It is also used with good effect in typhus when there is violent, furious delirium, amounting to roaring madness; the patient jumps out of bed, tries to run away, and is with difficulty restrained and calmed. Dr. Heer, of Beuthen, asserts that he has allayed rapidly, and as if by magic, the most violent delirium in cerebral typhus by means of this preparation of *Zinc*. (*Med. Ver. Zeit. in Preussen*, 1855, No. 42.)

Hahnemann's proving of this metal is so defective and imperfect that a careful re-proving of it is urgently required. We find there this kind of specific action of this drug in the brain and spinal chord not indicated; but these observations are not to be despised because we are unable to find their simile in the physiological effects of *Zinc* we possess. The convulsions of children during teething, the epilepsy of adults, are not shown in the schema, and yet both these forms of disease are cured by *Zinc*.

Zinc proved of great use in a case of furious and maniacal delirium, in which the patient got up in his bed, tried to run away, screamed and roared, with sunken features, cold feet, and quick pulse.

All forms of typhus abdominalis present themselves to the practitioner as of slight, moderate, or severe degree. The mildness or malignity of their course is partly determined by the character of each endemy or epidemic, partly by the constitution of the patient, and partly also by the nature of the external influences brought to bear upon him.

The malignity of the affection is therefore principally determined by circumstances beyond the control of the practitioner. He has no power to destroy or to moderate the malignity of a disease prevailing endemically or epidemically, and still less to improve the bad constitution of the patient during the course of the disease. The utmost he can do is to render more favorable the external circumstances in which the patient is placed. At the commence-

ment of an endemy or epidemy the majority of the cases have a malignant character, which increases until the malady has attained to its height or greatest extent; with the decline of this the malignity also declines. Hence the greatest number of malignant cases are met with at the commencement of the epidemy; towards its end the majority of the cases run a mild course.

[It is a matter of regret that the special remarks of Dr. Trink with respect to the use of *Rhus* and *Arsenic*, two of the most valuable of our remedies in enteric typhoid, could not be found among his papers. This gives a character of incompleteness to this otherwise valuable monograph. Had he been acquainted with *Baptisia*, that excellent remedy in the gastric form of typhoid, he would doubtless have been strengthened in his opinion of the power of homœopathic medicines in cutting short undoubted cases of typhoid.—Eds.]

DESCRIPTION OF DR. CASANOVA'S TOCOLOGICAL FLEXIBLE FORCEPS.

By Dr. TUTHILL MASSY.

DURING one of my friendly or professional visits to the late Dr. Casanova he drew my attention to two woodcuts illustrative of his flexible forceps, which he felt anxious to bring before the profession in England. The illustrations were intended for a work in part printed, but not published. The chief part of these sheets, which were named *A Compendium of Tocology*, were worked into and published in his *Contributions to Physiology and Medical Jurisprudence*, which has had a very large sale on the other side of the Atlantic.

In the fifth chapter of the *Compendium*, "On Instrumental Labour," the flexible forceps is thus introduced:—

“ When medicines have failed, and the efforts of nature are insufficient to bring forth the child, instruments must necessarily be employed ; *e.g.*

“ 1st. When there are protracted unnatural presentations of the head ;—2nd. When disproportion exists between the child's head and the passages, or when there is a deformity of the pelvis ;—and 3rd. In cases of extra-uterine conception.

“ In long-standing locked-head in the pelvic strait, the head is locked or impacted when it has advanced some distance into the pelvis, and cannot proceed further ; and when it is immovable, except upwards in the cavity. Whenever the head becomes thus impacted, it acquires the form of a wedge, or like the key of an arch. In this case the

“ *Flexible forceps* should be applied as follows:—The patient lying on her left side over the edge of the bed, or on her back, as circumstances may require, you should bend one of the branches of the instrument and hold the two extremities together with your right hand ; the bent part, being pressed and guided by the left, will be introduced over the face of the child, or over the occiput, according to its position, that you may reach the inferior part of the chin, or that of the occiput. The head being thus seized, you will be able to extricate it by pulling towards you in the most favourable direction : should one branch be not sufficient, introduce the other on the opposite side of the former ; and when you are assured that both branches are properly placed, you can move them in any direction you please, and perform the necessary rotations to extract it with safety. (See Plate I, fig. 1, and its corresponding numbers.) The simplicity of this instrument and its flexibility render its application much more easy and more safe than the old iron forceps. There is no force required to accomplish this operation, nor is any compression exerted on the child's head laterally. If you be patient and watch a favorable opportunity, when pain is present, you will succeed in bringing forth the infant without any injury to it or to the mother.

“ For more than twenty years I have been in the habit of

using this kind of forceps exclusively, and the experience of that time has taught me to appreciate its utility from the successful results I have obtained with it. (See the description at the end of this work.)”

This description has not been printed, but we have got a few sheets in manuscript by which our readers can fully comprehend the utility of this discovery. Dr. Casanova writes :—

“ Description of a Tbcological Flexible Forceps and a Flexible Cephalo-extractor, showing their use and application, with engravings.

“ For want of a better term I am compelled to retain the name of forceps, though forceps, literally speaking, means a pair of pincers or tongs, differing in every respect from the one which I am about to describe.

“ The first of these instruments (Plate I, Nos. 1 and 2) can be used in almost every case in which the common metallic forceps is generally employed to remove the head when locked in the pelvic strait.

“ The second (Plate II, figs. 2 and 3) has for its object to seize the head and to extract it when it has been separated from the trunk.

“ It is unnecessary for me to praise the utility and advantages that either of these instruments have over those previously invented for the same purposes, such as the horrible crotchet, hook, or fork ; the lever, prop, or fulcrum, and the iron forceps or tongs, whose forms and dimensions have been considerably and successfully improved by accoucheurs of different nations. The successful results obtained in my practice, as well as in that of others, is a sufficient evidence in favour of the flexible forceps as to the easier application, and to the security and safety of its results, both to the mother and child, where no injury whatsoever can possibly be effected in the hands of a skilful operator.

“ It was quite accidentally that I became acquainted with this valuable discovery, and yet I cannot claim the merit of

380 *Description of Dr. Casanova's Tocological Forceps,*

PLATE I.

PLATE II.

C

F

B



originality, for I have seen, since that time, a description of a similar one published by a Spanish physician, whose name I have not been able to ascertain, though I possess his memoir on the subject, which I have obtained without title-page. In this memoir I see that the first operation successfully performed with the flexible forceps, by its original inventor, was on the 13th of June, 1796, in Barcelona, capital of the province of Catalonia, Spain, where he treated many similar cases up to the year 1798.

“ My discovery was as follows :—During a visit to a friend in the country, near the city of Manilla, Philippine Islands, in 1830, I was requested to see a native woman, thirteen years of age, who had been several days in labour and unable to be exonerated from the child. On examination I found the head was locked in the passage, owing to the narrowness of the pelvic outlet, in the form of a wedge or like the key of an arch ; that the position of the head was laterally on the right, with the face towards the pubis, and that from the weakness of the expulsive efforts of the uterus unaided nature was unable to effect the expulsion of the child. It was, then, absolutely necessary to employ mechanical means to assist nature ; to which effect I made several manual attempts to remove that impediment, but all efforts were in vain. Being destitute of instruments, and too far from the reach of any, I took a piece of green rattan, the native name for a kind of reed or genus *Arundo*, about three feet long, and reduced it to three tenths of an inch in breadth and one tenth in thickness. The natural flexibility of this material allowed me to bend it in different directions without breaking it. Having previously smoothed round its edges, and watching a favorable opportunity, I was able to introduce it into the passage and seize the head under the chin by its middle and inferior part ; but its lateral position prevented me from making any rotatory movement in order to bring it forward as much as it was required, the instrument having slipped several times from under the chin towards the throat. I was obliged to introduce another piece of rattan similar to the former, towards the opposite side of the former, seizing it round the occiput. Thus having the

two pieces introduced and placed as above stated (see Plate I, Nos. 1 and 2), to my great astonishment I succeeded in removing the said impediment and bringing forth the child alive without the least injury to it or to the mother.

“ Since that time I had several opportunities in which I have successfully employed the same method in similar cases, and also for extracting the head when separated from the trunk. Both instruments have been improved in the material, the description of which is as follows :

“ PLATE I.

“ FIG. 1—Represents the head seized with the double forceps, and the manner in which it is extricated from the pelvic strait.

“ No. 1. A single forceps seizing the head round the middle and inferior part of the chin.

“ No. 2. A do. do. seizing the head round the occiput.

“ No. 3. A frontal safety check-band, to prevent the forceps from receding, and choking the infant.

“ Each of these forceps is composed of a piece of whalebone about three feet long, three tenths of an inch in breadth, and one tenth do. in thickness ; of two silver rings at their extremities, well fastened, and the whole well polished.

“ The frontal safety check-band ought to be placed at about four inches from the centre of the forceps No. 1, on each side, and it must be well secured. A piece of silk, or any other smooth cloth, will answer. Its length is from seven to eight inches.

“ PLATE II.

“ FIG. 2—Represents the cephalo-extractor seizing the head of the infant after its death and separation from the body.

“ This instrument is made of two pieces of whalebone of the same dimensions, in every respect, as those of the forceps, without rings in their extremities. These two pieces are fixed by their centre with a silver pin riveted on both sides, flat and smooth, to allow the pieces to move, or open and close, as the blades of a pair of scissors. (See fig. 2.)

334 *Description of Dr. Casanova's Tocological Forceps.*

When closed and held by the two extremities, they represent one of the forceps. When opened, and holding the four extremities in one hand (see *a*), they represent fig. 2.

"FIG. 3—Represents the head seized in a different manner; as the diameter is generally found to be less from *A* to *B* than from *c* to *D*, I have found it much easier to seize and extract it in that way (when such a difference is clearly manifested) than otherwise.

"FIG. 4—Represents a plain silver plate of an oval shape, with three holes in its centre, to allow the four extremities of the cephalo-extractor to pass when it has seized the head. The lateral extremities pass through holes Nos. 1, 1, one by each, and the anterior and the posterior both pass through the hole No. 2. (See Fig. 2, *a*.) This plate renders the instrument open and immovable, and it serves to press the head as much as may be required, to extract it without injury to the mother."

The latter part of this description is taken from a paper read before the Medical and Physical Society of Calcutta in 1834. The woodcuts are engraved from original drawings by Dr. Casanova, who excelled in this accomplishment as well as in surgery, physiology, and pharmacodynamics.*

* As I am indebted to the *Compendium of Tocology*, I should wish to give a note from Dr. Casanova's Dedication:

"To the MANES of the venerable, the illustrious SAMUEL HAHNEMANN, the Hippocrates of the North; and to my professional brethren in every country, is this little volume most cordially dedicated in testimony of high respect and fraternity.

"To the former, as the indefatigable discoverer of a new truth and the reformer of the Medical Sciences:—the discoverer, not the inventor of truth; for truth has never been, nor can be invented. Truth, though hidden from those whose intellectual faculties were not sufficiently developed to discover it, has existed from the beginning, and will exist for ever. It is eternal like the infinitely wise and gracious God, as the Rev. Mr. Barrett observes: 'Man may disregard it for a time, until the period arrives when its rays, according to the determination of Heaven, irresistibly break through the mists of prejudice, and like Aurora and the opening day, shed a beneficent light, clear and inextinguishable, over the generation of men.' Hahnemann, whose *Intelligence, Conscientiousness, and Firmness*—true characteristics of a discoverer—has ultimately found out and developed the two great principles upon which his doctrine is founded; to wit: the *Principium vitale* as the law by which

**‘REMARKS ON SCIENTIFIC THERAPEUTICS,
BY SAMUEL WILKS, M.D., F.R.S. A LEC-
TURE DELIVERED AT GUY’S HOSPITAL.’**

Some Remarks thereon by J. H. NANKIVELL, Surgeon, York.

HAVING sat at the feet of Bright and Addison and Hodgkin in the noble hospital of Guy, we are and ever shall remain deeply interested in the medical lore of our old school ; and so, when in the pages of the *Lancet* for February 18th, 1871, we read the above heading, our curiosity and interest were deeply excited, and we naturally expected that we should find in the “Scientific Therapeutics” of these times some advance and improvement compared with those of the olden times. Dr. Wilks at the onset of his discourse declares with much candour and honesty that his method of treating disease is empirical,

Physiology is governed, theoretically ; and the law *Similia similibus curantur*, as the expression of a natural—a positive fact in Therapeutics, practically.

“‘The man that develops a principle which shall improve the condition of his race in all nations of the earth and for all generations to come, should live in their remembrance ; and he who brings to light some great principle of nature essential to the preservation of the health and life of man, and develops its laws, should command the respect and love of his day ; live in their remembrance, and for all time receive their reverence and gratitude.’

“A man like Hahnemann, in the Republic of Medical Sciences, never lived. He, like Newton in Astronomy, turned the force of his mind, characterised by powers of unequalled investigation, into the field of health and disease, with self-sacrifice, diligence, and perseverance, notwithstanding the bitter persecutions of his potent opponents to deprive him of everything that renders life precious, except honour ; and by the providence of God, he became the true interpreter of nature. *Nascitur, non fit* : he was born, not made a Physician.

“To sum up the moral and professional character of our master, no poet can better describe the qualities of such an eminent genius than the author of the following lines, though not written for him :

“‘How happy is he born and thought,
Who serveth not another’s will ;
Whose armour is his honest thought,
And simple truth his utmost skill.’”

or, in other words, that of a pretender, whose experience, as he thinks, has taught him "what remedies are most effective in arresting morbid processes," but has not taught him "any explanation of their action."

Dr. Wilks, in his *Scientific Therapeutics*, does not attempt to lay down any fixed principles of treatment, that is to say, he does not attempt to give any more fundamental teaching than that which his own experience (*res fallacissima*) has furnished him with; he virtually says that in these his *Scientific Therapeutics* he should have preferred to offer his pupils some principles based on true scientific grounds, but that it is not wise to speak of principles when framed from conclusions whose premises are altogether false." And so in fact the title of his lecture should have been *Unscientific Therapeutics*, and would have been so were it not that the term "scientific" has become in medicine that which conveys no definite meaning, or, rather, is only assumed as the watchword of a party who hold also that it is synonymous with "empirical."

Dr. Wilks proceeds with his argument thus:—"To say that I have no principles is a humiliating confession, and in declaring it I am conscious of running counter to the opinion of the greater number of our profession." There is some confusion here, no doubt, but we beg to assure Dr. Wilks, for his comfort, that he is by no means running counter to the opinion of almost every member of the profession with whom we are acquainted; and, after reading his lecture, none will be disposed to credit him with what he is at such pains to disclaim. But we proceed:—"We have no especial indications whereby we can be certain of the action of medicines in disease. The individual symptoms afford us but little aid, but we are rather guided by the *totality of symptoms*." Now, to any man acquainted with the teachings of Hahnemann this passage affords a rich study. Is Dr. Wilks in his heart of hearts a homœopath? One would be tempted to think so from the above tone of the paragraph, but he stops short of the guide afforded by the correspondence of pathogenetic symptoms caused by medicine with pathogenetic symptoms

caused by disease. We are guided by the totality of symptoms ; but how can this be when, as he says, we know nothing about the distinctive properties of drugs, nor under what circumstances these may be employed. But again, "although we are to be guided by the totality of the symptoms, we must treat the disease and not the symptoms," as if it were possible to treat the one without the other, and to discard with safety our guide Totality, when we had come face to face with the concrete being, Disease, itself. Surely by the time this lecture was finished the students of Guy's must have found themselves in a greater state of bewilderment than if they had been listening to a lecture on metaphysics.

"Our remedies were never suggested by any theoretic considerations whatever." This statement might have had some degree of truth up to the time of Hahnemann, but since his days it is utterly false, for he it was who by provings of old and new remedies discovered with certainty that they had been rightly administered in many diseases empirically, and also that they were of the greatest possible advantage in many other forms of diseases for which they had not been hitherto given, and our daily clinical experience proves that his law of *similia similibus curantur* was right and just.

Dr. Wilks proceeds to state, without any grounds for such an opinion, that our knowledge of the action of a drug on the healthy body assists us very little in the treatment of disease, and all we can do at present is to meet such an opinion with an opposite one, and to declare before the world that the experiences of the thousands of physicians of our school—aye, and of many of his own school too—prove by daily demonstration that such action affords the only true guide for administering drugs in diseased bodies. We maintain that *Opium* and *Conium* are not of less value in their own peculiar spheres of nervous disorder than *Quinine*, *Zinc*, or *Iron*, and that *Nux vomica* is *par excellence* of "great avail" in nervous diseases. But the subject is too extensive to be gone into in detail in this place.

Dr. Wilks wisely advises his pupils not to give *Opium*,
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Chloroform, or *Conium*, in tetanus, mania, or chorea. No man of our school requires this advice ; we have other and better remedies ; nor do we think that the full doses of these drugs, given so as to produce narcotism or anæsthesia, can ever—ought ever—to be regarded as a “*scientific*” treatment of such diseases ; on this point we are glad for once to agree with the lecturer. Dr. Wilks speaks in praise of *Zinc* in chorea, but he maintains that its action has never been satisfactorily explained ; if he will read our *Materia Medica* he will find ample explanation, but we have a far more reliable remedy in *Cuprum*.

Dr. Wilks, when a student, was taught when and how to relieve ophthalmias with leeches, blisters, *Calomel* and *Opium*, but his first teacher was succeeded by another who told him that he must ignore this altogether, that such “*scientific principles*” were gone by, and that he must adopt an *opposite system* !—which system he saw carried out with equally good (bad?) results. But the error of *this* last surgeon is said to be in his assuming that symptoms should be treated ; but we naturally ask how this would be an error if it was demonstrated that there were equally *good* results from the “*scientific principle*” of regarding symptoms, as from the other “*scientific principle*” of disregarding them and prescribing by a kind of blind instinct.

The sentence from the words at the top of page 225, viz. “*These notions were all based,*” &c., down to “*a little learning is a dangerous thing,*” is so confused and indefinite that it reads like an Act of Parliament ; we will try, hard as is the task, to condense it. “*The value of symptoms cannot be found—they are an unknown quantity. The notion that one can find them is based on mere theory without facts, but it is still held by many ; what we have heard from mouths and learned from books is a doctrine which you would all accept as just and reasonable were I to propound it to you (!). You have heard, e.g. it said, ‘Don’t treat pneumonia as a mere name, but treat each case on its own merits.’ I once had a half-belief that this was true, but if it is true it cannot be accomplished (!!!), for the attempt is the grand cause of the bad therapeutics of the present day.*”

But I must take you a retrograde course and teach you to regard diseases rather than symptoms, and so lead you back to pure empiricism." How a disease is to be regarded, excluding its symptoms, is to us incomprehensible, and upon this point we have no "half-belief."

"That a disease is nothing more than the *totality* of its symptoms is incorrect, but this, by-the-bye, is the homœopathic theory." Now, a few lines above Dr. Wilks says that *individual* symptoms afford but little aid, and that we should be rather guided by the *totality* of the *symptoms*. What does he mean? The much-enduring homœopath meekly observes that the totality of symptoms, physical and rational, objective and subjective, affords us a true and real picture of disease, if we have eyes to see—relates as plainly as it can do the story of the disorder, if we have ears to hear—and then it is our own fault, the unpardonable fault of our own ignorance, if we are not able to prescribe a remedy or remedies which shall relieve the patient.

"Diagnosis is founded on probabilities." This we deny. Is the diagnosis of pleurisy, pneumonia, bronchitis, croup, laryngitis, peritonitis, enteritis, to be founded on probabilities? That there are obscure cases we grant, but these are the exception and not the rule.

The therapeutics which follow are, we honestly confess, beyond our grasp; an ignorant imaginary dummy is first set up to be knocked down again, and then you are told (ye blessed happy students) that you overlook syphilis as having induced a paralysis, and don't cure it with tonics and galvanism; but by-and-by you discover the syphilis and cure it and the paralysis with *Iodide of potassium*. Now, what, in the name of common-sense, does this prove beyond the fact that in the example given, fictitious or otherwise matters not, the very main point in the disease, or the totality of the symptoms, was stupidly overlooked? Besides, Dr. Wilks must pardon us for reminding him that paralysis is not a disease so much as a symptom of disease, and that in the case he gives, syphilis was truly the disease. If a man gets loss of motion and sensation on one side of his body from a centric cause, as incipient softening in one of the hemi-

spheres of the brain, all appliances to the weakened limbs must be useless. But why argue this point? The whole subject is easily understood, and sure we are that not one physician or surgeon, be he Tyrian or Trojan, can have gone along with Dr. Wilks in his argument with the slightest satisfaction or conviction.

A case (founded on fact) of inflammation of the appendix cæci is related by the doctor, which case, he says, is treated according to the symptoms, viz. a purge to overcome the constipation, and therewith a stimulating injection. The stomach of the patient is filled with carbonic acid gas to relieve the sickness, the abdomen is rubbed with a liniment to relieve the flatulence, and the patient dies, and Dr. Wilks inserts this precious observation:—"Let us (here) see the course pursued by one who does not care to recognise the disease (after homœopathic fashion), but declares that his business is with symptoms alone." We assure Dr. Wilks that a homœopath would not set about relieving such symptoms in such an insane "scientific" manner.

Again, in the treatment of typhoid fever we do not apply "cold to the head to remove delirium, leeches to the belly to relieve tenderness, give chalk mixture to arrest diarrhœa, purges to remove constipation," or "stuff for the cough," so that all which he says about the sins committed by symptom-mongers have not the slightest application to our mode of treatment, although he would seem by implication to convey such a slur upon us.

The first part of this lecture is concluded with some sad stories of "scientific" practice, which prove one of two things—either that Dr. Wilks is very unfortunate in the class of practitioners with whom he consults, or that they afford a fair sample of the manner in which allopaths in general attempt to benefit their patients, and certainly in the presence of these scientific people Dr. Wilks shines as a diamond of the first water.

This lecture is concluded in the *Lancet* for February 25th, at page 261, and we have expended so much time on the first part that the residue of our remarks must be brief. It is clear throughout that Dr. Wilks' confession of the

hap-hazard guess-work of the scientific method of medicine, sincere and well founded as it is, will not suffice him unless he hurls his darts at a rival method, to wit, the homœopathic, which professes to have, and really does possess, a rule of practice. He says, "I deny that we have a scientific use of medicine, and therefore the experienced man is the best practitioner." So by this declaration we are to understand that every medical student has to pick up what he thinks are the best crumbs of the experiences of others, digest them as well as he can, and then turn them to account for the benefit of his patients; all is uncertainty and chaos, and the theory which is right to-day is wrong to-morrow, and even the opinions of Dr. Wilks himself may be flung to the winds as mere chaff by the next teacher who succeeds him in his chair. We have then a story of a man of no *scientific* attainments, who treated a case of mania very properly, and was superseded by a younger man of, it is to be presumed, great science, who treated the case very injudiciously, from which we are to gather that *science* is for the most part bad in medicine, and experience is everything.

With much which Dr. Wilks has written on routinism we heartily agree, and for the benefit of those people, in or out of the profession, who do not read the *Lancet* we transcribe a list of diseases which Dr. Wilks has very recently seen treated with ammonia and brandy. Here they are:—Typhus, pneumonia, delirium tremens, rheumatic fever, cynanche, scarlatina, typhoid, hepatic ascites, erysipelas, smallpox, morbus cordis, pyæmia, bronchitis, diphtheria, carbuncle, pleuritis, besides a number of nameless chronic maladies. "In three instances he had countermanded these stimulants, the cases being acute cynanche tonsillaris, and had prescribed a purge, a saline mixture with antimony, and some Dover's powders with calomel, and in all the rapid subsidence of the symptoms was most marked." Very probably, but if a third man of *experience* had been consulted, he would probably have disapproved of these prescriptions, and have given something else equally effectual.

With much that Dr. Wilks says on the use of *Iron* in chlorosis and anæmia we heartily agree, for this medicine is given in such enormous doses that it cannot fail to be injurious. In many instances it hurries the patient from anæmia into phthisis, and so makes a bad matter worse, but this is scientific practice with most men, with men who would not try one harmless experiment with the medicines and doses we use for the relief of chlorosis and anæmia, though an angel from heaven were to counsel them to do so.

With the remarks on the abuses of *Strychnine*, *Chloral*, *Opium*, *Carbolic acid*, *Bromide of potassium*, we heartily agree also, but it is amusing, when reading the accounts of the doctor's success with *Digitalis* in heart disease and *Mercury* in brain disease to read as follows:—"Remember, I say not a word against sound scientific therapeutics, for in a few cases we have a glimmering of reason associated with the mode of cure," this sentence being a reduplication of "Remember, I have nothing to say theoretically against a scientific use of medicine."

Again, "I have alluded to the homœopathic doctrine, which ignores pathology, and treats only symptoms." One has scarcely patience to read such calumnies over and over again. Is Dr. Wilks really ignorant that symptoms are an important part of pathology, and that in diagnosis the rational and physical symptoms are our guides in diagnosis as much as they are the guides of our *scientific* brethren—nay, much more so, if we may judge from our own experiences?

Moreover, all that Dr. Wilks writes about our recognition of and treatment of the symptoms of scarlatina and smallpox is false. The homœopath does not trust to *Bell.* alone for the cure of scarlatina, nor to *Antimony* for the cure of smallpox, and this denial must suffice as far as we are concerned.

But at the end of this melancholy dirge of a lecture, this Jeremiad, this Threnody (let us not rant), we have granted us a word of consolation, which in substance is to this effect—that he, Dr. Wilks, although still lifting up his voice in Cimmerian darkness, does see a ray, one glimmer,

towards which he is groping, and he hopes before he ceases teaching at Guy's to be able to adopt different language, and enforce some more *strictly scientific* rules of treatment. That this goal may be attained for the good of himself, his patients and his class, and that he may receive a true illumination, and be enabled to commence a *new era* in medicine, we most heartily and most charitably desire.

ON HÆMATIN AND DIASTASE.

By Dr. DRYSDALE.

We find the following article in the *Allg. Hom. Zeitung* of December, 1871.

“ On Hæmatosine as a Therapeutic Chalybeate. By Dr. TABOURIN.

“Hæmatosine is the red colouring matter of blood in the higher animals, which is associated with another protein substance which is colourless, viz. globuline, which, combined with hæmatosine, forms hæmato-globuline. As a protein substance, hæmatosine stands, according to its chemical constitution, next to albumen and fibrine, but is distinguished from these by constantly containing one tenth of its weight of iron. It consists of a brown metallic-looking powder, inodorous and tasteless, insoluble in water and pure alcohol, but with the addition of acid or alkali it does dissolve (also colouring the vehicle), as also in ether, in essences, and fatty matters.

“It can easily be taken as a medicine—in powder, pills, lozenges, bonbons, syrup, liqueurs, or chocolate, with spices, food, or drink.

“Having neither taste nor smell, it is taken without repugnance even by children. The stomach digests it, like all other albuminoids, without any derangement; in the intestines it is only precipitated by the alkaline fluids which it encounters there, as bile, the pancreatic fluid, and the intestinal sugar, which is also the case with most preparations of iron, while at the same time it is soluble in acid or alkaline fluids alike (*sic* in orig.); this last ensures its assimilation. In a therapeutic point of view it has the great advan-

tage of surpassing the chalybeate medicines in common use, inasmuch as it does not in the least disturb the digestion nor hinder intestinal absorption, so that it can be continued as long as ever medical indications require it. It is immediately dissolved in the blood, and recovers its fundamental properties, absorbing oxygen and assuming the bright red of arterial blood. It is also capable, in its admixture with protein substances (which impart to the blood-plasma its plastic and nutritive properties), of uniting itself with the elements of globuline, and thus becomes the basis of new blood-globules. It is this colouring matter of the globules that is the essential portion of the circulating fluid, and all physiologists agree in this, that the chemico-vital phenomena of 'the blood formation,' whether pulmonary or interstitial, turns upon this much agitated point. Hæmatosine absorbs the oxygen in the lungs, and, assuming the bright red of the arterial blood, it conveys this combustion-gas into all parts and tissues; in the latter the oxygen is used in the various oxidation-processes of nutrition, and is replaced by carbonic acid, whereby the blood loses its lively colour, to assume the brown tint of the venous blood. The colouring matter of the globules accordingly plays a highly preponderating part in the closely allied functions of respiration and nutrition. This explains the languid and feeble condition of persons whose blood is deficient in globules, as in anæmia and chlorosis. Hæmatosine contains 10 per cent. of iron. The chemists of the new school recognise in iron various grades or stages of atomicity; in some combinations it is *biatomic*, in others *tetratomic*, differences which are expressed by the terms 'Ferrosium,' and 'Ferricum.' At this moment we are not aware in which of these states it occurs in the blood.

"In ordinary 'chalybeates' one is never sure of giving the iron in that molecular condition which the blood requires for the reconstitution of its globules. If, instead, hæmatosine is given as a chalybeate, one is quite sure that it is offered to the system in the molecular condition which the blood demands, being taken from the blood itself. Its administration can consequently be supported on physiological principles. In a similar manner we arrived at the knowledge that *Pepsine* is the best means for curing dyspepsia—that phosphorised fat, taken from the nervous centres, is the most certain remedy for feeble innervation. It is prepared by softening and working up clots of blood with a solution of some inoffensive coagulating salt, and then subjecting the so-formed paste to a

strong pressure. The cake obtained is broken in pieces and digested with alcohol, and two or three hundredth parts of any acid added to it. The hæmatosine dissolves in the alcoholic fluid, while the globuline remains undissolved and falls to the bottom of the vessel. The highly coloured fluid separated from the precipitate after neutralization deposits copious red flakes. This is the raw "hæmatosine;" it is put in a filter and washed out, little by little, with water, alcohol, and ether. This then gives pure hæmatosine, which only requires to be dried and powdered."—*Bl. f. Heilwissenschaft*, 1870, 18).

Remarks on the foregoing, by Dr. Drysdale.

The hæmatosin here spoken of is the same as hæmatin of other authors, and is the substance imparting the colour to the blood, and in this form containing all the iron as far as is known. But the iron is in no way the cause of the colour, as it may be entirely removed by digesting with *Sulphuric acid*, after which the solution has the same colour as before to the eye, although there is a difference to the spectroscope. It appears, according to *Kühne's Physiologische Chemie*, that the blood-corpuscles consist of a solid stroma, whose constitution is as yet only partially known, but in which the chief constituent is protagon along with a smaller portion of the paraglobulin of A. Schmidt, the active fibrino-plastic substance. Besides this stroma, the blood-corpuscles contain, combined with it in a way not yet known, the colouring matter, viz. the hæmoglobin, a crystallizable substance easily separable in the blood of many animals. The hæmoglobin in the complete corpuscle is very different from the hæmatin spoken of above, and is a very complicated substance, with a very high atomic weight (13,280). It is easily decomposed by the stronger acids and alkalies, and many salts, and splits up into hæmatin, and not one, but several, albuminous substances and several acids. The chief among these albuminous substances is often called globulin, but incorrectly, and for the present it must be considered an albuminous body, *sui generis*, and it cannot

be said that globulin can be got as a product of the hæmoglobin. Kühne here says that A. Schmidt was in error in ascribing fibrino-plastic properties to the hæmoglobin. It is true that the paraglobulin of the stroma frequently adheres to the hæmoglobin, and thus gives it the appearance of being fibrino-plastic, but he always found well-washed hæmoglobin had not that property. One hundred parts of hæmoglobin contain only about four parts of hæmatin, and the other ninety-six consist most probably of the above peculiar form of albumen, which differs essentially from ordinary albumen, and all the usual tests for that substance give negative results unless they have also the power of splitting up the hæmoglobin. The optical properties, even to the naked eye, but more accurately by the spectroscope, show that the hæmoglobin is the true colouring matter, and pre-exists in the corpuscles as such. The bright colour of the hæmoglobin, and the corresponding bands seen by the spectroscope from the absorption of oxygen, can easily be removed by various reducing agents, while by shaking up the reduced hæmoglobin with oxygen it can be again converted into the bright-coloured oxy-hæmoglobin. This experiment can be repeated many times at will.

But it is different with all agents which decompose the hæmoglobin and cause the formation of hæmatin. The optical signs of this latter substance are quite distinct, but, contrary to the opinion of Stokes, the hæmoglobin cannot be restored from hæmatin by reducing agents.

From the consideration of these facts we may see at once the extremely improbable nature of the hypothesis of Dr. Tabourin, which is much the same as Bence Jones's, that by its mere chemical properties the hæmatin, even if immediately taken in solution into the circulation, could unite itself with the elements of globulin and become the basis of new blood-globules. This we must consider as extravagant as the idea that by eating ready made bone, muscle or membrane, we could renew those parts within us. There can be no doubt that the hæmatin is assimilated and the blood-corpuscles formed by a purely vital process, like other tissues and parts. But the suggestions here made

are by no means destitute of practical value, as we know that relative facility of assimilation is as important in judging of the value of a nutritive substance as its chemical composition, if not, indeed, more so in many cases. We do not, indeed, know whether the iron in the hæmoglobin is in the state of ferrosium or ferricum, nor do we even know whether in the prepared hæmatin it is in the same state as in the hæmoglobin, nor that in ordinary chalybeates it is *not* in the same state, but, as our author says, the presumption is certainly in favour of the hæmatin containing the right form. I do not understand the statement that it is precipitated by the alkaline fluids, while it is soluble in alkalies and acids. As a matter of fact, out of the body hæmatin is insoluble in water, alcohol, ether, or chloroform, but easily soluble in alkalies, in ammonia, and in acids; so the probability is that it is the most easily digested of all the preparations of *Iron*, as said by our author. I think, therefore, that we ought to adopt it in practice, if the dispensing chemists can furnish us with a good and palatable preparation, in all cases where we require iron as pabulum alone. Since writing on this subject, about two years ago, I have been in the habit of giving two grains of *Lactate of iron*, either alone or with eight or ten grains of *Pepsin*, at the beginning of dinner, in the form of "dinner powders." At the same time any other medicine is given at a different part of the day on an empty stomach. I find this plan successful in curing cases of chlorosis more quickly than by the specifically indicated medicines, which act as vital stimuli alone, because, as before said, the feeble powers of digestion cannot assimilate sufficient bulk of food to extract the extra quantity of iron. As the process of cure is in the main the restoration of the vital powers of assimilation, the mere furnishing the requisite quantity of iron is seldom sufficient of itself. As to the quantity required for this purpose we have here some data. The iron forms $\frac{1}{10}$ of the hæmatin, which itself is only $\frac{1}{100}$ of the blood-corpuscles, and the last about $\frac{13}{100}$ of the mass of the blood, in all, $\frac{13}{1000}$, or about $\frac{1}{1933}$; so that one grain of iron would furnish sufficient iron for 1928 grains of blood, that is, about

4 oz., if all assimilated and converted into blood-globules. The formation of 4 oz. of fresh blood per day would be a very rapid cure of chlorosis, so we can easily perceive the enormous surplus of *Iron* that is usually given as so-called chemical food in these cases, and we can also imagine how such a surplus must be hurtful as an irritant and spoliative to the *prima viæ*, and as a specific stimulus on the general system. As it is plain that by far the greater part of the iron usually given is not assimilated as pabulum, we ought to choose the form most likely to be digested and taken up by the subsequent vital process of assimilation, and certainly it is reasonable to suppose that the hæmatin would be so, and it might be given at meals in doses of ten or twenty grains. But the question will still remain in many minds, why we should need to give iron at all as pabulum, when it is already contained in sufficient abundance in all the common articles of diet? This question I endeavoured to answer in the article on "Analeptics," in this Journal, vol. xxvii, p. 297. I would, in addition, make a few remarks on the extreme tenacity with which the inorganic constituents are retained in the body, as well as the wonderful power by which all excess of them is quickly removed by the organs of excretion. In the case of common salt, and most of the inorganic salts of the urine, the quantity excreted corresponds with that contained in the food. In absence of salt in the food, the excretion of it sinks to almost nothing, although it never entirely disappears, and thus a certain drain reducing the quantity below the proper needs of the system is made, and important changes in nutrition are soon manifested. According to Wundt, even on the third day after total abstinence from salt in the food albumen begins to appear in the urine (Kühne, p. 581). When sufficiency is again given the normal quantity does not appear at once in the urine, but only after the salt deficit has been made up throughout the fluids of the body. The same phenomena, no doubt, take place with respect to the iron and the other inorganic elements, although they have not been so accurately noted experimentally. So when the animal is in health, a pretty wide margin of variation

of quantity of food, or even proportion of iron in the food, may take place without change of composition of the tissues or blood. The quantity excreted is merely retained, and thus the balance is made up. It is otherwise when disease is present, which alters the nutrition of tissue and the recombination of the blood. The iron which should have been used for these purposes is then, for the time, in excess of the normal quantity in the blood, and is immediately removed; and if this has gone on long enough, when the vital powers of nutrition are restored the quantity of iron in ordinary food is insufficient to restore the normal proportion without the retention of the whole for a long time, while at the same time the digestive organs are in a very unfavorable state for the consumption of a large bulk of food. Iron is contained, besides, in the hæmoglobin, in the chyle, the bile, the lymph, the milk, the liver, the spleen, the choroid, the brain, the muscles and muscular juice, and the gastric juice. To supply all these (except the chyle soon after a meal) it must be (and is) contained in the plasma of the blood, and from that source the greater part of the red blood-globules must be supplied, viz. all those that originate in the white corpuscles of the lymphatic and conglobate glands. Whether the white corpuscles formed by the lacteals take up iron directly from the chyme is not stated, nor have I been able to meet with any analysis of the white corpuscles distinct from the red. On the destruction of the red corpuscles, which takes place largely in the liver and spleen and other parts, the iron is, no doubt, restored to the unknown state of combination in which it exists in the plasma, and is thus used to a great extent over and over again in the decomposition and recombination of the blood and other parts into which it enters. All these processes are strictly vital, and therefore under those influences which control vital action, such as the natural and preternatural stimuli which are chief conditions of health and disease, and compared with which chemical alterations in the proportion of the components of our food play a very subordinate part. In the case of chlorosis I do not know that it was ever found to arise from a deficiency of iron in the food, but its causes

are alterations of the stimulus conveyed by the nerves of organic sympathy from disordered states of the ovaries or uterus or the brain, induced by cold or mental emotions. These act rapidly on the vital actions on which recomposition of the blood depends, and the chlorotic state and deficiency of iron may be developed even in a few days. It is plain, therefore, that by far the largest part of the treatment of such a disease must be by specific stimuli, which may counteract the derangement of vital function, as well as by general hygienic measures, and that a comparatively small part of the field is open for treatment by furnishing an extra amount of the defective chemical element. That small part must, however, not be overlooked, and therefore the use of *Iron* as pabulum should be conjoined with that of the specific stimuli, such as *Pulsatilla*, *Cyclamen*, *Ignatia*, *Senecio*, *China*, *Ferrum* itself, *Manganese*, *Squilla*, and other medicines chosen according to their pathogenetic action.*

On Diastase.

Another chemical aid to digestion has been brought under notice by Fonssagrives in his work on *Hygiène Alimentaire*, viz. *Diastase*. We are all now familiar with the use of *Pepsin*, but, as it is used chiefly with the albuminous principles, there was a want to be supplied. On this point our author observes, "*Diastase* has been lately pointed out as a means of favoring the assimilation of the feculent principles, and is a condiment analogous to *Pepsin*, and which acts upon the amylaceous aliments as the latter does upon the meat kinds. I have observed a very convincing case of the utility of this substance. A young medical officer of the navy, who had brought back from the colonies the remains of a chronic dysentery and a painful dyspepsia, consulted me in 1860. I ordered large doses of *Nitrate of Bismuth*, and one gramme of *Pepsin* daily, along

* I have requested Messrs. Thompson and Capper to prepare the *Muriate of hæmatin* by the process described by Kühne, and if they are successful I intend to use it. I hope other practitioners will do the like and report the results.

with his meals of animal food. Under the influence of this treatment the animal food was digested well, but (as usually happens in chronic dysenteries) bread and other farinaceous substances caused flatulence and heaviness in the stomach, and the remains of those articles of diet were rejected by the stools. He then took it into his head to resort to *Diastase*, and as a first test he tried a meal of haricots, one of the most indigestible things of the kind. His stomach bore itself well under this hazardous experiment, which, however, he repeated several times, and found that the favorable action of the *Diastase* was beyond question" (p. 264). Dr. Fonssagrives then recommends this as a general means for the above indications. The form used was the "tablettes de Peuvret." After trying to get any preparation of *Diastase* in this country without success, Messrs. Thompson and Capper procured these tablettes from Paris, and I have ordered them frequently within the last two years. I think I can report favorably of their action, and when the supply was exhausted, and could not be renewed during the late siege of Paris, I have found the want of them in several cases.

The importance of the action of the *Ptyalin* in changing the starch into sugar is obvious in all animals whose food is largely farinaceous; and if *Diastase* can even for a time make up for any defects in the salivary secretion, it must frequently be of use in our practice. Of course we do not rely on such as it or *Pepsin* for the actual cure of the diseases, but it is something to tide over a difficulty, and especially in convalescents to help ever so little in respect to the quantity of food assimilated.

ON SOME CASES DIFFICULT OF DIAGNOSIS.

By Mr. HARMER SMITH.

(Read before the British Homœopathic Society.)

I do not use the word difficult in regard to several of the following cases in any absolute sense. In reference to some of them, I merely suppose so much of difficulty as might raise a question amongst observers who did not care to agree. The first case which I have to refer to fully exemplifies this remark, since after the stage of invasion there was little or no difficulty in the diagnosis, and yet this was made the ground of a charge against me by two allopathic doctors, that I had completely misunderstood the case.

G. D—, æt. 11, Greenwich.

June 20th, 1865.—Walked to my house this morning. He had not been well for a week or two, had been languid and weak, and lost his appetite. Had a cold plunge bath on Saturday last, and had appeared worse since.

Present symptoms.—Heavy expression of countenance; tongue furred; thirst; skin hot and dry; pulse 110; but as he was going about as usual, I thought that these last symptoms were probably due to having walked a mile uphill in hot weather. *Arsenicum* (1 cent.), half a drop every four hours. Pil. *Nux vomica* (1 \times) ij o. vesp.

23rd.—My patient called on me again (I had heard nothing of him in the interval). The symptoms were much the same, but the languor was more pronounced, and he complained of vertigo on first awaking on a morning. Pulse 120. Continue *Arsenicum*, and omit *Nux vomica*.

The nature of the case, which I had not suspected on his first visit, was now unmistakable, and I told him to go home at once and stay in bed.

24th.—Found on visiting him symptoms of febrile reaction; skin hot and dry; face flushed; pulse full and

strong, 120, but no headache; had slept well, but moaned during sleep; intelligence perfect, but depressed in spirits. Dry cough, mentioned now for the first time. Urine high coloured and scanty. *Baptisia* (1^x), ten drops to a quarter of a pint, a dessertspoonful to be taken every two hours. Omit other medicines. To have a tepid sponge bath night and morning.

25th.—Skin and pulse as yesterday; flushed face, and heated scalp, still no headache. Tongue covered with thick white fur, but papillæ prominent, and margins and tips red. Bowels moved once, motion dark and solid; still dry cough. Continue *Baptisia*.

26th.—Pulse 100 when lying down, softer and more compressible; complained of feeling very weak when taken up night and morning for the sponge bath; slight nocturnal delirium. Continue *Baptisia*.

Evening.—No change since morning. Temperature under the tongue 104°.

27th.—Countenance expressive of exhaustion; pulse 110, feeble; has had a relaxed motion this evening (the first), peasoup like; gurgling and tenderness in right iliac fossa.

Cough has continued since the 24th inst., and there is now dyspnœa; as well as sonorous and sibilous râles, all over the anterior surface of chest. No dulness on percussion; no expectoration. Ascertained that he had been sleeping for several nights in the neighbourhood of an open window, and the question at once suggested itself to me, how far the chest symptoms were simply a complication of the fever, or were superinduced upon it from this cause. Continue *Baptisia*, and take *Bryonia* (1) in alternation.

Evening.—Another relaxed ochrey motion; skin hot and dry; pulse 105.

28th.—10.30 a.m. Reported to have slept tolerably, but had two relaxed motions during the night, and they sent word before I was up this morning, when my assistant sent *Arsenicum*. One of the motions was passed involuntarily and in bed. Tongue still thickly furred, with red margins and tip. About half a dozen lenticular rose-coloured spots

had come out on the abdomen during the night. This was their first appearance, as I had been looking for them at every visit for several days; they disappeared on pressure. Pulse 110; temperature under tongue 104; articulation indistinct.

Evening.—The spots seen this morning have faded away, and no others appeared in their place. Great increase of delirium, both in intensity and duration; loud shouting to schoolfellows. Bowels moved once since morning, the action preceded by griping pain, and followed by exhaustion, so that he nearly fainted.

Panting respiration 42 per minute; loud râles all over anterior surface of lungs.

Three ozs. of port wine in twenty-four hours. Beef tea ad libitum. Continue *Baptisia*, and alternate it with *Phosphorus* instead of *Bryonia*.

29th.—A very delirious night, repeatedly calling out loud, talking as if at school, &c. About a dozen lenticular rose-coloured spots on abdomen, *and four distinct mulberry spots on the body*, one on abdomen, and three on extremities; skin dry, but cooler; pulse 120. Fur lessening on tongue, and red surface increasing, especially near the tip. Bowels not moved all night, passed about half a pint of clear deep-coloured urine. Respiration much quieter, on right side respiratory murmur accompanied with slight roughness only, on left side sonorous and sibilant râles still; did not examine the posterior aspect of chest. Continue *Baptisia* and *Phosphorus*.

Evening.—Muttering delirium all day, articulation more indistinct than before; pulse 104, muscular tremor. Two motions passed involuntarily and unconsciously.

Has taken 2 ozs. of port wine, half a pint of beef tea, and a pint of milk to-day. Continue medicines.

30th.—Delirious talking all night, scarcely any quiet sleep, answers questions rationally when aroused; pulse 100 when asleep, rose to 120 on awaking; tremor of muscles of arm when raised, also of tongue when protruded. Fresh rose-coloured spots on abdomen, disappearing on pressure. Several relaxed motions passed unconsciously

during the night. Some tympanitic distension of abdomen ; panting breathing. Continue medicines.

Continue beef tea and wine.

Evening.—Has dozed most of the day, with little talking ; has passed three ochrey loose motions, two of them only involuntarily. Loud sibilant râles heard on both sides of chest anteriorly. *Baptisia* and *Arsenicum*.

July 1st.—10 a.m. Decided improvement ; pulse fallen to 100 when awake. Slept quietly most of the night. About fifty rose-coloured lenticular spots on the abdomen, the purple ones persist. Bowels not moved since 5 o'clock yesterday afternoon. Loud sonorous râles, evidently confined to the larger bronchial tubes ; tympanitis diminished, and no abdominal tenderness. Continue medicines.

This was my last visit. An allopathic doctor, the ordinary medical adviser of the family, had been visiting the father professionally, while I had been in attendance on the son, and to-day was asked (without any intention of superseding me) for his opinion on the case. He at once exclaimed that I had totally misunderstood the nature of the case, that it was idiopathic bronchitis alone under which the boy was suffering. He called in one of his colleagues, who unhesitatingly confirmed his dictum. I was therefore summarily dismissed, in spite of the improvement which had taken place in the symptoms since the previous morning. I afterwards learnt from a patient, a relative of the parties, and at whose recommendation they had consulted me, that blistering and other active treatment was at once resorted to, under which the lad became so much worse that, a few days after the last-mentioned date, he was left as dying by his new doctors. He however subsequently rallied. My informant also afterwards told me that during his convalescence all his hair came off.

He also added, as a token of the bad feeling which had been imported into the matter through the representations of the allopathic doctors, that a decided coolness had arisen between himself and relations as a consequence, and that he had no doubt, if the case had proved fatal, that a permanent breach and cessation of intercourse would have been the result.

I think my colleagues will agree with me that there was little difficulty in the diagnosis of this case, especially to one who had watched it from the commencement; my view of it would doubtless have not been called in question if I had not been known as a homœopathist, and been considered to have unfairly made this a door of entrance to another man's preserves. As, however, the question of diagnosis was raised with so much emphasis, it may not be uninteresting or un instructive to state a few of the reasons which led me to conclude the case to be one of typhoid complicated with bronchitis, and not, as asserted, of pure or idiopathic bronchitis.

1st. The order of the symptoms. There was lassitude and languor many days before the cough came on, and vertigo and acceleration of the circulation several days before; now languor and giddiness, although ordinary precursory symptoms of fever, are not so of bronchitis. The dyspnœa and bronchial râles supervened several days later than the cough.

2nd. The furred tongue, with raised papillæ and red edges and tip, the abdominal tenderness and meteorism, and the early occurrence of delirium, were all symptomatic of blood poisoning rather than of inflammatory irritation of the bronchial tubes. The high temperature of the buccal mucous membrane (104) would point to the same conclusion. The absence of expectoration (though not in itself conclusive) is favorable to the same view of the case, as idiopathic bronchitis very rarely remains so long in the congestive stage (in this case at least eight days).

3rd. The crucial test, however, was the presence of the lenticular rose rash and of the ochrey diarrhœa.

4th. The presence of the mulberry spots led me to think it likely, that the boy had been exposed to the infection of typhus as well as to the causes of typhoid fever. Dr. Murchison mentions several cases in which the two eruptions and, as he judged, the two diseases, coexisted.

The depilation which was reported to me subsequently to have taken place was confirmatory of the diagnosis of idiopathic fever. Thus, it would appear that at the time

the objection to my view of the case was made, the difficulty in the diagnosis which had existed in the early stage of the disease was at an end.

A word as to the treatment. A great improvement had taken place in the symptoms on the last day of my attendance, and there appears reason to believe that this was due to the *Baptisia* which I had given steadily for eight days, although alternated during a portion of this period with other medicines, to meet the bronchial and other complications.

I attended a case about the same time as the above, which I did not doubt was really typhoid fever, although the diagnosis was by no means so simple as in the case which I have narrated. My patient was a young lady nine years of age, and in her case, which lasted nearly a month, there was cough and sonorous and sibilant râles during the whole period of my attendance. Moreover there was no eruption. On the other hand, however, the affection of the bronchial mucous membrane remained all the time in the stage of dry congestion; there was also, during the first week or two, more excitement of the circulation than is usual in bronchitis, then followed frequent starting and talking during sleep with incoherency on awaking, somnolency in the daytime, from which it was often difficult to arouse her. There was also diarrhœa, and sordes on the lips and teeth. Deafness also began during the invasion of the disease, and continued for some time after convalescence.

In the case of the boy I think my colleagues will agree with me that there was some difficulty in the diagnosis on his first calling on me, and that there was some excuse, at least, for my viewing the case then as one of dyspepsia with debility, aggravated by fatigue and the heat of the weather. But is not the diagnosis confessedly difficult in the stage of invasion in many cases of typhoid fever, and has not this circumstance in part given rise to the nosological distinctions which have been so long made between typhoid, and gastric, and bilious fevers, &c., and which are no longer admitted as generic differences by systematic

writers? I have published a case of typhoid in the twenty-third volume of the *British Journal of Homœopathy*, in which almost the only symptoms during the first week of my attendance were nausea and vomiting, and, judging by my own experience, I should conclude that cases in which some gastric or other local trouble has for some time preceded the full development of the disease, are not infrequent. The question arises, Are there any tests by means of which these insidious cases of typhoid in the early stage can be diagnosed from ephemeral fever, or from feverishness symptomatic of acute dyspepsia, slight cerebral congestion, &c.? This question has an important bearing on practice.

Dr. E. M. Hale and other American physicians, as well as Dr. R. Hughes and others of our colleagues, are of opinion that *Baptisia* has the power to cause typhoid in the early stage to abort—a judgment which my own experience of the remedy has led me to indorse. But it would convert probability in the matter into, at least, moral certainty, if we had some well-marked signs by which we could diagnose typhoid in the stage of invasion from disease of a milder type, and remove any misgiving as to the power of our remedies to cut short the grave affection. Physiology teaches us that the rudimentary condition of animals of one class, corresponds to the perfectly developed state of others of an inferior grade, so that it might be hard to distinguish an abortion of the one order from a perfect being of another. So in the case of fever we should sometimes be glad to feel certain that our remedies have killed a virulent disease, as it were, in utero, and that we have not merely been watching the progress of a more mild and ephemeral disease to its normal stage of decline, *i. e.*, whether nature or art is to have the credit of the recovery.

We not unfrequently treat cases where the symptoms of invasion and reaction together only last a few days, and in which the nervous stage is never arrived at. It is not necessary to adduce examples of these, but I will briefly allude to one of medium duration and severity, in which there were many of the symptoms of typhoid, but in which the disease—if such it were—never reached its full develop-

ment; although I felt it was a question how far the rapid convalescence and absence of grave symptoms were due to the treatment.

Mr. ———, æt. 50, a clerk in a responsible position in the Civil Service, called upon me in the city on two successive Mondays, in November, 1865, complaining of headache and restless nights, which he attributed to overwork and anxiety. At the time of his second visit he was also complaining of loss of appetite and debility, and his pulse was over 90. He went no more to business after this day, and on the Thursday following I found him in bed with decided symptoms of febrile reaction; the pulse, however, was only 100, which it never subsequently exceeded. The headache persisted, though temporarily relieved by cold applications, and there were fits of incoherency on first awaking from his short and fitful slumbers. There was a very furred tongue, with red tip and margins, and there was some abdominal distension and tenderness. There was, however, no diarrhoea and no eruption, and on the following Thursday he was convalescent.

Although we have frequently to grapple with the difficulty which I have referred to in diagnosing, and therefore of precisely estimating the value of our treatment in regard to individual cases, and I think that it is right that we should admit it; yet when we compare our cases *en masse* with those treated by allopaths, or even expectants, we cannot hesitate in assigning its proper value to our improved system of therapeutics.

I would press on my junior brethren, especially any who may be still in statu pupillari, by way of corollary on the remarks I have just made, the importance of a minute and accurate study of the natural history of disease, even if the only thought were satisfactory and successful treatment. And this is one of the many aspects of our noble calling, in regard to which, any of us whose "days are in the yellow leaf," (if there were no brighter hope in store for us in the future, "when we have shuffled off this mortal coil,") might desire to have our time over again. And to those of us who embraced homœopathy, only after many years spent in

the practice of the old and wellnigh impotent system of therapeutics which we learnt in the schools, the retrospect of so much of our lives comparatively wasted as to good done to our fellow men, cannot but be painful.

Before passing to another case I will quote an extract from Dr. Murchison's work on fever, in which he refers to the difficulty of diagnosing typhoid fever in the early stage. He observes: "During the first week of the disease, it is often difficult to form a positive diagnosis; but even then pythogenic fever is to be suspected, if diarrhoea co-exist with frontal headache, disturbed sleep, and with general febrile symptoms, increasing towards night."* And again: "If both the eruption and the abdominal symptoms be absent, the diagnosis of pythogenic fever can only be arrived at by a process of exclusion, or by carefully weighing the symptoms and circumstances of the individual case, and comparing them with those of the other diseases with which it is most apt to be confounded. Under the circumstances mentioned, a diagnosis from many other diseases may sometimes be impossible."†

As to the exact period of the invasion of the disease. In another place Dr. Murchison observes: "The invasion is often so gradual that neither the patient nor his friends can state the precise day on which the illness commenced. This was the case with more than one half of sixty-three patients under my care. Jenner could only ascertain the day of commencement in seven of fifteen fatal cases. . . . The contrast which pythogenic fever presents in this respect to typhus and relapsing fever is remarkable."‡

The following case was during most of its continuance difficult, both as to its diagnosis and prognosis. Its fatal termination was a surprise and disappointment to me, and I have reason to believe not less so to the gentleman who saw the patient with me. I have now no doubt that it was an example of what Murchison terms latent typhoid or pythogenic fever, although the closing symptoms are somewhat different from what his view of the pathology of the disease

* Murchison *On Continuous Fevers*, p. 523.

† *Op. cit.*, p. 524.

‡ *Op. cit.*, p. 496.

would lead one to expect. I regret that my notes are so meagre. This was in part due to my being busy at the time, also to my not at first apprehending the serious nature of the case, but chiefly to there being so few salient symptoms—those both subjective and objective being for the most part negative.

Mrs. —, Blackheath, has generally enjoyed good health, except that she has been for about two years undergoing the local treatment for ulceration of the cervix uteri, under more than one specialist, as she was extremely anxious to have a family, and, I was informed, suffered much mental depression from disappointment of her hopes.

First visited her on May 3rd. Has been troubled with an "influenza cold" for about three weeks; has had alternations of heat and cold for some hours; pulse 120; furred tongue. Right sublingual glands swollen and tender, also the gums on the right side. *Belladonna* and *Mercurius*.

4th.—Sleep fitful; pulse fallen to 100. Has a large ulcer inside cheek for some weeks, and says she has never felt well since. Continue medicines.

5th.—Has slept better, and has less pain in gums; pulse 90, rather weak; tongue cleaning.

6th.—Feels decidedly better, and I sanctioned her being removed in a carriage to the house of a relative in the neighbourhood.

Evening.—Says that she feels better since her drive.

7th.—Continued improvement.

8th.—Had a very restless night; tongue more coated, and breath fetid; gland hard and tender; depressed in spirits. *Hepar sulph*.

Evening.—Has not slept; pulse 110; skin hot and dry. To have a hot sponge bath.

9th.—Slept much better than on the previous night, and pulse fallen since last night from 110 to 80; less heat of skin; tongue and mouth coated; loathing of food; slight vomiting last night. Is depressed and nervous still; her chief complaint is of the swelling and tenderness of the

submaxillary gland, and of the gums. Takes milk well, but refuses beef tea and broth. Enjoyed and was much soothed by the sponge bath last night.

To have a general hot bath to-night. Continue *Hepar sulph.*

Evening.—Has slept most of the day; pulse as in the morning.

10th.—Visited her in consultation with Dr. —. (I have no doubt that he would not object to my mentioning his name, but as I have not asked him, and the case terminated fatally, I do not do so.) Dr. — did not give me any light on the diagnosis of the case, but thought it would require careful watching. He attributed the inflammation of the gland and gums to the cutting of a wisdom tooth on the affected side.

He prescribed a *Chlorate of Potash* wash for the mouth, and *Nitric acid* to be taken internally.

Evening.—Pulse fuller than it has been, 100. The dulness and depression which has been observable during her illness much increased, so that when asked to protrude her tongue, she kept it out till told to put it in; expression of countenance tranquil, but heavy and languid; dozed most of the day; no sound sleep; retching, no vomiting; has taken a great deal of milk, but little else. Fætor of breath, and soreness of mouth much less.

11th.—Lower than yesterday.

12th.—Decidedly worse; says that in looking out of the window every object appears black.

13th.—*Morning*, 8 o'clock.—Visited with Dr. —. Much lower, but no symptoms of immediate sinking. Pulse small and soft, about 100; temperature in axilla 102; no pain, perfectly sensible when roused, but otherwise dull and listless, and disposed to doze, but roused without difficulty.

Noon.—Sinking, and died at 2 o'clock.

There was an objection made to a post-mortem examination, and I did not press for it, as I have nearly always found autopsies of the brain very unsatisfactory, which is not surprising, considering our almost entire ignorance of the relation of structure to function in the nervous system.

I will conclude with an extract from Dr. Murchison on latent typhoid fever, as bearing on this case.

“The insidious or latent form is a most important variety of pythogenic fever. It was well described by Dr. Hewett, of London, in 1826, and it has been prominently noticed by Louis, Chomel, and many other writers. In this form all the symptoms are at first of a very mild nature. There may be no great acceleration of the pulse, and the characters may be so indefinite that the nature of the case is not diagnosed. In one class of cases the chief symptoms are irregular chills, alternate with heat and flushing, slight headache, loss of appetite, lassitude, and disturbed sleep; diarrhoea may be absent, or the bowels may be constipated. In another class of cases the patient suffers chiefly from catarrh, and he is thought to have merely taken a cold. In a third class the chief symptoms are nausea, vomiting, and a red tongue, and the patient is thought to be suffering from gastric derangement, or from slight ‘gastric fever.’ The above symptoms continue for two weeks or longer, but are so mild that no anxiety is felt about the patient, when suddenly he becomes alarmingly ill; profuse hæmorrhage from the bowels takes place, which may induce fatal syncope, or more commonly symptoms of peritonitis from perforation set in, and, after a few hours, terminate in death. Cases answering to this description are not uncommon. Before the alarming symptoms occur, the patient’s prostration may be so slight that he is able to follow his ordinary avocations until within a few hours of the fatal event. I have known a man walk more than a mile to the London Fever Hospital at the end of the third week of the fever, and die from perforation in less than thirty hours after his admission. Louis mentions the case of a man who walked daily in the hospital garden up to the twenty-third day, when perforation occurred, which was followed by death in thirty-six hours. In most such cases, indeed, the perforation is probably due to rupture of the denuded peritoneum, forming the base of the intestinal ulcers, which would not have happened if the primary

symptoms had been sufficiently severe to compel the patient to maintain the recumbent posture.”*

I had intended to have given an account of an outbreak of Rötheln which took place recently in a school which I attend, but I think my paper is already long enough, and contains sufficient matter for discussion and remark.

Discussion on Mr. Harmer Smith's paper.

Dr. HALE.—A very important question had been raised by Mr. Harmer Smith's paper, namely, whether *Baptisia* or any other remedy possesses the power of arresting the progress of typhoid fever. From the nature of specific fever, and as the result of the experience of those who have studied the disease, such a power appeared to be very doubtful. Dr. Hale had seen a great deal of fever in Ireland, and had seen various allopathic medicines given in the hope of cutting short the disease, but in no case had he ever seen any treatment succeed; indeed, the nature of essential fever would lead to such a conclusion. Its origin in an animal poison, its running a definite course, ending in a more or less well-marked crisis, the phenomena of the disease appearing to be nature's efforts to eliminate the poison, all seem to characterise fever as a profound disturbance of the entire system which no single drug could be supposed to have the power to arrest. While expressing this opinion it would be far from Dr. Hale's intention to question the truth of Dr. Richard Hughes' statements concerning *Baptisia*, but Dr. Hale contended that there was so much difficulty in determining whether the symptoms which *Baptisia* cuts short are really those of typhoid fever, or only that group of symptoms which resemble the invasion of fever and are only the symptoms constantly met with as the effects of bilious derangement, or overfatigue, or broken rest. Could it be shown that, in addition to the symptoms which often appear to indicate the invasion of typhoid, there were the characteristic rose spots, and dejections from the intestinal mucous membrane, there would be more ground for hope that in *Baptisia* we had really found a remedy capable of effecting what was claimed for it. With respect to the last case related in Mr. Harmer Smith's paper Dr. Hale was inclined to attribute the symptoms to the effects of long-continued cauterization of the os uteri; he had seen scores of cases where not only all kinds of local affections, such as ulceration of the tonsils and buccal cavity, distressing headaches and neuralgic sufferings, but a general and profound derangement of health had resulted from local treatment of ulceration, or supposed ulceration of the os and cervix uteri.

* Op. cit., p. 520.

In this particular case it was, he thought, not at all improbable that there had been a translation of the diseased action from the organ primarily affected to distant organs ; some of the symptoms indicated a more general state of disease somewhat resembling pyæmia. In the course of his remarks Dr. Hale expressed a hope that some of the members present would be able to give the Society the results of their experience of *Baptisia* in typhoid fever, believing that the discovery of any remedy having a specific character ought to be welcomed thankfully by medical practitioners of every school.

Dr. DUDGEON only rose to mention that one of the best monographs on the subject of the homœopathic treatment of abdominal typhus, as the Germans called what we denominate typhoid, was found among the papers of the late Dr. Trinks, of Dresden, and had been published in *Hirschel's Journal*. Although it was not quite perfect, it was so valuable that he had felt it eminently worthy of being translated for the *British Journal of Homœopathy*, in which accordingly it would appear. Dr. Trinks emphatically declared that he had seen cases of enteric typhus repeatedly cut short by the administration of the appropriate remedy, and that though he was sceptical by nature he could not now entertain any doubt on the subject.

Dr. A. WILLIAMS would mention a case of typhoid which occurred to himself where diagnosis would be difficult. There was constipation throughout, also headache was absent, then there was a relapse with diarrhœa, hæmorrhage, pain in the bowels, and death. The case occurred in a lady who enjoyed good health until the time of the attack, which came on suddenly with violent bilious vomiting and purging which lasted for about a couple of days, but had ceased before he saw her. When he saw her and throughout the attack she was in a state of great prostration. There was pungent heat of skin, pulse ranging from 120 to 130 ; temperature from 103 to 104 ; tongue, which had been covered with a thick brown fur, became clean and glazed, and afterwards very dry and cracked. There was abdominal tenderness, and she wandered a good deal, but there was no headache during the whole course of the disease, and after the first bilious derangement the bowels were habitually constipated, so that for several days together she would have no motion at all, and then only a scanty costive one. The fever ran the usual period, about twenty-two days, and when convalescence seemed to be nicely setting in the relapse so characteristic of typhoid took place, and now we got the ochrey motions slightly mixed with blood and the patient quickly sank and died. He was unable to get a post-mortem examination, but the onset of the disease, its whole course and duration, together with the confirmatory evidence of the relapse with diarrhœa and hæmorrhage, were so conclusive as to put the case beyond doubt. But the entire absence of diarrhœa and of headache in the primary attack after the

incipient stage had passed was very striking and shows how impossible it is to build on particular symptoms, any one of which may sometimes be absent. The case must be judged by the whole group of symptoms present and often can be properly diagnosed only by the process of exclusion.

Dr. WYLD had no doubt whatever that Mr. Smith's first case was one of typhoid fever, and the conduct of the two allopathic medical men in the case was only another illustration of the shameless manner they frequently assumed towards homœopaths. The paper spoke of certain cases of difficult diagnosis, and it was a matter of great rejoicing among the old school practitioners whenever they caught a homœopath making a mistake. Considering how we are debarred from the experience furnished by our great hospitals, where every form of disease presents itself, and where an attentive physician has so ample a field of instruction, ignorance on their part is much less excusable than with us. Notwithstanding, however, the superior advantages of those who have access to great hospitals, and notwithstanding the right we have to expect almost infallibility from the specialists of these great hospitals, yet constant errors, sometimes of an amusing character and sometimes of a very melancholy nature, are occurring. Dr. Wyld remembered the case of a poor woman who came into a great hospital. She was supposed to have a large polypus protruding from the vagina. A ligature was placed round this supposed polypus and which was then amputated. But unhappily it was not polypus but prolapsus of the uterus, and the poor woman died of the operation in a few hours. Had so appalling an accident occurred at our homœopathic hospital it might almost have led to its destruction. Dr. Wyld remembered another case. A strong woman came into the wards with white patches on her skin. She was retained some weeks and daily inspected; patches of skin from time to time became detached and the sore healed, only to be followed by new patches of white skin. This case excited great curiosity, and all the doctors and surgeons inspected it. No skin doctor could give it a name, at last a rising surgeon appeared who boldly pronounced it to be a *recorded* disease termed *white gangrene*. Well, the woman, after a time was dismissed, and for two years she went from one hospital to another puzzling the whole fraternity. At last she was detected with a small bottle of *Hydrochloric acid*, and it seems she had been in the habit of burning the skin from time to time with this acid, and thus producing the "*white gangrene*." She ultimately committed suicide. Dr. Wyld saw a third case in a strong man who had for years, by the advice of the doctors, worn a truss for inguinal hernia. One day the tumour became very painful and much swelled, and the man came into the hospital by advice of the doctor to be operated on for strangulated hernia. The case was, however, only one of retained testicle, and fortunately no operation was performed. Dr. Wyld begged to

record a fourth case. A man came into the ward and said he felt very ill and giddy. He, however, looked so well that the physician pronounced it a case of malingering and gave him a purge. The man, however, gradually became somnolent and died, and a post-mortem examination revealed an abscess in the substance of the brain as large as a walnut. Again, it was well known that women had often been operated on for supposed ovarian tumour, but on opening into the abdomen no tumours existed. Dr. Wyld had seen, in Sir James Simpson's practice, a case of *phantom* tumour which Sir James dispersed at pleasure from time to time by administering chloroform. The percussion over this tumour was as dense as over a fibrous tumour. Cases like the above were sufficient to show how the best informed practitioners might be misled. A noise has lately been made in a medical journal about a homœopath mistaking pregnancy for ovarian tumour. Mistakes of this kind were repeatedly made by medical men, especially if they were not accoucheurs. One such case took Dr. Wyld by surprise two years ago. The tumour was pronounced by him to be the result of pregnancy, but the woman denied that this was possible, as she had abstained from her husband for two years. In corroboration of her statement, the mammæ were entirely collapsed, and the emaciated aspect of the case made it look like one of abdominal disease, yet in due time a baby solved the mystery. Of course a careful examination of the abdomen with the stethoscope would have solved the mystery at an earlier period. *Typhoid* pneumonia, so called, is sometimes puzzling. Some years ago Dr. Wyld attended a little girl seven years of age. The family had previously lost several children under allopathic treatment from congestion of the lungs. In the present case there was very slight congestion and a pulse only 100, but there was a feverish and hectic condition of a mild character. The case was seen by another of our colleagues and no sufficient reason existed for anxiety except that the symptoms did not yield to treatment, together with the fact of the tendency which existed in the family to fatal pneumonia. This child took nourishment, no gastric symptoms existed nor diarrhœa, and there were no typhoid spots. Still the child gradually sank. A *post-mortem* examination revealed nothing beyond very slight local congestion in one lung. Another somewhat similar case occurred to Dr. Wyld lately. The lady was sixty years of age and had hepatization of the upper third of the left lung only, elsewhere the lungs were sound. Low fever accompanied the case from the beginning, but nourishment and stimulants were freely but carefully given. *Baptisia* and all the usual remedies were also given, but the patient made no rally although the rusty expectoration ceased and the affected lung cleared up. A colleague saw the case with Dr. Wyld, and although the case was regarded as very dangerous, still having regard to the healthy condition of the organs and the absence of any diarrhœa, and the

clearness of the brain, much hope was entertained. The lady, however, gradually sank and died in two weeks from the beginning of the attacks. Cases of the above kind have led Dr. Wyld to suspect that there may exist forms of fever of a *poisonous* and exhausting nature generally termed typhoid, but not possessing the symptoms or signs of true typhoid fever. Perhaps it is fevers of this kind which are said to be cut short by *Baptisia*, which medicine Dr. Wyld has occasionally found apparently very useful in low fevers. Whether a true typhoid fever, the only infallible sign of which is the presence of lenticular spots, is ever cut short by any medicine has not yet, he believes, been demonstrated by statistics. Disease of the kidneys is probably more frequently overlooked than disease of any other vital organ. Albumen or sugar may be passed by the kidneys for years probably undetected. In such cases a carbuncle may one day develop itself apparently not of a dangerous magnitude, but as it may resist all treatment and the patient begin to show signs of some deep-seated debility, it then, perhaps, occurs for the first time to the attendant to examine the urine, the result of which examination may entirely alter his treatment and his hopeful views of the case.

Mr. SANDERS STEPHENS said he had used *Baptisia* recently in a case of gastric fever with decidedly beneficial effects. It was after he had applied the ordinary remedies, and when the disease still persisted, that he determined on trying *Baptisia*. He was then anxious lest the disease should take on the ordinary typhoid type; but from the time when he began to use it, the patient improved, and was soon able to get about. Subsequently he suffered a relapse from too great and too early exertion. In reference to Mr. Harmer Smith's first case, he thought that when he first consulted him the symptoms were of a sufficiently grave nature to indicate further mischief than simple debility and exhaustion consequent on the walk. With the pulse above 100, he would have ordered him to bed at once, and with the earliest symptoms of continued fever, he would have abstained from ordering baths—which from the exertion tend to weaken—and been content to recommend warm water sponging under the bed clothes.

Dr. YELDHAM said he had tried *Baptisia* some years since in the hospital in cases of typhoid fever, and could not see that it had any influence over the disease. Very recently he saw, in consultation, a case of typhus in a child in which *Baptisia* had been given—almost uninterruptedly from the commencement—a fortnight—without effect. Cases of common continued fever could certainly be arrested by proper treatment, but he doubted the power of any medicine to cut short fever if of a specific nature. Mr. Smith's first case was clearly one of typhoid fever, accompanied, as such cases almost always were, especially in children, with bronchial cough, more commonly moist than dry.

It was generally perfectly harmless, and it was well to explain this to the patient and friends, so as to guard against misunderstanding and misrepresentation on the subject, as importance was often attached to it by persons unacquainted with disease. It was so in the case of typhus he had just alluded to. Pulmonic symptoms were apprehended, but the respiratory murmur was perfect throughout the chest, and he was able to say positively that the cough was from the larger bronchial tubes only, and would assuredly subside if the child recovered from the fever. He related a curious case of mistaken diagnosis. An allopathic medical friend of his many years ago attended a gentleman supposed to be affected with phthisis. One of the most celebrated physicians of the time attended the case with him in consultation. The patient died, and at a post-mortem, the physician, pointing to a particular spot on the chest, said, "You will there find a cavity the size of an orange." On opening the chest the lungs were found perfectly sound! Such cases should teach them the utmost caution and circumspection in pronouncing upon the nature of a case. Errors in diagnosis were much more damaging to a medical man's reputation than errors in therapeutics.

Dr. LEADAM had not had sufficient experience with *Baptisia* to form an opinion of its power of cutting short typhoid fever. He had used it in several cases, and found it influence the fever materially, but was not sure that it cut the disease short. He considered Mr. Harmer Smith's first case as one of typhoid fever with bronchial complication.

Dr. MADDEN agreed with the previous speakers in the opinion that in Mr. Harmer Smith's first case there was no room for difference of diagnosis, but in the last case he doubted very much whether it was a case of typhoid fever. He was rather disposed to consider it as a case of cerebral disease, manifesting typhoid symptoms towards its close. He had himself met with cases which, if seen for the first time near the fatal termination, might be easily mistaken for typhoid fever, and yet whose history proved that they were not so. In reference to the power of *Baptisia* to cut short the ordinary gastric fever of this country, which, if allowed to develop itself, became typhoid or enteric fever, he had no doubt whatever of its efficacy if used in well selected cases, and at the right time. He had used it too frequently during the last six years to have any doubt upon the subject. He was well aware of the difficulty of distinguishing the gastric stage of enteric fever from an ordinary severe bilious attack, and hence in his first few cases he suspected that he might have been misled. But a larger experience, both here and in Australia, where a modified form of typhoid, known as *Colonial Fever*, was very common, had convinced him that the effect of *Baptisia* is to break up the usual course of the complaint, and to bring in convalescence in three or four days in place of the ordinary twenty-one days. Where *Baptisia* acts well, no diarrhoea

comes on, but he has several times met with critical sweats or hæmorrhage. He always employs the 1st dilution. After the fully developed typhoid symptoms have set in, with the dark brown tongue, ochrey diarrhœa, and rose spots, he has not found *Baptisia* useful, *Arsenicum* being then the usually indicated remedy. He had been requested by Mr. Pope to mention that Dr. Blake, of Birmingham, was at present engaged in a careful investigation of the action of *Baptisia* in fever, taking thermometric observations, and in every way testing its action with precision.

Dr. DRURY, Vice-President (in the chair), thought it must be gratifying to Mr. Harmer Smith that all his colleagues (and in this he heartily joined) agreed with him in his diagnosis of his first case. The paper had led to a very interesting discussion, which was also satisfactory. A good deal had been said about errors that were liable to be made in diagnosis, and from the cases narrated it did not appear that the homœopaths enjoyed a monopoly of these cases, though from the way some allopaths spoke, one might think that deaths were unknown in allopathic practice, while to become a homœopath was to forget all previous knowledge and to commence a series of perpetual blunders and fatal treatment. Now it had leaked out that some very considerable blunders had been made by men in the first rank in allopathy. And it had also transpired that out of some 1200 cases sent to the Smallpox Hospital, about thirty were not smallpox at all, and mistakes of this kind could hardly all be traced to the unfortunate homœopaths, any more than the 1600 deaths in London every week; for as far back as the time of the late Dr. George Gregory, physician to the Smallpox Hospital, he had informed him that such errors on the part of medical men were by no means uncommon. The differences of doctors has passed into a proverb: he recollected one that was worth mentioning. A medical friend of his had a fall which hurt his leg; as his health gave way under it he consulted Sir Benjamin Brodie, who thought it was inflammation of the internal saphena vein, and recommended even bandaging, which gave temporary relief. A tumour having formed, another gentleman thought it was a fatty tumour, but Mr. Travers, who afterwards saw it, cut down on it, and let out some dark coloured unhealthy fluid. His health continuing to give way, he sold his practice; but before leaving town, perceiving something projecting in the wound, he pulled out with a forceps what proved to be a thorn that must have been lodged there a long time, and had found its way in some years before when he fell on a hedge, hunting. No blame could be attached to those who failed to diagnose the presence of the thorn, but the difference of opinion was not pleasant at the time to the sufferer. Perhaps more was made of these mistakes than they deserved, and it might be well for all medical men to be a little more lenient on those who had the misfortune to fall into an error of judgment

often pardonable, but often severely dealt with. Much had been said about the possibility of cutting fever short, and this had engaged the attention of the leading allopathic authorities years ago without any satisfactory opinion being arrived at. Purgatives and emetics, whatever they may have done to aggravate the disease, had certainly failed to cut it short. And whatever Dr. Graves may have done in lessening the number of his fatal cases by *Tartar emetic* and *Opium*, aided by food, or Dr. Alison with small doses of *Sulphate of Magnesia*, and *Tartar emetic* in rather frequent doses (a vile compound), aided by stimulants when called for, neither succeeded in a sudden cutting short of the disease. It no doubt happened from time to time, more frequently in homœopathic practice, that a disease that was supposed to be setting in sharply ceased suddenly; but the question must always be open as to whether this was an example of a disease cut short. That disease was constantly modified by treatment, there was unquestionable evidence: thus a disease that might be expected to run thirty or forty days, might become milder and milder under some judicious treatment, and be convalescent long before the time anticipated. This no doubt was a cutting short of the disease, but not in the sense of a distinct stopping of the disease. He had recently vaccinated one of the nurses in the hospital; the arm appeared to be taking, and a vesicle forming, but toothache having come on the nurse took *Mercurius*, when all signs of vaccination began at once to fade away. He would take the opportunity of calling attention to a most useful little book on fever by Rapou, which, though perhaps not brought up to the present day, was still excellent of its kind.

CASES TREATED IN THE LONDON HOMŒO-
PATHIC HOSPITAL.

By Dr. HENRY R. MADDEN.

Acute Rheumatism.

CASE 1.—James Collins, æt. 28, admitted February 17th, 1870. This man, whose occupation is that of a builder, had an attack of rheumatic fever two years ago, brought on by sleeping in a damp bed; at which time he states that he was confined to his bed for three months, and that his medical attendant told him he had “dropsy of the heart” (hydropericardium?). Three weeks ago, after working out of doors in a cold wind, he felt pains in his ankles, so severe as to oblige him to go to bed; where he had remained, with one day’s exception, ever since. The pains subsequently attacked the hands and then the knees. He has been applying cold water bandages.

Present state.—Complains much of his joints, which are swollen and very tender; pulse 88, full and soft; tongue moist, without much coating; skin perspiring; no cardiac complication; temp. 102·9°. *Acon.* 1^x, gtt. i, 2da. q. q. h.

Feb. 18th.—Pulse 72, full; temp. 100·8°; tongue clean and moist; urine very high coloured, depositing mucus and lithates copiously, re-action acid; sweat profuse. Continue *Acon.* 1^x.

19th.—Pulse 78; temp. 100·8°; no change. Continue *Acon.* 1^x.

20th.—Much the same; sweating less freely. Continue *Acon.* 1^x.

21st.—Pulse 80; temp. 101·2°; sweats less freely and it is foetid; urine copious and loaded. *Bryonia* 1^x gtt. i, 2da. q. q. h. In the evening complained very much of the acute pains in his joints, which were excessively tender to the touch.

22nd.—Pains became rather easier during the night; pulse 80; temp. 100.7° . Continue *Bry.* 1 \times .

23rd.—Decidedly less pain; temp. 100.6° . Continue *Bry.* 1 \times .

24th.—Still less pain, especially in left hand, which is nearly well; sleeps much better; temp. 100° ; pulse 68. Continue *Bry.* 1 \times .

25th.—Greatly better; pulse 52; pains slight. Continue *Bry.* 1 \times .

26th.—Improving in every way. *Bry.* 1 \times , 3tis q. q. h. Meat $\frac{3}{4}$ ij daily.

March 1st.—Pains have entirely ceased; urine acid, and without any deposit of lithates; pulse 54; temp. 99° . *Bry.* 1 \times , 4ta q. q. horâ.

2nd.—No pain; temp. 98.4° . Continue *Bry.* 1 \times .

7th.—Dismissed cured.

CASE 2.—Ellen Dubber, a household servant, æt. 29, admitted February 28th, 1870. States that she had good health till five years ago, when she had an attack of rheumatic fever, which laid her up for three months. Since that time has frequently suffered much from pains in her feet and ankles. Has also lately had palpitation and shortness of breath. A week ago she caught a severe cold, which has brought on cough with white frothy expectoration, and much soreness of chest. Two days ago, was attacked with severe pains in ankles, which speedily extended upwards to the knees and hips, and have continually increased until now. Had epistaxis yesterday evening. *Menses* regular, but of late they have been scanty; pulse 112; temp. 103° . *Acon.* 1 \times , 2da q. q. horâ.

March 2nd.—Not any better; pulse 112; temp. 104° ; tongue yellow and moist; has perspired freely, but slept very little. There is slight dulness on percussion at the base of the right lung posteriorly, with bronchophony and friction sound. Continue *Acon.* 1 \times .

3rd.—Feels better; legs much less painful, but the shoulders are affected; pulse 92; temp. 102.8° . Continue *Acon.* 1 \times .

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4th.—Knees better ; shoulders very painful ; pulse 86 ; temp. 103.9° ; tongue foul. Continue *Acon.* 1 \times .

5th.—Pains much better ; pulse 80 ; temp. 100.3° ; tongue yellow in centre, but cleaning round the edges. Has a good deal of pain across the chest, which is greatly aggravated by moving ; bowels constipated. *Bryonia* 12, 3tia q. q. horâ.

6th.—Improving ; pains less ; pulse 72. Continue *Bry.* 12.

7th.—Still better ; pulse 64 ; temp. 99.5° . Continue *Bry.* 12.

8th.—Steadily improving in every way ; pulse 60 ; temp. 98.8° .

19th.—The patient progressed steadily and satisfactorily under *Bryon.* 12, which was continued till to-day, when it was changed for *Sulphur* 12, gtt. i, ter die.

21st.—Dismissed cured.

CASE 3.—Lydia James, æt. 23, was admitted on March 21st. She states that she had rheumatic fever seven years ago, which confined her to the house for three months, since which she has had no return until three months ago, when she felt pain in the left side of her chest, in the cardiac region, worse on lifting. A fortnight ago was attacked with pains in her ankles and then in her wrists, and has now been confined to bed with them for ten days. The knees are the joints chiefly affected ; pulse 112 ; tongue clean ; there are no cardiac symptoms. *Acon.* 1 \times , gtt. i, 4ta q. q. horâ.

March 22nd.—Is perspiring gently ; pains in knees unchanged, and she has some pain in the right wrist ; temp. 100.3° . Continue *Acon.* 1 \times .

24th.—Better ; pulse 99 ; temp. 99.4° . *Bryonia* 12, gtt. i, 4ta q. q. horâ.

26th.—Feels better in herself ; pulse 92 ; tongue red down the centre ; much less pain in right hand, but the left elbow is affected. Continue *Bry.* 12.

28th.—Pains have returned to the right hand and arm. Continue *Bry.* 12.

29th.—Complains much of headache and sleeplessness ;

the pains are decidedly better, with the exception of the right shoulder. *Bellad.* 12, gtt. i, 4ta q. q. horâ.

31st.—Felt very sick last evening, and still had some headache; pain in the shoulder nearly gone. Continue *Bell.* 12.

April 1st.—Feels much better; pain in shoulder still continues slightly. Continue *Bell.* 12.

5th.—Complains now chiefly of weakness. *China* 1, gtt. i, ter die.

11th.—Greatly better in every way, but complains of an uncomfortable dryness of the palms of her hands. *Sulphur* 12, gtt. i, ter die.

16th.—Dismissed cured.

CASE 4.—Henry Hallett, a groom, æt. 17, was admitted on April 11th. He states that he enjoyed very good health until two years ago, when he had an attack of rheumatic fever and inflammation of the lungs, which laid him up for six months. Has had considerable tenderness of the joints ever since. Two months ago he had a severe attack of tightness across the chest with dyspnœa, which passed off in three days. Four days ago was troubled with pains in his shoulders, which spread from thence to his chest and limbs, and have progressively increased in severity to the present time. His pulse is now 120; temp. 100.4° ; tongue brownish and moist; respirations 32. *Acon.* 1, gtt. 2da q. q. horâ; 2nd diet.

April 13th.—Much the same; tongue whitish; pulse 120; Temp. 102.1° ; skin perspiring gently. The pains are most severe in knees and ankles. Continue *Acon.* 1.

14th.—No marked change; pulse 92; temp, 101.3° ; tongue foul. Continue *Acon.* 1.

16th.—Feels rather better; pulse 84. *Bryon.* 1^x, gtt. i, 2da q. q. horâ.

18th.—Pains in legs much better; right arm also better, but the left hand is swollen and tender. Pulse 80; temp. 100.6° . *Bry.* 12, gtt. i, 4tis q. q. horâ.

20th.—Better; pains considerably less; some perspira-

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tion; tongue still foul; pulse 84; temp. 100°. Continue *Bry.* 12.

21st.—Perspiring freely, but the pains are not relieved by it. *Mercur. vivus.* 3^x, gr. 1, 4ta. q. q. horâ.

22nd.—A good deal of pain in the left hand and right shoulder; rather less perspiration; pulse 92. Continue *Merc. vivus.* 3^x.

23rd.—Left hand better; pulse 80; is still sweating freely. *Bry.* 12 gtt. i, 4ta. q. q. horâ.

24th.—Sleeps well; perspiration less; pulse 80. Continue *Bry.* 12.

25th.—Pains in limbs better, but has a good deal of pain across the chest in the region of the diaphragm, which pain extends through to the back; temp. 98·8°. *Ranunc. bulb.* 1, gtt. i, 3tia q. q. horâ.

28th.—Feels decidedly better; tongue clean; pain in right wrist nearly well; left wrist swollen and red. Continue *Ran. b.* 1.

May 2nd.—Decidedly improving. Continue *Ran. b.* 1.

5th.—Much better in every way. *Tr. Sulph.* 1, gtt. i, 4, ter die.

13th.—Dismissed cured.

CASE 5.—Daniel Macdonald, æt. 27, a builder, admitted June 4th, 1870. States that four days before admission he began to feel pain in the right hip-joint, which extended the next day to the knee and ankle; then the joints of the left leg were affected, and finally the left elbow was attacked.

On admission he complains of much pain and stiffness, together with swelling of hips, knees, and legs, and left elbow; there is decided heat of skin, and his sleep was bad last night; pulse 88; heart's action normal; bowels rather loose, the stools watery; is perspiring. *Acon.* 1^x, gtt. i, 2da, q. q. horâ.

(Unfortunately the temp. is not recorded on this or the next seven days.)

June 9th.—Progressing favorably in every way. Continue *Acon.* 1^x.

13th.—Temperature on 11th was 101° , on this evening 100.7° . The man feels much better, and the legs are quite free from pain; both elbows and wrists, however, are now affected. *Bry.* 12, gtt. i, 2da q. q. h.

14th.—Doing well; temp. 100° . Continue *Bry.* 12.

15th.—Temp. 99.8° ; still improving. Continue *Bry.* 12.

16th.—Improvement continues; skin still perspiring, and the sweat having the offensive smell of an unwashed person, he was ordered a warm bath. Continue *Bry.* 12.

20th.—Since last report the temperature has continued at 99.8° , and he has not progressed much. *Acon.* 3^x, gtt. i, 3tia q. q. h.

21st.—Decidedly better. Resume *Bry.* 12, gtt. i, 3tia q. q. h.

23rd.—Pains almost gone, and no longer any fever. Continue *Bry.* 12.

27th.—Convalescent. *Sulph.* 12, gtt. i, ter die.

July 5th.—Dismissed cured.

These five cases may not prove much respecting the success of homœopathic treatment in acute rheumatism, but they nevertheless show that under it the progress is satisfactory and rapid compared with the course which the disease had run in the same patients when previously attacked, and treated allopathically. It will be observed that four had had rheumatic fever before the occasion on which they were admitted into our hospital, and in each the duration had been considerable, in three of the cases having lasted three months, and in the fourth, when it was complicated with pectoral symptoms, having continued no less than six months. On reading over the reports of these cases, I regret very much that the patient's condition from day to day has not been more fully detailed; nevertheless, I have preferred giving the reports almost unaltered from the case papers, rather than filling up the outline from memory, which would be very apt to mislead by an unintentional confounding of one case with another. I may, however, state that the strong impression left upon my mind when watching the cases, was favorable to the con-

tinued use of *Aconite*. It seems to me that we are too apt to forget that *Aconite* serves a double purpose in rheumatic fever, viz., both by its action on the circulation, and by its direct homœopathicity to the effects of the rheumatic poison. When studying this medicine with Dr. R. Hughes, we were both much impressed with this fact; and in his *Pharmacodynamics*, Dr. Hughes refers to this in the following words:—“A survey of the more localised effects of *Aconite*—its action upon the heart, the joints and muscles, the sclerotica, and the serous membrane—suggests at once its close analogy to the *rheumatic* poison. In acute rheumatism it is our main remedy. The fever is one element in the homœopathic relation between the disease and the drug; but being of the toxæmic type, it must not be expected to disappear under the *Aconite* in a few hours. Nevertheless, when occurring in persons of fairly good constitution, and not presenting asthenic symptoms, acute rheumatism will yield to *Aconite* perhaps more rapidly than to any other drug or mode of treatment.” In these remarks Dr. R. Hughes refers to this twofold indication for *Aconite* in this disease, but he leaves it to his readers to draw their own practical conclusions. For my part, I believe that the use of *Aconite* in acute rheumatism should not be limited to the early stage, but, contrary to what is the proper course in other fevers, should be persevered in as long as the case progresses favorably. It does not appear in the scanty reports of these cases, but I know that practically *Aconite* was continued until the case seemed to be coming to a stand, or in other words, until it had completely done its work. It is well known to all homœopaths that in most specific fevers *Acon.* has but little effect, and it is also well known that in such cases a continuance of the drug is injurious rather than otherwise; and consequently it is with many of us a practical rule that, if in an acute febrile affection, *Acon.* fails to produce any marked change for the better in twenty-four hours, it behoves us to look out for some other remedy. In rheumatic fevers, however, this rule does not hold good; it will often happen that twenty-four or forty-eight hours may elapse after *Aconite* has been

commenced and continued in repeated doses, before any sign of improvement occurs; nay, the pulse may continue high, and the temperature, as in cases two and four, may even increase; yet, so far from changing the remedy, the *Aconite* may be continued with every confidence of success. This is evidently attributable to the twofold indication which exists for the administration of *Aconite* in acute rheumatism. It is called for by the fever; it is also called for by the rheumatic poison, and the reason for persevering in its use, notwithstanding the continuance of the fever, is accordingly not far to seek. In ordinary catarrhal fever the pyrexia is dependent upon disturbed innervation of the circulatory apparatus, and hence *Aconite* covers the whole complex of symptoms, and generally quells the disturbance with great promptitude; when, however, the pyrexia is caused by the presence of some specific poison, it frequently happens that *Aconite* is powerless; since, the cause of the disturbance being unremoved, its consequences remain unchecked. In rheumatic fever, however, and in a lesser degree in measles, *Aconite* is indicated homœopathically for the specific poison, and hence the continuance of fever after its administration merely points to the necessity for perseverance in its use (the specific poison not being as yet removed), and does not indicate the need of a change of remedy. I have dwelt on this point a little, because I have known so many of my colleagues who almost from the beginning of rheumatic fever have alternated *Aconite* with *Bry.*, or some other remedy, in the hope of cutting short the disease, whereas I strongly suspect that such an alternation is positively hurtful, and tends to prolong the attack by interfering with the full action of the *Aconite*. Our best plan is to keep steadily to *Aconite* until it has accomplished all it can, before giving any other medicine, and then we ought to drop *Aconite* altogether.

Organic stricture of lower end of the colon.

CASE 1.—Henrietta Roberts, a widow, æt. 51, was admitted on May 7th, 1870.

History.—The patient states that during her last men-

strual period (about March 26th) the flow was very profuse, and in consequence of exposure to cold she had an attack which her medical attendant called inflammation of the womb. After the menses had lasted six days, she was seized with violent cutting and twisting in the hypogastrium, accompanied by constant vomiting, great accumulation of flatus in the abdomen, and constipation. The pain continued to be severe for about a fortnight, during five days of which she was insensible and delirious. For the last three weeks she has been much in the same condition as she now is. Has been subject to hysteria.

Present state.—Twisting pain over the whole abdomen, increased by movement, excessive tenderness on pressure. Abdomen greatly distended, much constipation, having had only one evacuation during the last five weeks. Frequent retching, especially at night; appetite small; not much thirst; tongue slightly coated; pulse 100. Much restlessness during the night, and frequent globus hystericus; some cough with a little expectoration. She is very emaciated; the abdominal walls are quite tense, and percussion gives a tympanitic sound all over, from the epigastrium to the pubes and from side to side. *Nux Moschata* 30, gtt. i, tertia q. q. h.

May 9th.—No better in any way. *Nux Moschata* 3^x, gtt. i, tertia q. q. h.

12th.—Tympanitic increased; pain varies; less retching; tongue clean; bowels not acting. To have an enema, and if it does not relieve the bowels, to have it repeated, using a long tube, so as to throw it high up into the colon. *Colchicum* 3^x, gtt. i., tertia q. q. h.

16th.—No improvement in any way; bowels not acting by enemata. *China* 1, gtt. i, tertia q. q. h.

19th.—Rather easier, but no other improvement. *Plumb. carb.* 3^x, gr. i, 2da. q. q. h.

23rd.—At first under the *Plumbum*, she seemed to mend; the abdomen became less tense, and the peristaltic movements could be seen very clearly over the whole abdomen, but the improvement did not continue. At this time there was a consultation held, and several members of the hos-

pital staff saw her. The question of puncturing the abdomen was discussed, but all agreeing that the distension was in the small intestines, and not in the colon, it was decided not to make any such hazardous attempt. *Asafœtida* 1, gtt. i, 3tia q. q. h.

26th.—More tympanitis, and complains much of spasmodic pains. Still no stool, and no desire for stool; complains much of painfulness of anus before and after the enemata, which were continued daily, with the long tube, which seemed to pass up the bowels easily, but the fluid pumped up always returned unchanged. *Zinc. valer.* 1^x, gr. i, 4, ter die.

30th.—Abdomen rather less tense; no action of bowels; tongue dry in the centre. *Opium* 1 gtt. i, 2da. q. q. h.

31st.—This morning the patient became suddenly worse, and died in twenty minutes.

Post-mortem.—The whole body very much emaciated. Abdomen extremely tense and perfectly tympanitic on percussion. On opening the abdomen not a trace of the colon could be seen, the whole anterior portion of the cavity being filled with the enormously distended small intestines. On turning these aside, however, the colon was found closely pressed against the posterior walls of the abdomen, and so distended as to have lost all sacculated appearance, and become perfectly smooth. It was completely filled with perfectly healthy-looking fæces of a normal colour and consistence, and totally free from scybala. The mucous membrane of the colon showed several dark sloughing ulcers near the hepatic flexure, one of which had perforated the peritoneum, and a small quantity of feculent fluid had escaped. At the rectal end of the sigmoid flexure there was a stricture of dense fibrous tissue nearly an inch in length, the opening through which would hardly admit a crow-quill. The rectum was perfectly empty and dilated, its inner surface, congested with many points of ecchymosis.

There is nothing in this case that bears any relation to homœopathy, as the condition proved to be one entirely beyond the reach of medicine of any kind; but there are some points of considerable interest in a medical point of

view ; the chief of these being the fact of *perfectly healthy fæces* being found under such circumstances. That a person in such a state of disease should have an accumulation of fæces going on increasing for eight weeks, during which time the containing intestine had ulcerated and sloughed, and yet that these fæces should have retained the appearance, consistency, and odour of perfectly healthy evacuations, was certainly not to be expected ; and one cannot help asking ourselves the question whether, after all, we are quite sure that the characters given as distinctive of normal fæces are perfectly trustworthy. Most undoubtedly in this case had the patient passed any stools having the character of what was found in the colon, it would have been at once and unhesitatingly stated that, at any rate, the colon and rectum must be in a healthy state, and yet a portion of it was studded with sloughing ulcers.

By Dr. R. D. HALE.

Case of Chronic Cystitis.

M. A. Dilloway, æt. 53, admitted an out-patient Feb. 17th, 1869. Has been lately in St. George's Hospital for five weeks, when various remedies, including injections into the bladder, were tried without the least relief ; but, on the contrary, a great aggravation of symptoms from which she has suffered more or less for twelve years. She has had nine children and seven miscarriages. Traces her complaint to wet feet after a confinement.

Present Symptoms.—Dreadful pain in the bladder, with forcing and cutting pain along the course of the urethra ; scanty emission of hot urine, with bearing down as if the contents of the abdomen would be forced out ; there is a sensation of swelling of the anus produced by hæmorrhoids which bleed ; bowels alternately relaxed and constipated ; tongue foul. *Bellad.* 3^x, 4tis horis.

Feb. 24th.—Much the same ; bowels very costive ; fæces lumpy. *Nux vom.* 3, o. n., *Canth.* 3^x, 4tis horis in die.

March 3rd.—Feels better than she has done for a year ;

retains urine better, which is passed with less pain, and is less hot; bowels more open. Rep.

10th.—Still improved; passes water freely; bowels difficult; hæmorrhoids are troublesome. *Sulphur* 5, o. n.; rep. *Canth.*

17th.—Rather more pain in urinating; bowels better. *Nux v.* 3, o. n., *Canth.* 6, ter die.

24th.—Was better for some days; there is still frequent micturition with pain and bearing down. *Sulphur* 6, o. n., *Canth.* 3, 4tis horis.

April 7th.—Much better; all the symptoms getting better; bowels costive. *Pulsat.* 3, o. n.; rep. *Canth.*

21st.—Still better; can retain urine much better; much less pain; costive at times. Rep. *Canth.* 3, ter die

May 5th.—Better. Rep.

19th.—There is conjunctival inflammation of the right eye; tongue very foul, bitter taste in the mouth; forcing pain in the bowels; fæces white; symptoms of the bladder not quite so well. Rep. *Merc. sol.* 5^x, 4tis horis.

26th.—The conjunctiva very injected; sleeping badly. *Bellad.* 3^x, 4tis horis.

June 2nd.—All symptoms much better. Rep. *Bell.*, ter die.

16th.—Forcing in region of the bladder. *Canth.* 3, ter die.

July 7th.—Improving; she has not had any medicine for a fortnight; bleeding piles. *Sulph.* 6, o. n.; rep. *Canth.* 3^x.

21st.—Much better. Rep.

Aug. 18th.—Still better. Rep.

22nd.—Very much better; has some pain occasionally. *Canth.* 6, bis die.

Oct. 14th.—Sharp pain in bladder, but urination easier. Rep. *Canth.* 3, ter die.

1870, Jan. 12th.—Slight return of symptoms, but *Cantharides* and *Dulcamara* were given, and, being well, ceased to attend.

**OBSERVATIONS ON WHOOPING-COUGH, WITH
A FEW CASES.**

By WILLIAM V. DRURY, M.D., Physician to the London Homœopathic Hospital in charge of Diseases of Children.

LECTURERS on clinical medicine desirous of calling the attention of their pupils to some particular disease are often compelled to wait some time before a case is admitted into the hospital that will serve as an illustration, while, on the other hand, some diseases are never absent from the wards. Among those diseases that can always be represented, if it is desirable to admit such cases, is whooping-cough. Thus, in the dispensary practice at the London Homœopathic Hospital, amongst the diseases of children there are always some suffering from this complaint. The number may vary with the season of the year—thus, during the winter months there may possibly be a greater proportion of cases on the books, owing to the fact that the disease is more protracted at that time, but unless there is an epidemic tendency the numbers do not vary very much. When we look to the death rate of the metropolis and observe an increase in the mortality from whooping-cough, it does not follow that the disease is more prevalent, but it may be that there is a large number cut off from causes due to the severity of the season, in the same way that we see the deaths from bronchitis increase.

It is often thought that disease may be spread amongst those collected together in the out-patients' waiting-room of our hospitals. I have no doubt whooping-cough is so spread, and this has frequently been a matter of concern to me; but it is almost impossible to guard against it; parents bringing children suffering from whooping-cough may be told to sit apart from the others, but cases come when it is not known that whooping-cough exists, and against such no provision can be made. Children free from the disease may escape taking, it occupying the same house, or even room,

as others who have it, if they are prevented coming too near one another.

It is thought by some that whooping-cough may be conveyed by a person going from one house to another, or by clothing. Unless by means of the sputa becoming dry and being carried on clothing, and then getting detached in some way, and then coming in contact with the air-passages of some one who has not had the disease, it is difficult to conceive how otherwise it could be spread to persons at a distance.

Though whooping-cough is, as a rule, a disease of childhood, no age is exempt from it. A child has been born with it, and at threescore years and ten it is occasionally met with.

Like other diseases that show the peculiarity of only attacking the same individual once during life, whooping-cough is said to have its exceptions. That a cough will at times present many of the features of whooping-cough in a person who has had it before, when living in the house with others who have the disease, there is no doubt; and this may give rise to the belief that the disease has appeared a second time, but whether such a person could communicate the disease I very much question. Where there is any hysterical tendency the disease is often very closely imitated, so that in this way a person may appear to have the disease twice; or some irritative impulse, or even morbid influence may produce it. Again, at times, the character of the prevailing cough of the season may be convulsive, and present many of the features of the whooping-cough. I recollect a few winters back finding vomituration, or even actual vomiting, with cough, by no means uncommon among my patients; and the presence of this peculiarity led me to the selection of *Carbo vegetabilis* as the suitable remedy, which acted exceedingly well. In those cases where absolute vomiting took place I may have given *Drosera*, but *Carbo vegetabilis* seemed to be the medicine of the season. Medical men, if they looked over their prescription book, would often find that at one time they frequently used the same medicine: it would appear that some leading symptoms present, due to what Sydenham called the epidemic constitution of the year, had led to the selection of the same medicine in many cases.

Though routine practice is very objectionable, still, for a time, such routine would be found very successful ; but then it is a routine founded, while the peculiar characters of a disease last, on true homœopathy.

The severity with which some children are attacked with this disease, while others escape with a mild attack, is chiefly due to constitutional causes. A child with a tendency to convulsions may be attacked with them during the progress of the disease, and consequently be exposed to considerable peril, for this complication is always dangerous. That the presence of worms, or what I often speak of as "a worm constitution," exercises a marked influence on the disease, I have no doubt. The large number of cases that pass through my hands, and the observing the effect of treatment where this has been kept in view, confirms me in this opinion. Highly strumous children often suffer severely and for a long time with copious secretion from the bronchial tubes. In such cases the after effects are apt to continue for some time. It does not, however, prove the existence of highly marked struma if the disease continues long in healthy children, especially if they do not get change of air at the proper time. Nor does the return of a convulsive cough long after the disease had apparently passed off prove any special weakness ; this peculiarity may show itself without causing any alarm for some months.

Uncomplicated whooping cough may be exceedingly violent, and yet free from any element of danger. If protracted, danger may arise from exhaustion ; but if such a case is met with, there is in all probability something lurking in the back ground, that if recognised would at once remove it from the class of uncomplicated. Thus struma, or chronic bronchial catarrh, may cause a case to be protracted ; the convulsive character of the cough, which shows itself as the cough returns from some fresh irritation induced by changes of weather, is more annoying than dangerous ; but the cases that cause alarm are those complicated with convulsions indicating cerebral mischief, bronchitis, pneumonia, or pleurisy. In some cases inflam-

mation of stomach or intestines may take place, and cause trouble. Excessive vomiting and straining, when present, must act more or less injuriously on the stomach.

The time whooping-cough takes to develop itself after the seeds of the disease have been sown, is uncertain. After five or six days, catarrhal and feverish symptoms may show themselves, and an attack of pneumonia or sharp bronchitis usher in the convulsive cough. Whooping-cough may prove fatal in this early stage from these complications, even before the characteristic cough is established. It is well to be aware of the possibility of whooping-cough being about to supervene when dealing with one of these diseases, as it enables us to give a more favorable prognosis in some cases than we otherwise might do. In other cases, as pleurisy, the presence of the cough must add greatly to the distress of the patient, happily this combination is very rare. It is very uncertain when the peculiar cough will show itself, as an indefinite sort of cough may last for many days before the regular one sets in; but it most frequently comes on in a few days. At first there is a violent shaking cough, the child getting red in the face. After this has been fairly established, the whoop comes on; or if not a distinct whoop, the cough is so peculiar that but little doubt exists as to what it is. Vomiting may or may not be present. The cough will usually continue severe from two to four weeks, but may do so much longer. It is in protracted cases, where the cough has lasted about six weeks, that change of air is so valuable. My experience of change of air in the early stage, unless for the sake of getting from positively bad air to good, is not favorable; but at a later stage it is one of the best things we can recommend.

It has not been my intention to give a regular description of whooping-cough, but rather to preface a few cases with some general remarks, and a short notice of the medicines most useful in homœopathic practice.

It is impossible to treat whooping-cough or any other disease properly without knowing the order in which the symptoms show themselves, and what complications or variations may be expected, and what medicines may be

relied on to meet these conditions. Though allopathic practice is very inferior to homœopathic in treating the disease we are speaking of, it is not so deficient or unmeaning as it is in some other diseases. Thus, if the convulsive character of the cough is supposed to depend on irritation of the vagus nerve, the use of sedative embrocations, or the administration of such medicines as *Belladonna* or *Hyoscyamus* may be useful. When the cough is excessively violent, *Hydrocyanic acid* is also given to check it; but this medicine cannot be used with impunity. Many years ago I saw it given in a case complicated with convulsions; instead of stopping them it produced a form of convulsion peculiar to itself, on one occasion the child lying for an hour pale and deathlike, so that it was almost impossible to say that life was going on. I had asked the doctor who ordered it—a man of great eminence in the profession—if he thought the *Hydrocyanic acid* could be producing the convulsions; he thought not; but after this severe attack I declined giving any more of the medicine, and there was no more return of convulsions. When I became a homœopath, I read in Jahr, under the head of *Prussic acid*, among the symptoms produced by it, “cataleptic convulsions.” The allopaths have sought in *Alum* and other medicines a specific for the disease, but without success; but could they find one, in cases of complication they would continue to break down, while it is in such cases that homœopathy has such immense advantages; but if an attempt is made to follow homœopathic routine, failure and not success must result. From time to time we meet amateurs, and sometimes doctors, who have a sort of homœopathy at their fingers’ ends, who can tell at once from the name of the disease what medicine to give—thus, if whooping-cough is the disease, *Drosera* is the medicine. Now, valuable as this remedy unquestionably is, and useful as its provings show it must be in a large number of cases, there are cases where it is very inferior to other medicines, and, if trusted to alone, may lead to the loss of the patient. This is one of the dangers of domestic practice, but one that there is no guarding against in this or any other complaint, where

knowledge of disease is not thought necessary if a good domestic guide is at hand. Such cases bring discredit on our system, and entail great anxiety on the medical man called in at a late stage of the disease, when precious time has been wasted in the vain effort to do without the doctor.

As whooping-cough is apt to set in with catarrhal and feverish symptoms, there are some medicines more than others that occur to us. Thus, if called to see a child suffering from an influenza cold, we may order *Nux vomica* or *Mercurius*, and perhaps *Aconite* at night to allay the fever. In this stage *Kali hydriodicum*, a medicine less used than it ought to be, will be found worth a trial. If the cough is attended with some hoarseness and scraping, *Kali bichromicum* may be of use. The presence of croupy cough will point to *Spongia*, *Hepar*, and *Iodium*. If inflammation of the lungs is to usher in the cough, then *Aconite*, *Bryonia*, and *Phosphorus* are the medicines most likely to be needed; but others, as *Antimonium tart.* may be required. As the cough advances, if there is no pneumonia, *Ipecacuanha*, *Hyoscyamus*, and *Belladonna* will engage our thoughts; but the medicine I have, I think, most frequently given in this stage, is *Carbo vegetabilis*, the catarrh and character of the cough being indications for its use; and unless the patient is seen very early in the disease, it is more frequently needed than the medicines I have named. This medicine in cough should, I believe, be given in not too low a strength, while in gastric affections it may at times be given with advantage in the triturations; but if the practitioner has a leaning to large doses, I would advise the use of *Ipecacuanha* or some one of the other medicines I have named, as preferable to a low dilution of *Carbo*. At a later stage of the disease, when the vomiting has ceased, but when there is much expectoration, it may again be of use. Rawness in the chest is an indication for its use, as it is also for *Phosphorus*. When vomiting is established, *Drosera* is the medicine most frequently needed; but it is the complications that arise, or constitutional or disturbing causes that call for the administration of other medicines instead

of, or intercurrent with, *Drosera*. Thus, if *Drosera* is our chief medicine, it may be given through the day, while if the child is restless and disturbed at night, *Chamomilla*, *Belladonna*, or *Aconite* may be given in the evening and through the night. If the presence of worms call for *Cina* or any other medicine, it may be given at night; but as the cough is often a marked indication for *Cina*, it is the chief medicine for worms connected with whooping-cough. The rigidity of the body before the cough, the moaning after the cough, the throwing about of the limbs, the interrupted breathing, all point to *Cina*. In no cases of worms have I seen *Cina* act better, and their presence should never be overlooked in this disease, for some way or another they seem to cause a good deal of disturbance.

Convulsions dangerously complicate whooping-cough, and cause a good deal of mortality. *Belladonna* is our chief medicine; but *Hyoscyamus*, *Nux vomica*, *Ipecacuanha*, *Cuprum*, *Ignatia*, and *Opium*, each as indicated, may help. Dashing cold water on the forehead is a remedy that can be used instantly, where a hot bath cannot, and should not be forgotten. When convulsions threaten, supposing *Drosera* or *Cina* to be indicated for the cough, one of these medicines may be given every one or two hours as needed, while *Belladonna* is given night and morning; and if an attack comes on, every ten or fifteen minutes till it has quite passed off. A crushed pilule, three globules, or drops, or portion of a drop of tincture on some powdered sugar, may be placed on the tongue.

Epistaxis will generally be met with *Belladonna*, *Ipecacuanha*, or *Drosera*.

The accumulation of mucus in the chest, with the inability to raise it, arising from its quantity, adhesive character, loss of power to throw it off, or incipient paralysis of the lungs, demands our immediate attention. The rattling breathing, dry cough, raising of blood, effort to vomit, may lead us to give *Ipecacuanha*. *Drosera* or *Stannum* may be of use, also *Carbo vegetabilis*; *Cuprum* where there is rattling of mucus, blueness of the skin, fainting, asthmatic or interrupted breathing, spitting of blood; but the

medicine most frequently needed, and which, I think, is our sheet anchor in these cases of suffocative accumulation in chest, is *Antimonium tartaricum*: this should be given in rapid repeated doses. This is a medicine I have not given in what may be called tangible doses; I have given it again and again with the most gratifying results in the 6th, 12th, and 30th dilutions; in fact, with such good results that I have not cared to use it in a lower strength.

Veratrum may help us in some cases of great exhaustion induced by the cough, with distress about the heart, and threatened collapse.

Arsenicum may be of use in cases of debility; and this or the preceding medicine will be of use where diarrhoea comes on in the course of the disease, as sometimes happens.

When whooping-cough is protracted, *China* may give material help; but, indeed, at any stage it may prove of use where there has been exhaustion: night cough or cough after meals would specially indicate it. When bronchitis is present *Bryonia*, *Chamomilla*, *Aconite*, *Hepar*, *Spongia*, *Phosphorus*, *Ipecacuanha*, *Carbo vegetabilis*, &c., may be called for.

I have already spoken of change of air as the best remedy in protracted whooping-cough; but as this cannot always be managed, we must be guided by existing symptoms as to our choice of remedies; and as the cough varies, old symptoms reappearing, medicines that had been laid aside may again be required. *Carbo vegetabilis*, *Drosera*, and *China* are those we perhaps most frequently use; but others that will occur to the practitioner, such as *Conium*, *Silicea*, and *Sepia*, will come to our aid; purulent discharge from the lungs, if present, may call for one of these, or an offensive expectoration may turn our thoughts to *Pulsatilla*.

Many medicines might be enumerated that would aid us at times, but naming them might also cause confusion, and as my object is rather to give an outline of what the treatment should be, with such help as my own experience and judgment can suggest, no good would be gained

by a bewildering record of medicines that a good repertory and a *Materia Medica* may better point out.

The following cases from my hospital note books are chosen, not as the best selection that might be made, but rather because they occur in the same volume. The notes were taken for the convenience of remembrance and prescribing, and not as full narratives, so that they are necessarily imperfect.

CASE 1.—November 1st, 1864. Catherine H—, æt. 15 months, is brought to the hospital suffering from violent cough attended with retching and vomiting; gets red in face from violence of cough; had wheezing breath, but that is better; much mucus in chest. *Drosera* 30; 3 pilules in 12 teaspoonsful of water, a teaspoonful every two hours. If much distress from mucus in chest, to have *Antim. tart.* 30 instead of *Drosera*.

8th.—Cough much better, but frequent whoop continues. Continue *Drosera*.

27th.—Cough a great deal better. *Drosera* every four hours.

Dec. 20th.—Cough troublesome the last few days, with great restlessness at night; apparently worse after medicine.

In my absence the house-surgeon omitted medicine. As the irritation probably arose from teething, *Chamomilla* might have been given.

1865, Jan. 8rd.—Cough less. *Drosera* three times a day. The child got well, but more than a year later on, viz.

1866, Jan. 30th.—Child is brought with a cough similar to whooping-cough; whoops and vomits; there is rattling of mucus in chest. *Ant. tart.* 30; 3 pilules in 9 teaspoonsful of water, a teaspoonful 3 or 4 times a day.

Feb. 8rd.—Cough much better; child is weak. *Ant. tart.* three times a day. No further treatment was required.

This case shows in a remarkable manner at how late a period a fresh cough may imitate whooping-cough.

CASE 2.—Nov. 8th, 1864.—Alice B—, æt. 14 months. Is brought, suffering from a violent cough for the last four-

teen days. Has been heard to whoop; vomited at first; cough very bad at night; wheezing breathing at night; is pretty well through the day; some hoarseness. *Carbo vegetabilis* 30; 3 pilules in 12 teaspoonsful of water, a teaspoonful every three hours.

Nov. 18th.—The child being worse, at the request of the mother I visited her at her own home, and finding much accumulation of mucus in chest, gave *Antimonium tartaricum* to be taken at any time child was much oppressed with phlegm, at other times *Drosera*.

Nov. 22nd.—Child is improving; better in herself; takes her food; no vomiting, but still accumulation of phlegm in chest. *Dros.* 30, every three hours, and if mucus *Ant. tart.* every hour till relieved.

As a supply of medicine, to last some time, was given, nothing further was required.

CASE 3.—Nov. 8th, 1864. Jessie W—, æt. 3 weeks, was brought, suffering from whooping-cough. The cough was violent and there was some vomiting. *Drosera* every three hours.

Nov. 22nd.—Cough well, but the child was suffering from flatulence, pain, and green stools. *Chamomilla* 30, every three hours.

Dec. 13th.—Relief was obtained, and at this date the child was reported much better. No further medicine was needed.

The only point of interest in this case is the early age at which the child had the cough, and its comparatively short duration. There is unfortunately no record of how long the child had been coughing, but as the cough was well established when seen, it most probably was ill from its birth.

CASE 4.—Jan. 3rd, 1865. William L—, æt. 6½. It is stated that this child has already had whooping-cough. However, he is now suffering from violent cough, producing redness of face and vomiting. *Drosera* four times a-day.

Jan. 10th.—Cough better. Continue *Drosera*.

Jan. 24th.—Cough rather less frequent; nose has bled twice. Continue *Drosera*.

In February the child was improving; but a change in the weather brought on a return of the cough, which was severe at night. *Drosera* was continued.

The cough continued in March; some *Bryonia* was given with advantage, and on the 14th, as there was much mucus with cough, *Carbo vegetabilis* every three hours.

In April, having finished medicine and being without it some days, cough again returned, *Carbo v.* was again given. After this, nothing further was required, till the child came again to the hospital for another complaint—discharge from ear.

This case presents many of the features of whooping-cough, it very possibly was a return of the convulsive cough, continuing for a long period, as it returned during the winter months. The stethoscopic signs have not been noted in these cases, and might generally be set down under the head of sibilant and mucous rales, unless in cases where there was pneumonic complication. In such cases crepitation and absence of respiration in portions of the lung would be our diagnostic indications.

REVIEWS.

Applied Homœopathy; or, Specific Restorative Medicine.
By WILLIAM BAYES, M.D., &c. Turner & Co.

THE papers comprising this volume have already appeared in the *Monthly Homœopathic Review*, under the titles of "Cure-Work" and "Facts and Impressions derived from Ten Years' Homœopathic Practice." But we think that Dr. Bayes has had good warrant for republishing them in a more permanent and accessible form. They present an agreeable picture of the theory and practice of homœopathy; one containing much to recommend it to outsiders, and not a little to quicken and enlarge the apprehension even of its disciples. The "Specific Restorative Stimulation," which is the author's phrasing of the *rationale* of our practice, has much plausibility as far as it goes. It is not, indeed, much more than an aspect of the organopathy of which Dr. Sharp is the latest expositor. It classes all drugs as stimulants or paralyzers, according to dose, of the nerves of the parts on which they act. It supposes all diseases to be a loss of equilibrium, and drug-action in cure to be a graduated righting of the disordered balance. But it takes no account of that specificity which belongs, not to *seat* only, but to *kind* of morbid action; and in which alone the rule of similarity finds its full application. Local affinity and restorative stimulation are insufficient to account for homœopathic action here.

In his second chapter, Dr. Bayes uses the word "entity" as applicable to such diseases as smallpox and measles. He seems to have been much taken to task for this expression in a discussion at the British Homœopathic Society, lately reported in our columns (p. 140 of the present volume). We think that his use of the term was misunderstood. He was not maintaining that disease is something distinct from the diseased organism; something which can be indepen-

dently antidoted or eliminated. But he was asserting what Tessier called the "essentiality" of disease, and which he and his school, following Bretonneau against Broussais, manfully contended. A dog is a dog, in whatever variety of size, colour, and capacity he may exist. He is specifically distinct from other animals; and he propagates his kind, and no other. In like manner smallpox, gout, syphilis, are true *species*, and their genuine *simile* must be one corresponding not merely to the group of symptoms present to-day, but to the whole or to a definite portion of the proper life of the malady. As he says, "In a case of intermittent fever we do not treat the chill stage with one drug, the hot fit with another, and the perspiration with a third, but we administer a medicinal drug which has the power to induce all these consecutive conditions in the order of their natural course, and which will induce, if long-continued, a cachectic state similar to the sequelæ of the disease." There is nothing but what is true and valuable in all this.

The observations on the properties and sphere of action of our medicines, which form the bulk of the volume, are full of practical life. They do not profess to be a treatise on pharmaco-dynamics; but a record of the "facts and impressions" about drugs to which the author's own experience has led him. They are of the same order as the "Notes on Medicines," which the late Dr. Chapman contributed to our earlier volumes; and, while adding material for future therapeutic generalization, supply many a useful hint for present practice.

The Homœopathic Medical Directory of Great Britain and Ireland, for 1871. London: Turner & Co.

THIS annual is so well known to all our readers that it requires no lengthened notice from us. Suffice it to say that it increases in excellence with every year of its growth.

We grumbled a little the year before last because it did not contain by any means a perfect list of the names of foreign homœopathists. This year that want has been to a very considerable extent supplied. The European list, however, contains the names of many practitioners who have been dead several years. The United States list would be more handy for reference if the alphabetical order had reference solely to the names of the towns, for it is not likely that any European, far less an average Briton (unless freshly crammed for a Civil Service examination), would be able to tell off-hand the particular State under the heading of which he must seek for various small towns in the great union. The South American list is defective in making no mention of the homœopathists of the United States of Colombia, whose numbers are by no means despicable, and to judge by what we read, homœopathy makes considerable stir and progress in that part of the Continent, for there is a goodly number of practitioners, and a monthly journal is published at the capital, Bogota ; and, as we learn from the journal, there is a hospital where homœopathy is practised at the holy city of Chiquinquirá. But we cannot expect perfection all at once, and we are much pleased at the great stride towards it taken by the Directory for this year. One more hint, and we have done. In the calendar, which contains a judicious mixture of bloody military victories, and bloodless homœopathic triumphs, we miss the notification of Hahnemann's birthday on the 10th or 11th of April, which would, we imagine, be as well worth recording in a homœopathic calendar as the battle of Saarbrück, or the burning of Moscow.

Annual Record of Homœopathic Literature, 1870, edited by G. C. RAUE, M.D. New York : Boericke and Tafel.

THIS is the American work corresponding to our *Homœopathic Directory*. The Directory portion of it, however, is

quite secondary in importance to the Record part, and is merely tacked on to the end as a sort of appendix, not even following the paging of the main part of the work. It professes to give a list of all the homœopathists in the New and Old World; but as regards the latter part of the globe the list is still more imperfect than that of our British Directory; for while the editor of the latter has taken pains to weed out from his list the names of the most conspicuous adherents of homœopathy who have departed this life, the American editor boldly represents Leon Simon the father, Tessier, Fleischmann, Weber, Elwert, and the Wolfs of Berlin and Dresden, to be still alive and kicking. We see our British friend has followed his American leader in keeping the four last physicians in this world; but it is entirely the fault of the Germans and French themselves, who will not take the trouble to publish a directory oftener than once in fifteen years or so.

But, as we said, the Record is the chief feature of *Raue's Annual*, and he has had able assistants who have helped him to make abstracts of all the homœopathic periodicals published in 1869 on both sides of the Atlantic. In fact, this is, as it were a *Braithwaite's Retrospect* of Homœopathy, and, as far as we can judge from a cursory inspection, the gigantic task has been very well performed. An excellent index (excellent indices we should rather say) accompanies the volume, making it the easiest matter in the world to refer to any desired name, subject, or remedy in the book. We seriously trust Dr. Raue will continue his work from year to year. We could scarcely expect to see the editor of the *British Directory* attempt anything so comprehensive as the Record Dr. Raue has prepared with the assistance of his numerous coadjutors; we feel it would be out of place in a British directory, and even were it not so, it would be utterly impossible for any any one man to do it satisfactorily, and co-operation in literary work is no easy matter in this country, as many of us know. There is, however, plenty of room for both works, and we trust to see them both flourish in perpetuum.

CLINICAL RECORD.

Dysentery seated in the Rectum cured by Podophyllum after the failure of other medicines.

By MR. J. HARMER SMITH.

Mrs. —, Lewisham, æt. 45.

March [17th, 1869.—Sudden attack of pain in the rectum accompanied with bearing down, and frequent scanty mucous evacuations; tenesmus and strangury; no abdominal pain or fever; has been constipated unusually of late, which she attributes to the use of *Ferrum redactum*. *Merc. cor.* 2, in alternation with *Ignatia* 1.

18th.—About a dozen motions a day. Some strangury still; motions nearly consist of pure mucus tinged with blood. Cont. *Merc. cor.*, omit *Ignatia*.

19th.—No relief to symptoms.

20th.—Has passed some pure blood, bright coloured, followed by syncope. *Ipecac.* 1, in alternation with *Merc. cor.*

21st.—No return of bleeding; mucous discharge. As before.

22nd.—Several motions consisting of dark-coloured scybala; mucous discharge continues unabated. Continue medicines. To have an enema, consisting of a pint of tepid water.

23rd.—Abdominal pain since the enema; also parted with more scybala; but no abatement of the mucous discharge.

Finding so little improvement in the symptoms under the treatment I had so frequently found curative in dysentery, I recollected that Dr. Hughes, in his *Pharmacodynamics*, strongly recommends *Podophyllum* in dysenteric diarrhoea affecting the rectum. I therefore discontinued the other medicines, and gave *Tincture of Podophyllum* 1, 5 drops every four hours.

24th.—Has had no return of abdominal pain or of mucous discharge since she began to take the *Podophyllum*.

30th.—No return of symptoms.

She had some months afterwards a slighter return of the complaint,

which was cured by the same medicine, which she seemed surprised I had not given her before.

Caladium Sequinum in Pruritus Pudendi.

The same lady, who is remarkably susceptible of the influence of homœopathic remedies, recently consulted me for this troublesome affection, for which she had been using local applications for several days.

I prescribed merely from her report—she said that the labia externa were swollen, and that there was a papular eruption on them. I gave her *Caladium Sequinum* 1, without directing any local application.

When I visited her on the following day she told me that her intolerable complaint was quite cured, that the itching had ceased, and the parts, as it were, “shrunk up.”

Two cures with Calcareo.

We take the following two observations from consecutive members of the *Allgemeine Homöopathische Zeitung*. The first is by Dr. Carl Heinigke, of Leipzig.

“Mrs. P—, a widow lady between 40 and 50 years of age, a large woman, with well-rounded form, brown hair, blue eyes, white delicate skin, rosy cheeks, regular menstruation, excitable temper; had already suffered for several successive years, in the spring, from gouty affections of the joints. Once only did the gouty diathesis not localize itself in the larger joints, but tortured her for several weeks in the form of cardiognus and asthma. Her doctor, an allopath, with his various mixtures and such like remedies, had always required from six to eight weeks to let the disease get gradually well in spite of his medicines. In 1867, when the usual paroxysm came on in spring, he declared that the patient must go to Teplitz as he thought he could do no more for her; and the patient had long been of the same opinion. When, therefore, a new gouty attack came on in the spring of 1868, the aid of homœopathy was sought. “It was the 25th of June, the patient had already been in bed some days without the power of moving. Both knee-joints were

affected; hot, pale, swollen, highly sensitive to touch, constantly in pain, most so, however, at night. Little fever, sourish taste, increased thirst, loss of appetite, costive bowels; dark, scanty, sour urine. Restlessness and sleeplessness; dry skin. As there were no violent symptoms requiring immediate removal, the disease being deeply rooted in the system, I resolved on wasting no time with vegetable remedies, but prescribed at once *Calcarea carb.* 12, in globules, with milk sugar; a powder to be taken every morning before breakfast.

"On the 28th June, in the afternoon, the patient tried to get up; the experiment was successful. On the 1st July she visited me in order to show me that she could again walk. For the last three days she had perspired a little in bed, there was still some tenderness to strong pressure between patella and condyles.

"As there still remained some tendency to the formation of acidity and costiveness, I gave on the 10th July five doses of *Lycop.* 12, in globules, a dose once a day. Since then she has remained well and happy.

"In November of this year I observed a similar rapid cure by *Calc. carb.* 30, five doses, in a lady between 60 and 70, but where *Bryon.* 12 had been given for some days previously. In this case, in additions to the knee-joints, the right hip-joint and the left shoulder-joint were affected with arthritis. The constitution similar to the former case. It was interesting to observe that the attacks of gout, which returned almost every year, alternated with angina parenchymatosa; so that in the year when the quinsy occurred, no gouty attack supervened, and *vice versa*. I gave this patient also during her convalescence, *Lycop.*; and some weeks later, when she complained of nothing but a little weakness, a few doses of *Baryt. carb.* 30. Eight months have elapsed, and she has remained well, but it remains to be seen how she will get through the winter."

The other observation on *Calc. carb.* is by a writer who gives only his initial, "G."

"True scrofulous ophthalmia is very obstinate. Although *Acid. nit.*, *Hepar*, *Merc. sol.*, *Ars.*, and *Sulph.*, and some others, are the most important remedies, they sometimes fail us. This can scarcely be said of *Calc. carb.*, a very powerful *antiscrofulosum*. To give the last case that fell under my observation: on the 11th July a girl of seven was brought to me affected with the symptoms of ordinary scrofulous ophthalmia, which it would be superfluous to

describe. Three drops of *Calc. carb.* 12, mixed with three table-spoonfuls of water; of this a teaspoonful night and morning, was my prescription. In a fortnight a remarkable change had occurred in the eyes that had been ailing for weeks; indeed, I may say months. The mother of the child says that the eyes, which had been at first dull and dim, had become as bright as natural, that they had never once been gummed together after commencing the medicine. The improvement could be seen from day to day. At the first glance I was convinced that the eyes were very much improved. The action of the remedy, and the proposed influence of the *Calcareæ* was all the more evident, as after the last dose the improvement which had steadily gone on was evidently arrested, rendering a repetition of the remedy necessary. Relying on much experience, I can advise that in true scrofulous ophthalmia no time should be lost with other remedies, but that the most evidently specific one *Calc. carb.* in various potencies (6, 12, and 15) should be given. Of course the remedy will not have the effect of absolutely cutting short the disease; and perhaps *Bellad.* would do more in the first days than *Calc.* But whenever marked photophobia, even ulceration, spasm of lids, burning, lacrymation, redness, even the so-called scrofulous vascular band of Adam Fischer, at the apex of which is a phlyctena, are present, then *Calc. carb.* is the remedy."

These two observations, though there is a sort of resemblance between them, yet offer a marked contrast. While in the first the disease is well described, in the last it is not described at all; and we are at a loss to know why it should be thought insufficient to describe a case as being one of rheumatic inflammation of the joints, and yet that it should be held sufficient to say of another case that it was ordinary scrofulous ophthalmia. The latter disease has, as our readers are aware, as many varieties as the former; but we are left to conjecture which of the fifty varieties of scrofulous ophthalmia our author had to treat. Altogether the last case may be held up as a warning how not to give the history of a case, for it teaches us absolutely nothing, and serves only to mislead. It is in vain that the writer says, towards the end, that *Calc. carb.* is the best remedy in all the varieties of scrofulous ophthalmia he enumerates; this is not likely to be, and we know it is not the case.

Yellow Fever in Barcelona. By Dr. GERARD VALARDELL.*

Shortly after coming here the yellow fever broke out, and I offered my services to the public, as I had acquired some experience of the disease in Cuba, where it is endemic. For truth's sake I must confess that with regard to this epidemic there has been much cry and little wool, as the saying is; the panic among the inhabitants being all the greater on account of their recollection of the ravages caused by the epidemic of 1821.

The majority of the cases I observed offered marked modifications, doubtless due to the effects of the climate. Thus, the general form of the disease in America is *ataxic*, with all the varieties incident to this form; but here the common form was *adynamic*. In the West Indies, about the fifth or seventh day, the patients usually either die or the malady loses its gravity, and they enter on convalescence. Here, on the contrary, the stages were longer, extending over three weeks, like the ordinary typhus of Europe. Here the icteric tint was more pronounced, especially in the conjunctiva; also the bleeding from the gums and the petechiæ, which are seldom met with in Cuba. The vomit and stools were more bilious, and less like coffee grounds. In a word, though it was essentially true *Typhus icterodes*, it presented many of the characters of the European disease in its mode of development; and in this also that *Rhus* was found in many cases to be very useful; but it was necessary to recur frequently to *Arsenic*, *Lachesis*, *Crotal. hor.* and *Carb. veg.*, according to the tableau of symptoms, on carefully individualizing the cases.

In America, with *Acon.* and *Nux V.* alternately from the commencement; or *Bryon.* in some cases, and in others *Bellad.* when the brain was affected, I succeeded in cutting the disease short at its beginning; and also with *Ipec.* occasionally, when gastric symptoms predominated.

By these means I generally succeeded in cutting short the disease; preventing the occurrence of the peculiar *vomiting* and of the *ataxia*. At the same time I did what I could to provoke abundant stools and to promote perspiration. By these simple means I obtained many cures, preventing the disease from developing itself by passing into the second stage; and even when this could not be avoided, I have obtained brilliant results with *Lachesis*, which is a powerful remedy, *Arsen.*, *Crotal.*, &c.

* *Criterio Medico*, February, 1871.

Two cases. By Dr. DUDGEON.1. *Fissure of the Anus.*

In Vol. XXII, p. 350, an account of M. Maisonneuve's operation for fissure of the anus by dilatation, was copied from one of the medical journals, and since then I have had several opportunities of testing its value in this distressing disease. Some of these cases were of comparatively recent date, and the operation was easily performed, not attended by much pain, and the relief immediate. But a case that came under my care last December was one of the severest I had yet witnessed. The patient was a lady upwards of sixty years of age. She had already undergone a cutting operation for the disease six months before, with perfect relief for some months; but when I saw her on the 8th December, she complained of a return of all the old feelings. The motions were rather costive, attended by sharp pain, and for many hours afterwards she could get no rest or relief from the excessive spasmodic pain in the anus characteristic of this disease. Some blood was generally observed on the stools. I proposed to operate at once, but the ill-success of the previous operation made her very unwilling to subject herself to another operation; so I prescribed *Collinsonia* 1, three times a day, and a very mild ointment of nitrate of silver, 1 gr. to the ounce of lard, for local application. I had in former years seen good effects from this local application in fissure of anus. On the 15th she was no better; on the contrary, the pain had become even more intolerable, and she begged me to do what I thought best for her relief. I placed her on her left side in bed, and introduced the forefingers of both hands within the sphincter, which felt more like a piece of hard rope than human muscle. With the greatest difficulty, on account of the extreme rigidity of the muscle, I succeeded in over-stretching the spasmodically contracted sphincter, and I kept up the dilatation until I felt satisfied that every trace of the spasm was overcome. The patient was highly nervous, and the operation very painful, causing her to give sudden movements at the commencement, and making it difficult for me to retain my fingers within the anus. However, she was unable to displace my fingers, which was fortunate, as I believe she would not have consented to their reintroduction, so nervous was she. As the constriction yielded the pain subsided, and after keep-

ing up full dilatation for about two minutes or a little longer, I withdrew my fingers from the now thoroughly relaxed anus, and advised her to rest for a few hours, and take *Bell.* 1, every two or three hours. On the 17th she still complained of some bruised pain about the anus, and the bowels had not been moved since the operation. When they were moved, shortly after this, she was delighted to find that the former agonizing spasmodic pain was gone. She soon lost the bruised pain, and it is now upwards of three months since the dilatation was performed, and she has never felt the slightest sensation of her old enemy. It, of course, remains to be seen if the cure is more permanent than after the cutting operation. In this case the pain attending the operation was so severe that I regretted I had not given chloroform; but I had not found the pain in former cases of sufficient severity to require an anæsthetic.

2. Pustular Cutaneous Disease.

In Vol. XXIV, p. 311, I related the case of a young lady affected with a pustular cutaneous eruption on the face, of long standing, which had rapidly subsided under the use of *Tartar emetic*.

In detailing the history of that case, I mentioned incidentally that it had already been under the care of a homœopathic colleague without advantage. I have since learned from this esteemed colleague that he had prescribed the self-same remedy under the use of which the disease disappeared during my attendance, viz., *Tartar emetic*, and in the same doses as I had employed. This rather staggered me in my belief that the cure was due to the medicine given by me, and I was extremely anxious to meet with another similar case to put the matter to the proof. It was only about the end of February last that such an opportunity presented itself. I was called on the 25th of that month to see a young lady, aged 16, who had been affected for upwards of a year with a most disfiguring eruption on the face, consisting of small discrete pustules which, after drying up, left for a long time an ugly bluish-red mark, so that her naturally handsome features were quite spoilt by the blotches left by the old pustules as well as by the yellow-headed moist pimples. In addition to the eruption on the face, for upwards of a month she had been tormented by a similar eruption about the genitals and tops of the thighs, which was so excessively painful that she could not sit down without suffering, and was

utterly unable to walk even a few hundred yards, so tender had it made her. She was also latterly unable to sleep at night, on account of the pain and irritation of the eruption on the genitals; and she had quite lost her strength, spirits, and appetite. I at once gave *Tartar emetic*, trit. 2, one grain in nine tablespoonfuls of water, a spoonful to be taken three times a day. Under this medicine, in a week, the eruption had much declined on genitals and face; her appetite, spirits, and sleep had improved. In a fortnight the eruption was quite gone from the genitals, and she could walk about as well as ever; the eruption on the face was diminished to an insignificant rash, and the whole appearance and bearing of the young lady were vastly improved. In three weeks she was quite well, with the exception of just a trace of the eruption about the chin and nose (it had formerly been spread all over the face, the nose, forehead, cheeks, and chin); the appetite was much improved, and she could again enter on all her former occupations and amusements, from which she had been so long debarred. I saw her again four weeks after the commencement of treatment, and found her quite free from eruption of any sort, and, indeed, quite well in every respect.

I may remind my readers that Dr. Imbert Goubeyre, in his essay on "Antimonial Eruptions," published in the 19th volume of this Journal, has collected together a large number of observations, showing the specific power of *Tartar emetic* to cause eruptions on the genitals precisely similar to those my patient suffered from.

MISCELLANEOUS.

The one Subject on which all Allopaths Agree.

In the March number of the *Practitioner* Dr. Anstie attacks the therapeutical views of Dr. Wilks. We do not object to this, on the contrary, we experience a sense of satisfaction at the bickerings of our enemies among one another: we feel that when allopaths fall out, homœopaths ought to get their own. But these quarrelsome allopaths must needs go out of their way to have a kick at homœopathy. Misrepresentation of homœopathy is a subject on which they can agree, and by means of which Dr. Anstie can say polite things to Dr. Wilks to take the edge off the severity of his other criticisms. Says Anstie to Wilks, "My dear fellow, you are all wrong in other respects, but you are quite right about that detestable homœopathy, and I can join you in giving it a hearty kick." Now we need not stand this—and we won't. Here is what Dr. Anstie says:—

"It is when he speaks of the treatment of *diseases rather than symptoms*, however, that Dr. Wilks seems to us most thoroughly wrong. He is, in the first place, labouring under an entire misconception. He speaks of the treatment of mere symptoms as if it were the great characteristic of recent medicine. Now it is quite true that the treatment of symptoms is the prominent characteristic of *homœopathy*, as Dr. Wilks remarks. But that is the very reason, or, at least, one of the most powerful reasons why the homœopathic system has been decisively rejected by all the representative workers in modern therapeutics. To such men the fallacy of such a system is obvious, for its method rests upon a diagnosis which is a mere *inductio per enumerationem simplicem*. . . . We, on the contrary, believe that disease is the sum total of the actions evoked in the body by the action and reaction of certain external influences and certain individual peculiarities of structure."

Ah! Dr. Anstie, why did you not content yourself with your abuse and misrepresentation of homœopathy? Why did you attempt to formulize your own idea of disease, which, allowance

made for different modes of expression, differs in no essential particular from Hahnemann's own definition of disease in the sixth paragraph of the *Organon*.

"The unprejudiced observer," wrote the sage of Coethen, "takes note of nothing in every individual disease, except the changes in the health of the body and of the mind (*morbid phenomena, accidents, symptoms*) which can be perceived externally by means of the senses; that is to say, he notices only the deviations from the former healthy state of the now diseased individual, which are felt by the patient himself, remarked by those around him, and observed by the physician. All these perceptible signs represent the disease in its whole extent; that is, together they form the true and only conceivable portrait of the disease."

We trust Dr. Anstie will endeavour to show us in his next number wherein his idea of disease differs from that promulgated by Hahnemann, and why, if Dr. Anstie scorns the appellation of symptomatic treatment applied to his own practice, he should join with Dr. Wilks in branding Hahnemann's practice with that opprobrious epithet.

Public Lectures on Homœopathy in the University of Salamanca.

On the 23rd of January last was commenced the public course of lectures on homœopathy in the Faculty of Medicine of Salamanca, by Dr. Anastasius Garcia Lopez, who occupies the chair of physiology in that Faculty.

When Dr. Garcia Lopez asked the rector's leave to deliver a course of lectures, the rector referred his petition to the Faculty of Medicine to judge of its convenience, and that body named a committee to report on the subject. The committee drew up a voluminous report, in which it concluded that there could be no inconvenience in acceding to Dr. Garcia Lopez's request. But the Faculty rejected the committee's advice, and, after a warm discussion, arrived at a different conclusion. They asserted that it was impossible to grant the use of the locality desired in order to deliver a course of lectures on homœopathy, and, *moreover, that the said course would be prejudicial to the official teaching of medicine.* It is worth mention-

ing that no reasons were assigned for the latter statement either in the resolution handed by the Faculty to the rector, or in the meeting of the Faculty at which it was passed; no doubt they thought these high sounding words were sufficient proof of the impossibility of allowing the establishment of a chair of homœopathy by one who, till lately, had been their colleague in the Faculty.

However, leave was granted by the rector, and the course was opened by Dr. Lopez before a very numerous audience, composed chiefly of students of medicine and medical men of the city, among whom there was but one professor of the Faculty. The result of the first lecture and of the subsequent ones was highly successful, as was to be expected from the intelligence and profound learning of Dr. Lopez. All the lectures—which are delivered on Mondays, Wednesdays, and Fridays—are attended by large audiences. Very few of the medical students have ceased to attend on account of the warnings addressed to them by their professors to refrain from going, and many have taken diligent notes of the lectures they hear with such pleasure.

We understand that Dr. Garcia Lopez only intended in the present course to give a detailed exposition of the homœopathic system; but in consequence of the attitude of the Faculty of Medicine, he devoted the first lectures to a harsh and withering criticism of traditional medicine, especially of its *Materia Medica* and the actual practice of allopathy.—*Criterio Medico*.

To the Editors of the British Journal of Homœopathy.

GENTLEMEN,—Had my letter to you been correctly printed, I might well be content that it stood side by side with the comment thereon in the last number of your Journal. But through its typographical errors I am made to write very slipshod English, a fault which is almost as shocking to me as a breach of the homœopathic law. Thus—holding a cynic to be a cur, I am not likely to be guilty of the pleonasm of writing “cynic cur,” though *cynic fur* is a term, I think, used by writers of two or three centuries ago. Perhaps this example may typify and, as one amongst others, serve to show the kind of correction my missive stood in

need of, its errata being rather of the letter than the spirit. Your mode of showing a contributor respect; the esoteric meaning of the motto of the Journal; the *quantum valuit* of the judgment passed on Professor Pascucci, Count Mattei, and myself; and how far one is guilty of the lèse-majesty of medicine in curing disease by secret remedies, when it is not to be cured by any that are known; all this, and very much more besides, I leave to the consideration of your readers, if this short note may find insertion in the Journal to obviate the possible charge of the writing being as incorrect as the thinking and doing are said to be, of

Your obedient servant,

E. ACWORTH.

9, Montpelier Terrace, Brighton;

Jan. 7th, 1871.

Koumiss.

THE *Koumiss* or *Kumis* of the Bashkirs and Tartars, their almost universal remedy for all diseases, is simply mare's milk fermented in a peculiar manner. The invalids of these nomadic tribes resort to certain places renowned for the preparation of this milk, just as on the Continent crowds of sick people flock to certain spots where they can have their whey-cure, their grape-cure, their fir-needle-cure, their water-cure, or their thirst-cure. Dr. Jagielski has endeavoured to give us the advantage of the *Koumiss*-cure, by manufacturing an artificial *Koumiss*. As mare's milk is not readily procurable in this country, and its "horsey" flavour might not be appreciated by our invalids, this imitated *Koumiss* is prepared with cow's milk fermented in a peculiar way. There are three different kinds or strengths of it suitable for different kinds of cases. It is prepared and sold by Chapman, Duke Street, Portland Place. It is supplied in bottles, and being very effervescent, must be drawn off either through a syphon stopper, or through one of the champagne taps sold by ironmongers. We have recommended it in several cases where milk could not be digested, and have found it grateful to the palate and easily digestible. For those who would like to inquire further into the subject, we must recommend them to get Dr. Jagielski's pamphlet entitled *Koumiss, and its use in Medicine*.

On the Scope of Lilium Tigrinum.

THE following *résumé* of this remedy is taken from an article by Dr. Carroll Dunham, published in the *North American Journal of Homœopathy*, p. 159, current volume. We commend *Lilium* to the careful attention of the profession, as being a remedy which promises to be of great value in the treatment of a class of diseases of great frequency. The symptoms which will make the differential diagnosis between it and similar remedies are peculiar and marked. Other records of the provings may be found in the *Trans. Am. Inst.* '67, '68, *Hahn. Month.*, vol. 5, 147. A list of "apparent characteristics" is given in the *Annual Record*, p. 38. Judging from some experience, it is best not to push the remedy very far, or severe aggravations will follow; in one case at least, needless suffering was caused by too frequent repetition of the dose. If farther experience with *Lilium* confirms the provings, or develops any new facts concerning it, such matter should be sent to Dr. Wm. E. Payne, Bath, Me., the discoverer, who will, no doubt, incorporate it with the other material collected, and give it to the profession.

"When taken in moderate doses the effects are not immediate. Days elapse before unmistakable symptoms of the drug-action appear. But the effects are very persistent, as the record of every prover shows. They tend, moreover, to recur at longer or shorter intervals, and in groups which preserve a definite order. Thus, prover No. 1 reports a third recurrence of a group of symptoms nearly two months after the dose of *Lilium*. In male provers the same recurrence of symptoms in definite groups has been observed, with an interval of comparative freedom from symptoms. The simultaneous observation of these peculiarities in provers residing far from each other, and not known to each other, precludes any doubt of its genuineness.

"Of the symptoms observed by women, as well as men, the effects on the mind are noteworthy, and are of two varieties. First, as noticed by Dr. Payne, anxiety and apprehension that an incurable disease exists or is impending, and this produces despondency. Second, as exhibited most decidedly in prover No. 1, and clearly, though less pronounced, in several others, a consciousness of an unnatural state of mind and feeling, which at last develops into an exalted condition in which the prover is dis-

posed to find fault with persons and things, to exaggerate her own importance and excellence, and look down upon others; conjoined with this is an exaltation of the sexual instinct. In several provers this state of things has resulted in hysterical paroxysms. In prover No. 1 it assumed such marked proportions that I was constrained to put an end to it by administering *Platina*, the indications for which are evident from the mental symptoms. Intellectual activity is impaired in both men and women. Both have complained of the feeling of hurry and restlessness, which is so well described by prover No. 1.

“Menstruation is accelerated, in some cases recurring in two weeks. The flow is very scanty.

“An acrid, thin, brownish leucorrhœa was, to several provers, a troublesome symptom.

“But the most striking symptoms, and those most widely observed, relate to the pelvic organs. They did not generally present themselves until a number of days after the proving was begun. They consist of a dragging, or pulling or forcing-down sensation in the pelvis, as though the entire contents of the pelvis were pulled down through the vagina, or would issue from the vulva. This sensation is not confined to the back or hips—nor again to the hypogastric region—but is described as pervading the entire pelvis. And the two provers on whom this symptom was most marked, describe the dragging as coming even from the *thorax*, the mammary region, and the shoulders. So marked is the sensation of downward and outward pressure that the provers place the hand on the hypogast. or vulva, as though to prevent protrusion. In three provers, physical inspection revealed the existence of anteversio uteri, a trouble which none of them had ever before experienced.

“In this train of symptoms belong also the tenesmus of bladder and rectum, and the diarrhœa and frequent micturition.

“There is agreement of the provers respecting pains, burning or cutting, and tenderness in the region of the ovaries, especially the right ovary.

“The symptoms generally are worse in the afternoon and before midnight; except the diarrhœa, which seems to be a morning diarrhœa.

“If now, with the light which these provings afford us, we seek to place *Lil. tig.* in its appropriate niche in our *Materia Medica*, and to estimate its value by comparison with other drugs, we observe

first : The uniform occurrence, in so many provers, of pelvic symptoms, as well as the demonstration by physical examination, of a uterine displacement, establish its *a priori* claim to rank among the remedies for prolapsus and displacement of the uterus, for catarrh of the vagina and uterus, for inflammation of the ovary. And if we run a parallel with the symptoms of other remedies, we find marked peculiarities which characterise *Lilium*.

“ In the morning diarrhoea, coming suddenly with tenesmus, it resembles *Podophyllum* ; and *Podophyllum* has, likewise, a general bearing down in the pelvis, confined, however, to the lumbo-sacral region, while the mental and moral symptoms produced by *Podophyllum* have no resemblance to those of *Lilium*. Moreover, in so far as my observation goes, *Pod.* both produces and removes these pelvic symptoms only when they occur in connection with certain symptoms of the digestive tract, such as *Lilium* has no relation with.

“ *Sepia* produces certainly a bearing-down sensation upon the lumbar region, together with dragging and even sharp pains from the region of the ovaries extending downwards to the pudenda ; but besides that *Sepia* presents us no symptoms of diarrhoea and irritation of rectum and anus, and no such leucorrhoea as *Lilium* ; the *conditions* are very different. The *Lilium* pains are aggravated in the afternoon and before midnight. They grow worse during repose and when one's mind is passive ; worse, therefore, on lying down and trying to compose ourself to sleep. Whereas, on the other hand, the *Sepia* pains are worse from 9 a.m. to noon, and are relieved by repose ; being aggravated by motion and occupation. The state of mind produced by the two drugs is very different. Almost the same differences exist between *Lilium* and *Pulsatilla*.

“ *Belladonna* resembles *Lil.* in the bearing-down sensation, both in the back and in the pubic region, and in the fact that there is not immediate relief from repose. But, on the other hand, *Lilium* gives no evidence of that general affection of the organism, especially of the circulation, which accompanies every well-pronounced group of *Bell.* symptoms. On the contrary, under *Lil.*, when the patient suffered most, nutrition and appetite were not impaired. They were even improved.

“ It is probable that further provings of *Helonias dioica* will show a strong analogy between it and *Lil.* as regards their action on the

female organism. We know enough already to recognise a difference in the mental symptoms. *Lil.* dulls the intellect, produces a sensation of *hurry* with *inability*, and a distress based on a clearly defined apprehension of having some fatal or serious malady. *Helonias* produces profound melancholy, deep undefined depression, with sensation of soreness and weight in the womb, a 'consciousness of a womb.'

"*Platina* seems to me to present the strongest features of resemblance to *Lilium*, both in the pelvic symptoms and in at least one phase of mental symptoms, and the result of my trial with prover No. 1 shows its power to antidote *Lilium*. But *Platina* does not present any of the symptoms of the intestinal tract which are so prominent under *Lil.*, nor are its effects on the function of menstruation similar."

As Dr. D., for particular reasons, did not add the analysis of the heart symptoms, we append some of the most prominent. They are of two varieties, first: dull, heavy, pressive pain in the region of the heart, with fluttering, throbbing, or palpitation. The aggravations of these symptoms are in the afternoon, and *by lying down at night*, especially on the left side, stooping or leaning forward. Second: "Pain in left side, as if the heart were violently grasped and released, interrupting pulsation and breathing relieved by rubbing and pressure." "A pain through the heart to the back, and a feeling as if the heart were squeezed in a vice. She cannot walk straight by going into a warm room, the symptoms having occurred while walking in cold air." These symptoms bring to mind *Cactus*. They may affect either males or females.—*Med. Investigator*.

A Renegade.

A MEDICAL school, like a church, to be complete, ought to have its adherents, its martyrs, and its renegades. The homœopathic school in England has hitherto only been able to show its adherents and its martyrs, and we have long envied our transatlantic brethren their possession of a renegade in the person of Dr. Peters, so well known for his eminent services to our cause before his lamentable defection. Though we cannot point to such an illustrious example—or rather warning—in this country, we have, it seems, our little renegade in the person of Dr. Charles Phillips. Unlike Peters, who did much for homœopathy, our Phillips has done nothing—

that we are aware of—for homœopathy, but homœopathy has done everything for him, and given him all the little notoriety he possesses. He now, however, spurns the ladder that enabled him to mount to practice, and he thus proclaims his defection in the congenial columns of the *Lancet* :—

“SIR,—As I find my professional position in London has been misunderstood by some of my professional brethren, I beg you will allow me to explain the circumstances of it, through the columns of the *Lancet*.

“My name appeared for some years in the Medical Directory of the Homœopathic Practitioners; but I beg to state that I withdrew from that body for three reasons. 1. Because I am not a believer in infinitesimal doses. 2. Because I do not believe that the law of similars is a universal law. 3. Because I felt that had I remained longer amongst that body I should have been crippled in the exercise of my private and independent judgment in the treatment of disease.

“I am, Sir, your obedient servant,

“CHARLES D. F. PHILLIPS, M.D.

“Lancaster Gate, Hyde Park; March 15th, 1871.”

It is to be hoped that homœopathy will survive the loss of Dr. Charles Phillips's “private and independent judgment,” from which it derived no sensible benefit as long as he professed himself an adherent. On the whole it is better that he should sever himself from the fostering tree, than cling to it like a pernicious parasite, destroying while embracing it.

OBITUARY.

DR. J. N. CASANOVA.

A SPANIARD by birth, but a true citizen of the world by choice, Dr. Casanova had made himself familiar with almost every portion of the earth. He was a very original thinker, and had gained much out-of-the-way experience in his travels. Though comparatively an old man when we first knew him, and older than his years, owing to delicate health, he still retained much of the enthusiasm of youth about him, and would talk for

hours on some favourite subject connected with medicine, illustrating his theories by quaint and amusing anecdotes. His turn of mind led him to select out-of-the-way subjects for study, by preference; but he had also great stores of information on more common-place matters, such as climate, his essays on which subject are familiar to the readers of this Journal. He latterly lived retired from practice, at Cliftonville, Brighton, where he died on the 29th January of this year.

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A Medical Controversy. By L. SALZER, M.D. Calcutta, 1870.

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The Electro-Chemical Bath. By F. J. CAPLIN, M.D. London, Trübner, 1871.

Report of the Liverpool Homœopathic Dispensary. Jan., 1871.

Buffalo Express. Jan. 2, 1871.

Australian Homœopathic Progress.

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El Criterio Medico.

La Reforma Medica.

La Homœopatía.

The Calcutta Journal of Medicine.

La Revista Omeopática.

The Food Journal.

The Chemist and Druggist.

Populäre Homöopathische Zeitung, for 1869 and 1870.

THE
BRITISH JOURNAL
OF
HOMŒOPATHY.

HOMŒOPATHIC JOURNALISM IN AMERICA.

WE have long intended giving our readers a sketch of the past history and present condition of homœopathic journalism in the United States of America. Very few British homœopathists take in more than one of the transatlantic periodicals; many never see them at all. Yet they are numerous and active, and contribute no little to the stock of current homœopathic literature. They represent a great body of the practitioners of our school, greater than can be found in all the rest of the world taken together. So that their characters and tendencies cannot but be of importance to our system,—whether as symptoms of what its followers now are, or as determining influences on what they shall yet be. We think, then, that in every way we shall be doing our readers good service if we give them some account of what American journalism has done and is doing, and endeavour to form some estimate of its meaning and value.

So far as we know, the first medical journal instituted to expound and advance homœopathy in the United States was the *Homœopathic Examiner*. Its first number appeared in the year 1840, and it continued in existence, appearing monthly, for five years, with an intermission of two. Its

editors were at first Dr. A. Gerald Hull, subsequently Drs. Gray and Hempel. It contained many useful articles, original and translated, and was not unworthy of its cause. It was published at New York.

Another early and now defunct periodical was the *Philadelphia Journal of Homœopathy*, which lived between the years 1852-6. Its first two volumes are really excellent. They contain the original provings of *Rhus radicans*, *Iris versicolor*, and *Rumex crispus*, and an abundance of interesting and instructive matter. Dr. Gardiner of Philadelphia was the editor; and among the names of his *collaborateurs* we notice those of Joslin, Okie, Preston, Kitchen, and Payne, besides others of more recently gained distinction. With the third volume, however, the life of the journal began to ebb, and the fourth was its last. Dr. Geary is in this the only associate of the editor, and the quality of the contributions grows more and more inferior to the close.

The year 1851, however, saw the birth of a journal which for a long time occupied the first place among American periodicals, and which survives to this day. We speak of the *North American Journal of Homœopathy*. It was then, and has been ever since, published at New York, and issued every quarter. It has passed through many vicissitudes of management. Its original editors were Drs. Hering, Marcy, and Metcalf: and the homœopathy then advocated was rather of the stricter sort. The illness, resulting in death, of Dr. Metcalf—who seems to have been an accomplished physician—led to the suspension of the journal after three years of useful existence. But in 1856 the break in its continuity was repaired, and the fourth volume appeared under the editorship of Drs. Marcy, Holcombe, Preston, and Peters. The latter gentleman continued for some years to be the presiding genius of its pages, displaying on the one hand great industry and practical acumen, but on the other a most perverse inclination to merge homœopathy into traditional medicine, and to mix up its practice with every novelty of the chemical and other allopathic schools. He ended his meteoric career by deserting our ranks altogether,

and has only been heard of since by occasional appearances in the journals of the old school, where with the same breath he vilifies the homœopathic system and communicates its valuable pieces of practice. At his departure a multitude of the prominent homœopaths of the States rallied round Dr. Marcy to carry on the journal, and the editorial list at one time numbered quite a score of names, among which we find those of Hale, Ludlam, Holcombe, Franklin, Helmuth, and Neidhard. But by degrees the energies of these workers became diverted into other channels, and the *North American Journal of Homœopathy*, now in its 19th volume, is conducted only by Drs. Lilienthal and Hunt. It continues to represent, as since its revival in 1856 it has always represented, the catholic and progressive school of homœopathy. It has varied much from time to time in point of quality, but has always rallied from its temporary depressions, and is still of much service to the practitioners of our system in the New England States.

In this last phrase, we have indicated one chief cause of its subsidence from its *quondam* pride of place. The group of States of which New York is the centre were once America, but this they are no longer. The rivalry of the South they have proved that they need not fear, but their real supplanter is the West. The tide of life and energy has long been setting in that direction: and however New York may retain its commercial pre-eminence, Chicago bids fair to supersede it as an intellectual metropolis and focus of true American (as distinct from American-Irish) nationality. Hence nearly all the newer homœopathic serials of which we shall have to speak "hail" from the Western States; are published at places like Detroit, Chicago, and S. Louis; and are supported by Western contributors and Western subscribers.

But before we reach these, we must say a few words upon the issues of the Philadelphia school, which form a group by themselves. The veteran Constantine Hering, having settled in this well-known city, has naturally gathered around him a company of fellow-workers like-minded with himself. He and they for some time contented themselves

with contributing to the congenial pages of the *American Homœopathic Review*. This excellent monthly journal first appeared in 1861. It was published at New York, and was edited after the first few numbers by Drs. P. P. Wells, Carroll Dunham, and H. M. Smith. While representing the strictest Hahnemannian school, it did this with courtesy and without bigotry; and the distinguished abilities of the two first-named gentlemen filled its pages with valuable matter. A running appendix, moreover, gave us pathogenesies of *Lachnanthes*, *Aloes*, *Tellurium*, and (in part) *Allium cepa*, chiefly arranged by Dr. Hering.

But in 1865, the members of this section of our practitioners in Philadelphia commenced the publication of the *Hahnemannian Monthly* as the advocate of their way of thinking. Of this journal Dr. Lippe has been the chief conductor: and here as elsewhere has displayed more antagonism and exclusiveness than we can commend. Although some new provings and other contributions of worth have occasionally appeared in the *Hahnemannian Monthly*, yet we think it very inferior to its predecessor: and regret that it was thought necessary to discontinue the latter in its favour.

A contribution from Philadelphia of less questionable value we have lately noticed in our Reviews.* It is the *American Journal of Homœopathic Materia Medica*. Dr. Hering himself carries on this monthly publication (Dr. H. N. Martin no longer, as at first, assisting him), and uses it chiefly as a medium for the issue of his long-maturing collection of pathogenesies. It should be subscribed for by every British practitioner, as these alone are worth the cost. Besides the drugs mentioned in our notice, it has now given us *Stramonium*, *Spongia*, *Carburetum sulphuris*, *Erythroxyton coca*, and *Nux moschata*.

The year 1863-4 saw the birth of three Western homœopathic journals.

The *Medical Investigator*, published at Chicago, for some time appeared in square newspaper form, without cover: but always gave something worth reading. It is now a

* *Brit. Journ. of Hom.*, Vol. XXVII, p. 328.

small octavo magazine of 48 pages monthly, resembling our own *Monthly Homœopathic Review*. It is edited by Dr. T. C. Duncan: and among the names of his assistants we notice those of Drs. W. Williamson (just, alas! deceased), Bushrod James, Comstock, Lilienthal, and Shipman. We cannot do better than let this journal speak for itself, by cutting some noticeable extracts from its recent numbers.

Characteristics of Iris Versicolor. By Dr. W. WILLIAMSON.

Increased acquaintance with the scope and range of action of the *Iris versicolor* indicates that it will be one of the most useful remedies of the materia medica.

Its Headache.—It is applicable to a form of sick headache which is characterised by dull throbbing or hammering and also shooting or acute boring pains in one side, with nausea, coming on with *repeated attacks through the day*, or appearing at intervals of many days; most severe in the afternoon and towards evening; aggravated by violent motion, cold air, and coughing, but relieved by moderate exercise in the open air.

The Throat Symptoms are characterised by a smarting, burning sensation, with a feeling of enlargement, like a burning cavern, while the throat is dry, injected, and has a bright red colour. Spasms of the pharynx while swallowing food. Salivation.

Stomach Symptoms.—Excessive nausea and retching, with *eructation of much tasteless gas*, and *burning in the region of the fauces*; vomiting and diarrhoea.

The Abdominal Symptoms are very prominent—severe rumbling of gas, excessive watery discharges, preceded by soft and more substantial evacuations, and intense aching cramp-like pains; excessive nausea and also vomiting—all of which point to cholera-like affections; while the bloody mucous discharges, with severe tenesmus and rectal prolapsus, indicate its usefulness in dysenteric affections. It appears to act on the small intestines before it does on the large, as the watery diarrhoea and colic pains precede the bloody mucous discharges.

In the Chest Symptoms we find short dry cough, excessive tickling in the larynx, preceded or accompanied by dry, smarting, or burning sore throat.

The Pains seem to pass from right to left; or from the right upper to the left lower portions.

The *Iris* appears in its action to have a partiality for the nerves and the joints of the body, especially those of the extremities, without much affection for the muscular system; strongly indicating its usefulness in neuralgic affections, and some forms of rheumatism, especially of the uterus, and in the lumbar and sciatic regions.

Pulsatilla and Mal-presentation.

Dr. W.-F. Dodge, of Earlville, Ill., detailed to us the other day the following case that may be of interest:

Mrs. —, bilious, lymphatic temperament; melancholy—a *Nux* patient—full broad head, but flat on the top. Had four children. Always had trouble; in labour two or three days; at one time three doctors were in attendance. Had lost two children with hydrocephalus.

Dr. D. found her sitting in a chair. She told him the child was "not right." "That it was across." "The head is here," said she, putting her hand on her left side. He found the os not dilated, and that there was without doubt a mal-presentation. He gave her a powder of *Pulsatilla* 2nd, and concluded to wait the results, anticipating a tedious time. In about half an hour she gave a scream, drew a long breath, and exclaimed, "the child has turned!" He put her to bed, and in *four hours*, to the astonishment of all, she was delivered of a living healthy child.

He was induced to give *Puls.* from reading Dr. Woodward's case of "*Puls.* in mal-presentation," published in vol. vi, p. 139, of *Medical Investigator*, knowing Dr. W. to be a careful observer and reliable.

He stated that he now always gives *Puls.* If the pains are false it arrests them, and if true it certainly seems to hasten labour.

Practical Hints. By Dr. CARLETON SMITH.

Vomiting of Milk.—When infants throw up their milk soon after nursing, curdled or otherwise, *Æthusa cynap.* will be found frequently to be the homœopathic remedy. The child may have also discharge from the bowels, of a *light yellow liquid*, with *tenesmus* and *great drowsiness*, confirming more strongly the choice of this drug.

Post-partum Ailments.—Immediately after confinement, a highly attenuated dose of *Arnica* should be administered to the mother. This will soothe the patient, assist in promoting sleep, as well as prevent a host of after troubles.

II. If, some days after confinement, the patient complains of great *soreness* of the *abdominal muscles*, tenderness when *pressed* upon, *worse* when *moving*, *Bryonia* will speedily bring relief.

Should the soreness be more *deeply seated*, and the patient says that *pressure* produces *pain*, "*away down deep*" in the abdomen, and also complains of *sleeplessness* at night, *Bell.* 200 or higher will at once restore the suffering organism.

III. Should the lochia suddenly change its character and become of a *dirty brown* or *chocolate* colour, with foetid odour, and the patient grows very *sad* and *melancholy*, and *fears death*, *Secale corn.* highly attenuated will soon bring about a change for the better.

Ante-partum Nervousness.—If, a few weeks previous to confinement, the woman complains, and shows signs of *excessive nervousness*, with *weeping*, *anxiety*, *sleeplessness*, and pain in the *small* of the *back*, *Cim. rac.* given in water, say the 80th or 200th, will bring speedy relief, and prepare the patient for the great event which awaits her.

Dreads to be Washed.—If a babe suddenly *shrinks* from being *washed*, after having previously enjoyed its daily bath, it needs a highly attenuated dose of *Sulphur*, after which the *dread* of *water*, or of being *washed*, will disappear, and with it other difficulties which he may be suffering with at the time, corresponding to *Sulphur*.

Rheumatism of the Wrist cured by Actœa spicata.

Mrs. M—, 40 years old, lymphatic constitution, suffers habitually from flowing hæmorrhoids and abundant menstruations, had never before real rheumatism, although frequently facial neuralgia, so that she lost all her teeth.

February 2nd.—Found her complaining of intolerable pains in the right wrist, which is swollen and red, especially about the hypothenar eminence. The slightest pressure on that point makes her cry out, and every motion is impossible. She relates that, since three days, she knitted assiduously on a woollen net, with wooden needles, and that it fatigued her greatly, as she is

not used to such labour. Considering it a kind of sprain, she insisted to put compresses wet with *Brasic-solution* on; but the febrile state, the aspect of the wrist and the severity of the pains pointed to acute rheumatism, and I prescribed externally *Bry.*, 12, 6 glob. in a glass of water; a table-spoonful every three hours.

4th.—Had a very bad night; pains so severe that she had to leave, several times, the bed; cannot bear the wet compresses, and had the wrist enveloped in cotton-batting; fever; pulse 120; disgust for food; impatience and restlessness; swelling of wrist and painfulness increased; urine deposits a brick-sediment. Recollecting the action of the *Actæa* in rheumatisms of the small articulations, I ordered *Actæa spic.* 12, 1 drop in water, a table-spoonful every three hours.

5th.—Slept several hours; pulse 90; in better humour, and takes some nourishment; bears a little motion without producing such excruciating pains; urine clear.

6th.—No fever; good night; redness and swelling of wrist greatly diminished. Natural stool after three days' constipation.

7th—10th.—Constant improvement; taking the remedy every four hours. Can move the fingers and the wrist without any pain.—S. L.

Coccyodynia.

We were pleased to see this subject touched upon in a recent number of the *N. E. Medical Gazette*, page 552.

We have cured most of the cases which came under our care with *Phos.* 200, which we consider a leading remedy in this complaint; that is, it is quite frequently indicated, according to our experience. The characteristic symptom leading to the choice of this agent, is "pain in os coccyx as if *ulcerated*." This symptom is sometimes complained of by women after confinement; *Phos.* will then be found invaluable. Besides this remedy, we will also call attention to a few others, which will also be found of use according to their particular indications. *Borax*—itching and pricking in os coccyx. *Bovista*—itching of os coccyx. *Calc. caust.*—lacerating pain in os coccyx. *Carbo an.*—pain in os coccyx, which becomes a burning pain when parts are touched,

bearing down in os coccyx as if bruised; pain as of subcutaneous ulceration, worse when sitting and lying.

Paris quad.—*pulsative stitching* in os coccyx. *Zinc. met.*—*pushing, aching, and pricking* in region of os coccyx.

It will be observed that both *Phos.* and *Calo. caust.* have *ulcerative* pain, but the sufferings of the former are worse on motion or bodily exertion, while with the latter, the pains are somewhat abated by movement.—C. C. S. (*Med. Investigator*).

Surgical Diseases.

In the September issue of the *Medical Investigator* is an article under the above caption, from the pen of J. G. Gilchrist, M.D., in which he urges the importance of perfecting our knowledge of the scope and power of homœopathic remedies in surgical diseases. This is a subject which should command the attention of every member of our school. Let this be done, and homœopathic surgery will soon be, as it is destined to be, *the* surgery of the world. That our resources in this regard are far superior to those of the old school, no well-informed person will attempt to deny. With such potent and valuable agents as *Aconite*, *Arnica*, *Glonoine*, *Hypericum*, &c., for shocks to the nervous system produced by injuries, and our *Calendula* for lacerated wounds, we present a front that the combined armies of allopathy cannot break. The sooner the people are educated to the fact that he is more entitled to the name of surgeon who saves an injured or diseased limb, than he who, for want of better means, resorts at once to the knife, the sooner will homœopathy take her rightful station as the only true medical science. This she will soon do with a few such workers as Dr. Gilchrist. His contributions on this branch to your valuable journal will be eagerly looked for by many, and will no doubt stimulate others to go and do likewise.

Case.—To prove the superiority of our remedies over those of the old school, I will cite a case that came under my observation four years since. My patient was a lady about thirty years of age, dark hair and complexion. Eight or nine years previous she had a fall which sprained the left ankle. Soon after an ulcer appeared, which continued to spread, notwithstanding the exertions of the best advice (allopathic) that she could procure. Nine different physicians had treated her, and the tenth one, who was no other than the late celebrated surgeon, Dr. McDowell, of St.

Louis, was applied to. He advised immediate amputation as the only means of saving her life. This she positively refused to submit to. Some months later, I saw the case. Its appearance did not warrant me in giving much encouragement. The anterior and external surfaces of the limb from the ankle to the middle third of the thigh was one mass of ulceration, and spreading rapidly. It had a spongy appearance, with elevations and depressions, and constant ichorous pus-like discharge. There was constant pain, of a burning, stinging, gnawing character, greatly aggravated by the least touch. So sensitive was the limb, that she would keep her hands between me and the leg, for fear I would touch it. The whole limb felt heavy and weary, and there was a gnawing lacerating pain in the knee-joint. Walking, and even standing, was very painful. The symptoms were all worse at night, when she could not endure the warmth of the bed. She stated that for nearly three years she had been able to sleep but a few hours at night, and then only by having the limb before an open window. Enlarged and sensitive inguinal glands, which were at times quite painful, led me to suspect syphilis. *Merc. sol.* 200 given every night for a time, then about once a week, was soon followed by relief from the pains and nightly aggravation, so that she could sleep all night with the limb covered in the bed. An occasional dose of *Sulphur* 200 was given. In about three months the limb was entirely healed, and, as she expressed it, was "as good as new." At first I thought to hasten matters by using various washes and cerates, but they only served to irritate and increase the suffering. Even a carefully applied bandage from ankle to hip could not be worn. A few months after dismissal she was thrown down while in a crowd and trampled on. She was carried home insensible, and though badly bruised, the old trouble did not reappear.

I mention this case to show what those who make no pretensions to surgery can do by carefully affiliating the remedy. At first sight it seemed an utter impossibility to save this woman's leg. Now I am almost ready to affirm that nothing is impossible with homœopathy.—E. A. BALLARD.

The frequent use of the 200th potency, so rarely heard of in this country, by our transatlantic colleagues, is worthy of our notice. It is a fact which will appear from whatever journal we cite. The strong advocacy of the use of these

high dilutions by such men as Drs. Carroll Dunham and P. P. Wells has probably done much to popularise them: and the former physician has contributed to the confidence with which they are prescribed by preparing a number of 200ths with his own hands.

The *Western Homœopathic Observer* comes from as far west as S. Louis. It has been conducted from the first by Dr. William Tod Helmuth, one of the number of those *surgeons* proper who have formed so striking a phenomena of recent American homœopathy. Drs. Beebe, Franklin, and Bushrod James are others of the same character: and are doing much to win respect for homœopaths, if not for homœopathy, among their surrounding brethren. Dr. Helmuth's journal has naturally made surgical matters a predominant element of its contents. The removal of its editor to New York, to assume the professorship of surgery in the Homœopathic Medical College there, brings the *Western Homœopathic Observer* to a close with the end of its seventh volume.

The *American Observer* is edited and published by "Dr." E. A. Lodge, chemist and *medicus* in one, at Detroit. His staff, besides names common also to the *Chicago Investigator*, includes the well-known Dr. E. M. Hale. From his hand, and from that of the "Clinical Editor," Dr. W. S. Searle, we find several articles of value in the last few numbers of the journal: and these we proceed to give to our readers.

Podophyllin.

Dr. C. E. Sanford, of Bridgeport, Conn., communicates to me a characteristic symptom of *Podophyllin*. A lady patient observed that her usual heavy, tenacious, muco-purulent leucorrhœa was always changed to "*a reddish discharge, a dirty fluid like the rusty sputa in pneumonia.*" If this is verified it will place *Podophyllin* as an analogue of *Nitric acid*. This pathogenetic symptom resulted from the 3rd trit. as readily as half-grain pills of the crude. An eclectic physician complains in *Scudder's Journal* that a large dose of *Podophyllin* caused in one of his patients the following:—"Aggravation of internal piles, the rectum would protrude more than an inch after every motion of the bowels or after any sudden motion such as sneezing, and even during any

mental excitement,—the parts could not always be replaced very readily, would sometimes remain prolapsed for days, owing to the swelling and congestion."

Apis Mel. in Ovarian Tumour.

Dr. P. H. Hale, of this city, has just made one of the finest cures on record. A lady had an ovarian tumour of the size of a child's head at birth. It was so diagnosed by Prof. Byford, of this city, who wished to remove it, and a time was appointed for several months. Dr. Hale was applied to, and prescribed *Apis*, but in a peculiar manner. Ten or twelve living bees were thrown into a teacup of hot water. Of this infusion a table-spoonful was taken every four hours. In a week a perceptible decrease was observed, and before the time for the operation had come, the tumour had nearly disappeared. This method of administering *Apis* has many advantages. It is well known that few physicians have much confidence in the tincture, but prefer the trituration of the living bee. According to Dr. Marcy, the infusion of the bee was the original method of administration for dropsy. Certain it is that I have succeeded with *Apis* in this form when both the tincture and trituration failed.

A few pharmacological observations on *Apis* may be appropriate here. The infusion is not always to be had; living bees are not obtainable at all times. Some preparation must be established. I advise two methods—(1) A trituration with fine sugar of milk, of the whole living bee, or the lower third of the abdomen cut from the living bee. Ten bees to ninety grains of sugar for the first trituration. (2) The living bee in hot water for the mother-tincture. Ten bees to 9 drachms of hot distilled water. The first three dilutions to be made with distilled water, cold after the first. One tenth alcohol may be added to the dilutions to keep them. After the third, pure alcohol may be used. I believe the active principle is very volatile, and that all the preparations should be closed hermetically. I also believe that alcohol injures the crude poison, as it destroys nearly all animal poisons.

The Importance of a Single Symptom.

The physician occasionally meets in practice with cases in which the tongue is exfoliated in one or more places, where it is

sore and raw. Some time last spring I was consulted by a lady who was suffering from diffuse inflammation of the mucous membrane of the mouth and tongue, in which the peculiar condition of the tongue above mentioned presented itself. I treated her a number of days with the ordinary remedies, without any favorable result. Being puzzled, I referred to a repertory to find the peculiarity of the tongue mentioned. This led me to select *Ranunculus scleratus* ²⁰, which effected almost a complete cure in twenty-four hours. I have since prescribed, a number of times, the same medicine, in cases where the peculiar appearance of the tongue presented itself, with the same favorable result in all cases.

Quite recently a case of diphtheria, which I was called to treat, had this symptom of the tongue well marked. The tongue was thickly coated with a yellowish-white fur. On both sides of the median line there existed raw and denuded surfaces—little islands, as it were, surrounded by the thick coating of the tongue. Both tonsils were swollen and covered with diphtheritic patches. Prescribed *Ranunculus scel.* ²⁰ every two hours. Next day a new epithelial covering seemed to have formed over the raw surfaces; the throat affection, however, not improved; alternated *Lachesis* with *Ranunculus scel.* and a rapid cure of the diphtheria followed. It is worthy of mention that the left tonsil was first affected.—S. R. BITTENHOUSE, M.D.

Iris in Salivation.

The patient is a lady, æt. about 30, nervo-sanguine. The disease was *phlegmasia alba dolens* succeeding an abortion. Had been treated after the allopathic fashion by three physicians in succession. I treated the case, which presented the usual symptoms, with *Acon.*, *Bell.*, *Puls.*, *Ham.*, *Rhus*, and *Ars.*, in the low attenuations.

Of these *Arsenicum* did the most good, checking the severe irregular chills, burning fever, and other well-known symptoms of this medicine. The swelling also abated; the usual severe *hepatic* complication, with dark offensive stools, was considerably relieved by *Podophyllin* and *Leptandrin* ². But frequent relapses ensued in spite of all these remedies, and one symptom, salivation, first of tough mucus hawked from the throat, which presented a dark appearance, and afterwards a profuse watery discharge from the whole buccal cavity, existed from the first and was altogether

unchecked. There was no pain, or soreness, or fever, only a clammy greasy sensation in the mouth with bilious coating on root of tongue. There was also lethargy, hunger, with inability to eat, a general "bilious" condition, and constipation succeeding a profuse watery offensive (pancreatic?) diarrhoea.

There was slow articulation, the patient being unable to describe her symptoms, which were few, particularly the subjective. The salivation became more copious; I tried *Mercurius*, then *Dulcamara* vainly. Then I read in Burt's book, *Iris* v., "Salivation with profuse flow of saliva, gums and tongue feel as though covered with a greasy substance.—Lippe." I gave *Iris* ² alone in water, then in pills three times, then once per day. The salivation immediately diminished, was soon cured, and as it departed the relish for food came; patient more cheerful; talkative; improvement in all particulars till completely cured. The inevitable old lady with the long nose said she never would get well. There were the usual differences of opinion and excitement in the neighbourhood. The case is a triumph for homœopathy, *Iris versicolor*, and yours truly, G. STEVENSON.

Coccyodynia. By W. S. SKARLE, M.D., Brooklyn, N. Y.

Among the minor disorders, of female humanity especially, perhaps none is more harassing than this. Patients frequently appear who complain that while attempting to sit down, rising to stand, making any effort, particularly to defecate, they experience severe pain in the region of the coccyx. Often this is so severe as almost to prohibit exertion. At this moment I have a case of spinal hyperæsthesia in a young lady who has been nearly bed-ridden for two years, and whose chief complaint is of pain in the coccygeal region which precludes motion in walking, and yet keeps her restless in bed. Generally the most pain is felt in attempting to rise from a low seat, and to accomplish this the sufferer must aid herself by the hands placed upon chairs by her side, or be aided by others.

This condition of affairs often lasts for years, but is generally fugitive and occasional.

It is possible to confound it with only two other complaints—hæmorrhoids and fissure of the anus, and a careful examination will prevent mistake.

This affection was first described by Profs. Simpson and Scan-

zoni in 1861, and more lately by Thomas, in his work on *Diseases of Females*.

Its seat is doubtless in the fibrous tissues surrounding the coccyx, and as this bone serves as a point of attachment for several of the ligaments and muscles which are concerned in the movements above referred to, pain is experienced when motion is attempted.

From the character of the pain, its seat, the course of the disease, &c., we infer that the affection is neuralgic or rheumatico-neuralgic in its nature.

Its causes are thus enumerated: parturition; delivery by forceps; falls or blows upon the coccyx; cold; exercise on horse-back.

Treatment.—The physiological school advise that the part should be blistered and morphia applied. Should this not effect a cure, Simpson proposes that the attachments of the ligaments and muscles to the coccyx should be subcutaneously severed by the knife; and if this fail, "as it may do," that the bone should be amputated.

Most lame and impotent conclusion for a lame and impotent disease! Methinks there are few but would rather submit their case to the higher "Divinity that shapes our ends," than be *curtailed* by a Simpson or a Thomas.

But does homœopathy afford hope or certainty of better results from milder means?

There is nothing in our clinical records, nor in our works on practice, so far as I can discover, regarding the treatment of this affection. And it is in the hope that cases of cure may be elicited from the profession, that I have written this sketch as well as dug up from our materia medica the means which are likely to afford help from in troublesome complaint.

We will first mention those remedies which manifest the most marked action upon the coccygeal region, and afterwards take up those in which the relation is less plainly seen.

Belladonna.—The ischia feel sore, as if there were no flesh on them; yet he feels better when sitting on something hard than on cushions. Intense crampy pain in the small of the back and the os coccygis. He can sit only a short time. Sitting makes him stiff and unable to rise again from pain. Cannot lie down well; wakes often at night, and has to shift his position; unable

to lie at all upon the back, and is most relieved by standing or walking slowly.

Causticum.—Dull, drawing pain in the region of the coccyx. Darting pain in the coccyx. Pain as from bruises in the coccyx. Every movement of the body gives a pain in the small of the back. Pinching, crampy pain in the lumbar region and buttocks.

Carbo animalis.—Pain in the coccyx, which becomes a burning pain when the parts are touched. Pressing, bearing-down pain in the coccyx, as if the part were bruised. Pain as from sub-cutaneous ulceration in this region, mostly when sitting or lying down. Pressing, drawing, or stiffness in the lumbar region, as if the back were broken.

• *Thuja*.—Painful drawing in the sacrum and coccyx, and in the thighs when sitting. After having been seated awhile, the drawing hinders standing erect. Sudden cramp-like pain in the lumbar region after long standing, and then attempting to walk. It seems as if he would fall.

These four remedies seem to affect this region more powerfully than any others. Of less characteristic remedies, we find the following:

Cannabis sat.—Pressure, as if with a sharp point on the coccyx. Pain in the middle of the back, as if it were being pinched, the pain gradually extending towards the abdomen.

Cantharis.—Lancinations and tearings in the coccyx, causing him to start.

Oicuta.—Tearing, jerking, in the coccyx.

Cistus canadensis.—A burning, bruised pain in the coccyx.

Drosera.—Itching stitch in the coccyx when sitting.

Graphites.—Dull drawing in the coccyx in the evening. Violent itching of the coccygeal region, the part being moist with scurfy formations.

Gummi gutti.—Repeated gnawing in the coccyx.

Kali c.—Violent gnawing in the coccyx, both when at rest and in motion.

This remedy has many back symptoms, and may be frequently indicated, but the above is the only symptom recorded including the coccyx.

Kali hydriod.—Pain in the coccyx, as from a fall.

Kreasote.—Drawing pains along the coccyx down to the rectum and vagina, where a spasmodic, contractive pain is felt. Better when rising from her seat. Subsequent milky leucorrhœa.

Lachesis.—Continual pain in the sacrum and coccyx. Drawing pain, or as if sprained, in the small of the back, hindering motion.

Lactuca.—Pain, as if in the spinal marrow extending through the coccyx.

Magnesia.—Sudden, piercing pain in the coccyx; sudden, violent, concussive, tearing, stitching pain in this region as if the spine were bent back.

Mercurius.—Tearing pain in the coccyx relieved by pressing the hand against the abdomen. Pain in the sacrum, as if one had been lying on too hard a couch. Pricking, itching in the sacrum when walking.

Muriatic ac.—Drawing burning along the back, beginning at the coccyx, as if under the skin. Burning stitch in the sacrum, causing one to start.

Paris quad.—Tearing in the coccyx when sitting. Pulsative stitches in the coccyx.

Petroleum.—Pain in coccyx while sitting. Great uneasiness and stiffness in the small of the back and coccyx in the evening.

Phosphorus.—Pain in the coccyx as if ulcerated, hindering motion, and followed by painful stiffness in the nape of the neck.

Phos. ac.—Itching stitch in the coccyx. Fine stitches in the coccyx and sternum.

Platina.—Numb feeling in the coccyx as from a blow.

Ruta.—Pain extending from the coccyx to the sacrum, as if caused by a bruise.

Valeriana.—Bubbling pressure above the anus in the region of the coccyx.

Zincum.—Pain in the coccyx, sometimes a pushing-aching and sometimes pinching. Lancination in the sacrum, pressure, tension and weakness in the lumbar and sacral region, cracking in the back when walking.

Headaches (from the standpoint of a subjective symptom).

By W. S. SEARLE, M.D.

In the treatment of disease the homœopath is not uncommonly led to the curative remedy by subjective symptoms solely, and perhaps this happens more often in prescribing for the protean malady we have mentioned than for any other. In no disease do

we derive less help from pathology and its nosological subdivisions; in none are we so utterly dependent upon phenomena—their relations and modalities. Especially is this true of chronic headache, and precisely because we possess a clue to lead us through the labyrinth of symptoms which cluster so thickly around this noblest organ of our frame, are we more successful than our colleagues of the physiological school in our treatment of disorders of the brain.

It has, of late, been our good or ill fortune to treat several cases in which one of the most prominent of these subjective phenomena was "*a sensation of coldness about the head.*" And we write down the results of our study of the *Materia Medica* from the stand-point of this symptom, not because they afford anything new or original, but simply that they may save some busy practitioner the labour of a similar compilation for himself.

True, our Repertories, especially the large one accompanying the *Symptomen Codex*, are somewhat full upon this head, but they do not exhaust the list of remedies possessing this symptom, and moreover this large repertory is in the hands of comparatively few of our practitioners.

We do not aim at giving all the varieties of headache curable by any remedy, and through ignorance or inadvertence, we may omit many symptoms which are related to, or properly associated with, that already named, but we shall at least indicate to the worker where to dig in the mine that he may find the elixir of life for many a weary invalid.

Agaricus.—Icy coldness in the scalp after itching and scratching (*Arsen.* reverse). Pains as though sharp ice touched the head or cold needles run through it. (*Arsenic* has hot needles.)

Sensation as of a nail thrust into the right side of the head.

Drawing pain in the morning extending from the forehead to the root of the nose with epistaxis or great discharge of thick nasal mucus, followed by dropping of water from the nose.

In the headaches of those suffering from spinal irritation, nervous twitchings, and great uneasiness and weakness in the spine.

Arnica.—Sensation on the forehead as if touched with a cold finger end.

Cutting pain through the head, as from a knife, followed by a sensation of coldness.

Coldness changing to heat or coexisting with it.

The pains in general are aching and darting, mostly in the forehead, and are aggravated by shocks, motions, &c.

Arsenicum.—Paroxysms of excessively painful hemicrania with great weakness and icy cold feeling in the scalp, followed by itching. (*Agaricus* reverse.)

Tightness, heaviness, pressure, confusion, dulness, and loss of memory. Anguish, restlessness, and fear of death.

The pains are worse after eating, better at first by cold applications, but worse on their being removed.

The patient must move his head to and fro, toss his feet and hands about, is bloated, chilly, and relieved by the warmth of a fire.

Asarum.—Cold feeling at a small spot on the left side of the head a few inches above the ear. Pain as from contraction in the forehead, temples, and behind the ears, with watering and burning of the eyes, worse about 5 p.m.

Belladonna is one of the two main remedies that produce a sensation of cold in the brain. *Calcarea* is its congener here, and *Phosphorus* has a similar condition, less plainly marked.

In *Belladonna* this sensation is located in the centre of the forehead or temples; while under *Calcarea* it is more generally diffused; under *Phosphorus* the location is in the occiput or left side of the head. The collateral symptoms for *Bell.* are too well known to need repetition here.

Berberis.—Coldness in the temporal region. (*Bell.*)

Baryta c.—Right side of head feels icy cold to the hand, but burning to himself.

Calcarea c.—Icy coldness in and about the brain. Internal and external sensation of coldness of various parts of the head as if a piece of ice were lying against it, with pale puffed face. (*Verat.*)

There are fulness, heaviness or throbbing sensations, worse from mental exertion, stooping or walking in the open air. Better from closing the eyes and lying down. (*Verat.* reverse.)

Sweat on the back of the head and neck in the evening. Menses too soon, too profuse, and too long.

Principally of use in the chronic headaches of torpid, scrofulous constitutions, with pale face, rather fair complexion, and a disposition to corpulence.

Cannabis sat.—Cold sensation at a small place on the parietal

bone, and afterward, on other places, as if a drop of cold water had fallen upon it. (*Orocus*.) Heavy weight on the vertex. Great fatigue after slight exertion, sleepiness during the day, and sleeplessness at night from heat. He feels as if hot water were poured over him.

Chelidonium.—Sensation of cold about the occiput rising from the neck. (*Dulc*.) Weight and pressure in this region. The occiput seems fastened to the pillow, and the head must be lifted with the hand. Drawing pains from vertex down the neck.

Conium.—Sense of numbness and coldness on one side of the head.

Orocus.—Sudden cold sensation on the left parietal bone, as if a drop of cold water had fallen on it. (*Cannabis*.)

Acute tearing pain in the head and right eye, with dimness of vision, and a sensation as if cold air were rushing through the eye. When moving the head, sensation as if the brain were tottering to and fro. Great and alternate nervous exaltation and depression.

Dulcamara.—Chilliness in the cerebellum and over the back, returning every evening. (*Chelidon*.) Sensation of enlargement of the cerebellum and whole head. Worse in cold, damp weather, until midnight, better when lying down.

Gratiola.—Frequent feeling of coldness on the vertex, painful, and changing to a feeling of warmth when moving the head. In headaches attended with a peculiar biting burning in the face and other parts, langour in arms and legs, nausea, disgust for food, better in the open air. Peculiar coldness in and on the head, and in the stomach and abdomen.

Gummi gutti.—Sensation of great coldness in left temple, as if occasioned by a wet, cold cloth,—in the afternoon.

Hypericum.—Sensation in the forehead as if touched by an icy cold hand—in the afternoon—after which a spasmodic contraction is felt in the right eye. Curling sensation on the vertex. Confused sensation in the vertex with buzzing sensation at night, as if something living were in the brain.

Kali hydriod.—Pain in the vertex as if it would be dashed to pieces, with chilliness of that part of the head, although the scalp feels hot. The chilliness is relieved by external warmth. (The foregoing symptoms recurred for many days.) The scalp feels as if ulcerated when scratching it.

Laurocerasus.—Sensation of icy coldness on the vertex, as from cold wind, then in the forehead and nape of the neck extending to the small of the back, after which all the pains in the head disappear. Worse in warm room; better in open air. Stupefying pain in the whole head. Sensation of looseness of the brain, as if it were falling into the forehead, when stooping, without pain.

Lobelia.—Chilliness of left side of the head, with feeling as if the hair would rise on end. Dull, heavy pain passing around the forehead from one temple to the other, just above the eyebrows. Vertigo and deadly nausea.

Manganum.—Cold feeling at a small spot on the vertex.

Moschus.—Aching pain in the head with coldness as from cold poultices. The pains are compressive and stupefying in the forehead or back of the head, and neck. Better in open air, worse in the evening, on motion, and in the warm room.

Natrum mur.—Cold sensation on the vertex, and painful sensitiveness of the scalp with spasm of the eyelids.

Petroleum.—Sensation as of a cold breeze blowing on the head. Head feels numb as if made of wood, or as if bruised.

Phosphorus.—Cold crampy pain on the whole left side of the head; sensation of coldness in the cerebellum with sensation of stiffness in the brain.

Sabadilla.—Heat in the forehead followed by coldness of scalp, even the hairs feel cold to the hand, as if cold water had been poured over the scalp. The Sabadilla headache is more violent when intently reading or reflecting.

Sepia.—Coldness on the vertex, worse from moving the head and stooping, better when at rest, and in the open air. Dulness of head, sick headache with boring pain, forcing one to cry out, and with vomiting. Throbbing, most violent in the occiput, aggravated by warmth, alleviated by sleep. Most commonly indicated in inhibitory paresis of the cerebral vaso-motors from uterine irritation.

Sulphur.—Feeling of coldness about the head, a cold spot on the vertex continually; nightly headaches, with sleeplessness; heaviness in the occiput; piercing pains with buzzing in the ears; throbbing bursting pains in the vertex; better in warm room.

Strontia carb.—Chilliness over scalp and upper back (*Dulc.* and *Chelidon.*), worse at night and in cold air.

Valeriana.—Sensation of icy coldness in vertex when pressing it firmly with the hand.

Veratrum a.—Sensation of warmth and coldness at the same time on the scalp, the hairs being sensitive. Chilly on the top of the head, and at the same time about the feet; sensation as of piece of ice on the head (*Calc.*), cold sweat on the forehead; nausea, vomiting, stiffness of the neck, and profuse micturition; weakness and faintness; pains worse when rising up or lying down.

These, I believe, are all the remedies which in any marked degree produce a sense of coldness about and in the head. It is not a little remarkable that the peculiar developments and concomitants of this symptom are so well defined under most, if not all of them, as to render mistake in proper selection quite unlikely.

Can any of our pathologists read the riddle of this symptom from their standpoint? We confess our inability to do so. Doubtless it is a purely nervous symptom, but that affords us no help in its practical use. We are reduced to the necessity of regarding it as merely a subjective symptom—a valuable one often to the homœopath, but to all others an idle tale. It is seldom associated with an actual decrease in the temperature of the scalp, although, in the case of *Veratrum*, and perhaps other of the remedies mentioned, it may be. Sometimes it coincides with actual hyperæmia. It is connected equally with the congestive headache of *Belladonna*, the anæmic ones of *Arsen.* and *Verat.*, and with the hysteric phenomena of *Moschus* and *Valeriana*. Truly, it is a riddle to the physiological school, but how great a help to the follower of Hahnemann!

Will not some of our readers give us some cases in which this symptom is prominent?

Lilium Tigrinum. By E. M. HALE, M.D.

The honour of introducing this remedy into our *Materia Medica* is due to Dr. W. E. Payne, of Bath, Maine. Owing to his enthusiastic and industrious efforts, several provings of great value have been made. These provings are superior to nearly all our previous provings from the fact that the objective symptoms were noted, and certain pathological conditions recognised beyond controversy.

The sphere of action of *Lilium* appears to involve the *heart*

and the *sexual organs*, and it affects these organs in a profound and peculiar manner. The high character of all the provers, and those who vouch for them, affords the strongest guarantee of their genuineness.

The *tincture of flowers* is the officinal preparation, but the tincture of the seeds may be more potent. The bulbous root is probably not medicinal, being eaten by the natives of China and Japan, where the plant is indigenous.

The following is a collection of the most notable symptoms obtained up to this time, excepting, however, a large number which will appear in the complete *résumé* in the transactions of the American Institute for 1870. (See report of the *Materia Medica*.)

Mental Sphere.—Don't care to be pleased; don't care to talk; desire to sleep; confusion of ideas; pressure and crazy feeling upon the vertex, so that she cannot write her symptoms (vv); she wants somebody to talk to her and entertain her; feels quite nervous; feels hurried and yet incapable, as if she had a great deal to do and cannot do it; much thirst; drinks often, and much at a time; she is conscious of feeling nervous, irritable, and yet says she feels jolly; don't want to complain, and yet don't avoid people; she remarks that her symptoms are all worse when she gives up active resistance to them and control over herself, as for example, when she sits down to rest or tries to go to sleep; discouraged and despondent; averse to being alone, but does not dread it; wits and intuitions dull and languid; depression; desire for fine things of every kind; dissatisfied with what she has, and envious of others; while attending a lecture desire to strike the lecturer, and in the evening a disposition to swear at everybody and everything, and to think and speak of obscene things; languid, dull, and forgetful despondency, with aggravation at night; nervous tremulousness, and inability to apply the mind; wild feeling in the head as though she would go crazy, and no one would take care of her; thoughts of suicide.

Head.—Heat and pain in forehead and brow; severe blinding headache in the anterior part of the head; the peculiarity of which was a sensation as if all the blood were pressing out through every aperture; headache as if the head were too full of blood, as if the blood would issue from nose and ears (vv); pressure from within outwards (vv); darting pains in different parts of the head; grumbling pain in the right side of head and teeth; the

head grows "wild" after she has been quiet a short time; pressure and a crazy feeling upon the vertex, so that she cannot write her symptoms, dull and sharp pains particularly over the eyes; the pains, &c., of the head are heaviness, heat, fulness, dulness, right side mostly affected.

Eyes.—Dimness of sight (v).

Mouth and Throat.—Much thirst; drinks often, and much at a time; nausea.

Stomach.—Eructations soon after taking the medicine; nausea with desire to vomit, but unable to do so (vvv); bubbling sensation in the right hypochondrium; constant desire to vomit, with frequent hawking of mucus from the throat; great distension of the stomach, with frequent eructations and escape of flatus from the anus (vv); (escape of flatus upward and downward was a very constant attendant while under the influence of the drug).

Abdomen and Genital Organs.—At intervals the skin of the abdomen felt stiff and stretched; nausea, with bloated sensation in the abdomen, particularly across the hips and in the region of the *uterus* with darting pains in different parts of the head; some tearing pain in the lower part of the abdomen from the region of the *ovary* down both sides; pain in the right iliac region, better during motion; increased depressing weight over the pubes, worse in evening; the dragging downwards towards pelvis is felt as high as the stomach and even the shoulders; not relieved by lying down, though worse when standing; a disposition to place the hand upon the hypogastrium and press upwards in order to relieve the dragging sensation; wants to cry from a feeling of irritation and of something wrong in the abdomen and pelvis; a sensation in the pelvis as though everything was coming into the world through the *vagina* (vv); a very distressing sensation, not relieved by change of position; also an aching and pressure across the lumbar-sacral region, and some pressure on the rectum; when walking, pain in both *ovaries*, worse in the left, extending down the anterior and inner aspect of the left thigh, as if it would be impossible to take another step; as soon as she extended the limb she must immediately flex it again, and then, because of a restless discomfort, must again extend it; she cannot tell which pelvic pain is the worse, that in back or that in the pubic region; the whole contents of the pelvis seem to drag downwards and forwards and quite from the epigastric region; feels bloated, but is not so; somewhat tender on pressure in

region of the *ovaries*, especially the right ; aching in pelvis between promontory of sacrum and pubes ; it feels to her as if the aching was not in the *uterus*, but around it ; she feels constantly the two spots corresponding to the *ovaries*, and which ache and feel like coals of fire ; in the pelvis a feeling like dragging out, as if the whole contents were pushing down into a funnel, the outlet of which coincided with the *vagina* (vv) ; pain in the right *ovary* as if a knife were inserted into the *ovary* and ripped down the groin and the anterior part of the thigh ; the pain extended over the lumbar-sacral region, and she must cry herself to sleep ; somewhat relieved by pressure on the *ovarian* region ; *menses* occurred at the regular day, and normal, but *only while she keeps moving* ; the flow ceases when she becomes quiet ; *sexual* instincts, formerly dormant, are now quite strong ; wits and intuitions dull and languid ; *leucorrhœa* (a thin acrid discharge, leaving a brown stain ; she never had it before) ; (vv) ; burning pain across the hypogastrium from groin to groin ; *menses* recurred after an interval of two weeks ; a slight, dark, thick, and offensive discharge ; pressing down in the pelvis and burning all around the pubes and *genitals* worse from 3 to 5 p.m. (vv) ; rumbling in the lower part of the bowels, more on the right side (vvv) ; abdomen tender to pressure occasionally.

Rectum and Stool.—Her bowels had been regular, but now she has alternately a solid and loose stool, several during the day, and a constant feeling as though she must have a stool ; this feeling resulting from a sensation as if something were pressing against the anterior wall of the rectum, at the anus, and about one to three inches above it ; for thirty-six hours constant desire for stool from pressure on the rectum—a stool every half hour, lumpy, diarrhœic, with flatus, constant tenesmus, with burning in the urethra ; diarrhœa in the morning ; evacuations dark and hard, followed by heat in the rectum and anus, with slight pains in the abdomen.

Generative Organs.—[See “Abdomen and Genital Organs.”]

Urine.—Frequent desire to urinate ; worse during the day, weak, scanty discharge followed by an acrid sensation in the urethra ; tenesmus, resulting in passage of a little urine only ; urine foetid.

Chest and Heart.—Pain through the heart to the back, and a feeling as if the heart were squeezed in a vice ; short of breath, especially on going upstairs ; heaviness in region of heart, and

palpitation when lying on left side, worse when in bed—at night; heart's action intermittent—every intermission was followed by a violent throb (vv), causing an involuntary catching of the breath, at the same time the blood rushed up through the carotids to the head, producing great heat, and a crowded feeling of the head and face; pain about the heart was dull and pressive; pain in the heart interrupted its pulsations and breathing; constant heavy feeling in left side in region of the heart.

Back.—Dull pain in back and sacrum (vv); constant pain between the hips (vv); cold feeling in the back, as of cold water poured down the back (vv); pain in the back (vv); pain in sacrum (vv); pain in lumbar regions (vv); dull, occasional, shooting pains across small of back (vv); dull heavy pain, and great weakness in small of back and loins.

Extremities.—During the night a feeling in all the extremities as if the blood were pushed outward; restlessness; heat and pain in the forehead and brow (v); pain extending down the anterior and inner aspect of the left thigh; cutting pain in the left mammary gland, with aching beginning below the nipple, deep in the breast as though between the gland and ribs, and extending around that side to the spine, seeming to press under the lower end of scapula, coming on after retiring, and worse when lying on the affected side.

Kaolin. A New and Valuable Remedy for pseudo-membranous Diseases. By W. S. SEARLE, M.D.

This remedy, so far as I can discover, was introduced into the Homœopathic Materia Medica by Dr. Laudsman, of Vienna. Whether he advised its use in croup upon the basis of a proving, our journals have not yet informed us. But some months ago, it was remarked in the *Hahnemannian Monthly*, that Laudsman recommended its use in membranous croup. This information seems to have been received with the usual indifference which justly prevails in our school towards any new comer into our Materia Medica; especially when not backed by a reliable proving. But, as has happened before (quite rarely, it is true), we seem to have here stumbled upon a powerful and much needed remedy. True, its sphere remains quite undefined as yet, and this can only be developed rigidly by a proving, but enough is already known of its value clinically to demand that, if it has not already been proven, it speedily should be.

Membranous croup is a fearful disease. The slow, steady, persistent and malignant manner with which it strangles its victims never loses its horrible character even to the professional eye. And any one who has seen a patient die with this disease—a death worse than a thousand hangings—can appreciate the emotions with which I beheld its development in my only son. The history of the case is as follows :

On December 17, at 10 p.m., H. F. S., æt. four years, awoke from sleep with croupy cough. He was put upon the Boenn- inghausen powders, which had usually afforded him speedy relief. But he continued steadily to grow worse during the night, and in the morning, as the fever increased, and the voice was becoming husky, the same remedies were given in the dilution. They caused, however, no abatement of the symptoms, and he grew rapidly worse until, at 1 p.m. of December 18th, his voice had sunk to a whisper. There was the sawing sound during both ex- and inspiration, with high fever. No membrane could be dis- covered in the fauces, but I have never seen a case take this course, and exhibit these symptoms which did not become membranous.

At 1 p.m., becoming desperate, and being able to discover no in- dications for any other remedy, I put him upon *Kaolin* $\frac{1}{10}$ in tri- turation every half hour. No improvement was noted until 3 p.m., when he fell asleep, broke out into perspiration, and at 4 p.m. awoke and spoke in his natural voice. The fever, cough, and all other symptoms rapidly subsided ; he coughed hardly at all in the night, and was convalescent on December 19th.

Case 2.—On December 21st I was called to a child of four years, who complained of fever, headache in the forehead, with flushed face and sore throat. On examining the fauces, I found the right tonsil and arch of the velum covered with a heavy, yellowish-white membrane. There was some fetor of breath and great prostration.

Now, thought I, here I can see *Kaolin* work. But as the patient exhibited marked *Belladonna* symptoms, I did not dare to trust her to a new remedy alone. I was the more ready to give both, since I well knew that *Belladonna* alone had no power to cure diphtheria. The prescription therefore was *Belladonna* 3 alternately with *Kaolin* 6 (both in water) every half hour. This was at 12 o'clock in the day. At 4 p.m. I was summoned because of an epileptoid convulsion, which I learn was hereditary. But on examining the throat, what was my surprise and delight to

find more than half the membrane had disappeared, leaving a healthy surface, and that the rest was nearly detached. By the next morning it was all gone, and the child was convalescent.

Case 3.—L. R., a girl of ten years, was taken with sore throat, of which little was thought until at 2 p.m., December 25th, she was seized with a convulsion. It was slight, however; an examination of the fauces showed the whole pharyngeal region covered with a thick brownish-yellow membrane. The tongue was also of a similar colour. Skin hot and dry, and the pulse very rapid and feeble.

R. *Baptisia* $\frac{1}{6}$ alternately with *Kaolin* $\frac{1}{80}$ every hour. Rapid improvement at once set in. Nearly all the membranous deposit was gone within twenty-four hours. An ulcer was, however, left upon the right tonsil, which rapidly yielded to *Merc. prot.* $\frac{1}{6}$.

Having obtained these decidedly brilliant results I speedily made them known to my colleagues in this city (Brooklyn), and the remedy has been considerably used. Several cases in which the diagnosis between membranous and highly inflammatory croup was difficult have yielded to its influence with unexampled rapidity, when other medicines have failed to relieve. The following instances are more marked.

J. B. Elliott, M.D., reports as follows :

I was called on January 14th, 1871, to see a child with membranous croup who had been suffering both from the disease and from allopathic treatment for five days. The patient appeared to be almost *in articulo mortis*. At the suggestion of Dr. Searle, I prescribed *Kaolin* $\frac{1}{80}$ every half hour. After eight hours, but little improvement being discernible, and fearing to trust the case wholly to an unproven, and, to me, untried remedy, I alternated it with *Lachesis*. Now, in twelve hours, convalescence had fairly set, and full recovery was speedy. Dr. Elliott also reports a case of diphtheria in a young lady who was subject to it, in which the membrane was fully developed in this attack, and covered both sides of the throat. Twelve hours sufficed to remove every vestige of the membrane under the use of *Kaolin* 6 alone.

H. E. Morrill, M.D., also verbally reports several cases of diphtheria which have yielded very rapidly to *Kaolin*.

I would advise the use of this remedy in these diseases. And from analogy (although analogies in medicine are sometimes

dangerous, and again brilliantly successful) I shall try its powers upon croupous nephritis and pneumonia, and membranous dysmenorrhea, *et id omne genus*. We may well try it in these diseases since we have so little that is brilliantly useful in them. It may be that the preferences of the remedy are for the pharynx and larynx, but it may control similar pathological processes in other organs.

Kaolin is a silicate of alumina, and is found native in the form of a clay from which pipes are made.

(*To be continued.*)

ON THE USE OF *SANTONINE* IN EYE-DISEASE.

By D. DYCE BROWN, M.A., M.D., Aberdeen.

THE following article appeared in a more condensed form in the *British and Foreign Medico-Chirurgical Review* for April, 1871, as a joint paper by Dr. Alexander Ogston, of Aberdeen, and myself. Having been, after its publication, requested by one of the editors of the *British Journal of Homœopathy* to re-write the paper in a more extended form, with the cases in full, I now do so, with Dr. Ogston's permission. The reason of my own name alone being appended to this paper is, that Dr. Ogston, not being a homœopath, wishes it to be so, for reasons which are obvious. In lieu thereof, I take this opportunity of expressing my great obligation to Dr. A. Ogston for his kindness in assisting me in this investigation, by putting to the test of experience my suggestion as to the possible or probable value of *Santonine* in eye-disease. Not laying myself out for ophthalmic practice, I see few cases other than those diseases of the superficial parts of the eye, such as occur in ordinary practice. I should therefore have been unable to produce sufficiently extensive results, had not Dr. Alex. Ogston, who was then Ophthalmic Surgeon to the Aberdeen Royal Infirmary, very kindly agreed to test the value of *Santonine* in those cases coming under his charge, which seemed in any degree likely to be benefited by it. The cases, then, recorded here

are all Dr. Ogston's, with the exception of the first and the two last, which are mine. For the record of these cases only, he begs it to be stated, he is responsible; but for all comments on the cases with a view to their elucidation, and for the remarks upon the physiological and homœopathic action of the drug, I am alone responsible.

That which first suggested to my mind the possibility that *Santonine* might be of value in certain diseases of the eye, was the following case, which occurred in my practice.

J. B—, a pensioner, aged about fifty, came to the Dispensary on May 19th, 1869, complaining of worms, and various dyspeptic and other symptoms arising therefrom. He was blind, and on first coming to the dispensary, barely distinguished light from darkness. His sight began to fail about three years before, getting gradually worse till he lapsed into his present state. The worm he was troubled with was the tape-worm; but from his description at the first visit it was supposed to be *ascaris vermicularis*. He was therefore put on *Santonine*, in doses of a twentieth of a grain twice a day. I may here state, in passing, that I have found in numbers of cases *Santonine* in the above doses an almost unfailing remedy in *ascarides*—far superior to *Cina*. I hope to enter more fully, with cases, into this point at another time. On coming up, a week after, he volunteered the statement that the powders were doing his sight good. He could now distinguish, he said, certain objects, such as a large placard on a wall, and his wife's white jacket. On finding that it was tape-worm he was suffering from, his treatment was for a time altered, and when he seemed to be free of worms, he was put on small doses of *Quinine* twice a day, as he was exceedingly thin, pale, weak, and with no appetite. The *Santonine* was again given once a day, at bedtime. This treatment was continued for some length of time, during which his general health and strength immensely improved. By the middle of July he said he could distinguish a passer-by, and even say whether it was a man or a woman. One day, on walking to the river Don, he could distinguish the water, and two light-coloured cows in a field. On holding up

a hand before his eyes, he saw it, but could not say how many fingers were presented. Both he and his wife remarked repeatedly that his sight had never been so good since he had first lost it.

Soon after the commencement of this improvement, I was so struck with what seemed to be more than a mere coincidence, that Dr. Ogston and I together examined his eyes with the ophthalmoscope. The following was the state of parts. The right optic disc was completely atrophied, and with that eye he had no perception of strong light. The left disc was atrophied also, but not so completely as the right: he easily perceived light with this eye. Nothing further than this atrophy was observed.

One not unfrequently hears of more or less blindness caused by the reflex irritant action of intestinal worms, but this was clearly not a case of that sort, as improvement sufficiently perceptible to the patient to induce him to volunteer a statement to that effect was produced after taking the *Santonine* for a week, and before the tape-worm was in the least affected. This improvement in vision, so unexpected, led me to suppose that *Santonine* might turn out a valuable remedy in certain forms of eye-disease, and on my suggesting this to Dr. Alex. Ogston, he, as I have already stated, kindly agreed to try the effects of its administration in various diseases of the eye.

He tried it in thirty-nine cases of different sorts, some of them not with much expectation of any good result, but more to test the limits of its sphere of action. To give as faithful a view as possible of the results, positive or negative, the cases are not selected, but *every case* where the *Santonine* was administered is here recorded.

It may not be considered amiss if, before recording the cases, I shortly explain the formulæ for measuring good or bad sight; as, to those who do not lay themselves out for ophthalmic practice, the formulæ may be confusing.

There are two kinds of types used for testing sight. 1, Jaeger's, which are of an arbitrary size, No. 1 being the smallest; and 2, Snellen's, in which No. 1 is also the smallest, but in which the numbers attached to the

types correspond with the number of feet from which the respective types can be read. Thus, in perfect sight, No. 1 can be read from one foot distance, and would be stated as $\frac{1}{1}$; or No. 6 can be read at six feet distance, and would be stated as $\frac{6}{6}$. In this and other similar formulæ, the upper figure denotes the number of feet from which the lower figure (the type) can be seen. Thus, $\frac{1}{8}$ or $\frac{7}{20}$ means that at one foot distance only No. 6 can be seen, or in the second case that at seven feet only No. 20 can be seen; these, therefore, showing deficient sight. When two fractions are given, it means that two different measurements at different distances have been taken. The meaning of ∞ is that the patient can only distinguish light, and no type at all. In these measurements Snellen's types are uniformly used.

With these preliminary observations, I proceed to record the cases.

CASE 1.—Mrs. M—, aged about 30, a ship captain's wife, consulted me for her eyes, which had been ill for nearly six months, and for which she had had, without benefit, *Mercury*, leeching, blistering, and confinement, with bandaging-up of the eyes. Her right eye had irido-choroiditis, with total adhesion of the pupil. Her left eye was the subject of pretty smart irido-choroiditis of the serous variety.

Ten days of careful treatment by *Corrosive sublimate* and *Quinine*, with bandaging and *Belladonna*, but outdoor exercise in moderation, and with careful diet and attention to general health, so far improved both eyes that an iridectomy was performed on the right eye on the 29th of January, 1870, with complete success. Next day, a course of *Santonine* (one grain every night) was commenced, the sight in the left eye being then $\frac{1}{100}$. Recovery, as measured with the types, was very rapid, so that on the 12th February her sight was $\frac{1}{20}$ and $\frac{1}{1}$. She was then sent home, and the *Santonine* was discontinued.

Remarks.—In this case of irido-choroiditis, the improvement after commencing the *Santonine* is very marked; from being able only to read No. 100 at five feet, she can read No. 20 at ten feet, and No. $1\frac{1}{2}$ at one foot, the latter measurement being almost normal. This in thirteen days.

CASE 2.—Isabella M—, aged 52, had, on the 13th August

1869, been suffering for twelve months from cerebral amblyopia in the left eye; and for the last six months, in addition, from complete paralysis of the left motor oculi nerve. All other nerves, including the 4th and 6th, perfect. For two months she had been subjected to tonic treatment (under Dr. Ogston), cod-liver oil, and *Oxide of Silver*, without much benefit, the paralysis improving slightly. She complained also of headache.

August 13th, 1869.—*Left eye*.—Sight $\frac{1}{3}$. Colour vision perfect. Pupil beginning to be less dilated than at first. Retina, optic nerve, and choroid appeared normal; vessels of retina rather narrow. *Right eye*.—Sight $\frac{1}{3}$. Presented a caneroid ulcer of right upper eyelid, which was cured after some time by the local application of fuming *Nitrate of Mercury*.

To continue the cod-liver oil, and take one grain of *Santonine* every night. The *Santonine* produced in the urine, after standing for some time, a pink colour, dissipated by heat and acids.

September 7th.—Left eye, sight = $\frac{1}{3}$. Headache gone.

16th.—Left eye, S. = $\frac{1}{3}$. (S. = sight.)

22nd.—Had left off the *Santonine* for a week, and the sight had gone back to $\frac{1}{3}$. To resume the *Santonine*.

28th.—Left eye, S. = $\frac{1}{3}$.

October 4th.—The same.

11th.—The same. Can now turn the eye well inwards. Motion deficient upwards. When the eye is turned downwards, the bulbus rotates on its long axis from continued paralysis of the inferior oblique muscle. Ptosis gone. Complains of thirst, and is passing large quantities of urine, which contains no sugar. After omitting the *Santonine* for some weeks, her sight, on the 22nd November, had deteriorated, being = $\frac{1}{3}$. To recommence the *Santonine*. Her vision again improved to $\frac{1}{3}$, where it has remained ever since (16th February, 1870).

Remarks.—This case is very interesting, as besides the improvement in the sight of the left eye (the right eye being healthy), from $\frac{1}{3}$ to $\frac{1}{3}$, the headache of which she complained was cured, and the paralysis of the motor oculi nerve was so much improved that she could turn the eye well inwards and downwards; the ptosis also was cured. This, after two months of the first treatment had been carried on without much benefit.

CASE 3.—Mr. C—, student of medicine, aged about 30, congenitally colour-blind in right eye, and amaurotic in left eye;

complained on Jan. 13th, 1870, of pain in the eye on reading. He had been working hard with the microscope for some time, and showed signs of hyperæsthesia of the retina. Papillæ of the left eye red; retinal vessels a little hazy; arteries small.

[R. S. will be used to designate the sight in the right eye; L. S., left eye.]

R. S. = $\frac{1}{3}$. To use convex glasses, as he cannot leave off reading, and to take one grain of *Santonine* every night.

February 1st.—R. S. = $\frac{1}{2}$. Pain on reading much diminished. Continue *Santonine*.

23rd.—R. S. = $\frac{1}{3}$. He has been working hard with the microscope again, and has left off the *Santonine* for some days. He finds, however, he can use the eye much longer and with less trouble.

March 14th.—R. S. = $\frac{1}{3}$. Feels much better; no pain in the eye; can read comfortably now.

Remarks.—Cases of this kind are very common and very troublesome to the patient, and the beneficial effects of the *Santonine* are extremely well shown in this case, as during the treatment he, as a student, was placed in the very worst possible condition for improvement. While an ordinary patient would be directed to give the eye rest from reading, Mr. C— had to read for examinations, and, what was worse, had to continue microscope work. It will also be observed that, besides the cure of the symptoms of which he complained, his sight was improved, as measured by the types.

CASE 4.—Mr. A. S—, teacher, seen first on 11th December, 1869. *Left eye.*—Diffuse and intense retinitis; papilla undistinguishable, except from the convergence of vessels to it. Vessels contorted, bent, and interrupted by exudation into retina; arteries scarcely to be seen; veins huge. S. $\frac{1}{6}$, and counts fingers at five feet distance. *Right eye.*—Slight exudation into retina in the neighbourhood of the papillæ. S. = $\frac{7}{8}$, and with a convex lens of + 8 = $\frac{1}{3}$. To have $\frac{1}{20}$ grain of *Binioidide of Mercury* three times a day, and one grain of *Santonine* every night.

January 7th, 1870.—R. S. = $\frac{1}{3}$, (with a convex lens of + 8). L. S. = $\frac{1}{30}$. To omit the *Mercury* and continue the *Santonine*.

February 20th.—R. S. = $\frac{1}{3}$, and with + 8 convex lens = $\frac{1}{3}$. L. S. = $\frac{1}{30}$, and counts fingers at one foot distance. Atrophy of left retina is distinct; right retina appears healthy. Omit all medication.

Remarks.—This was a very severe case ; and though atrophy of the left retina resulted, yet the sight on this eye improved from $\frac{1}{6}$, and being able to count fingers only at five feet, to $\frac{1}{36}$, and counting fingers at one foot ; that on the left eye also improved slightly. Of course, in this case, no such definite conclusions as to the value of *Santonine* can be drawn as in the previous cases, owing to the simultaneous administration of *Biniiodide of Mercury*.

Dr. Ogston was in this set of cases making the experiment of the power of *Santonine* for the first time, and until he quite knew its sphere of action he did not feel himself at liberty to omit altogether the use of *Mercury* in serious inflammations of the deep-seated parts of the eye. *Mercury*, generally in the form of the *Biniiodide*, was the only medicine in whose curative powers in these diseases he had any faith ; his faith even in it being far from strong. The results of all the cases in which he gave the *Mercury* and the *Santonine* at the same time, he informs me, are far better than, judging from his experience, he could have expected from the *Mercury* alone. This statement introduced here, will therefore save a repetition in subsequent cases.

CASE 5.—John S—, seaman, aged 26, had syphilis three years ago. He felt blindness coming on, without pain, in January, 1869, and in March he had total loss of vision. He was admitted to the Hospital on July 27th, 1869. He then saw nothing with either eye, and could not distinguish light from darkness. There is a slight leucoma on each cornea. Retina totally and diffusely inflamed, and showing the enlarged and tortuous veins winding through the thickened retina. Arteries not visible ; optic disc obscured, and marked merely by the exit of the veins ; no choroiditis. To be leeches freely on both temples, and to have a pill containing $\frac{1}{2}$ grain of *Biniiodide of Mercury*, and one grain of *Santonine*, three times a day.

August 13th.—Condition of vision unaltered ; still unable to distinguish light from darkness. Retinal infiltration almost gone, only a little remaining in right eye. The retina shrunken and wasted, so as to contain only the narrowed and tortuous veins, and to show the healthy choroid through. Optic discs white, their margins obscured, and their vessels small, entirely venous, and indicating atrophic excavation. Dr. Ogston adds, “Incurable from the beginning, but the attempt was made to save him by treatment.”

Remarks.—This case of neuro-retinitis is a failure; but this is not to be wondered at, as it was evidently incurable from the first. It is merely recorded, as *every* case in which the *Santonine* was given is here related.

CASE 6.—Wm. R—, aged 19, farm servant, was admitted to the hospital on July 17th, 1869. His state was as follows:—On the *right eye* there was total old choroiditis, with complete atrophy of choroid, retina, and nerve. Cannot distinguish light from darkness with this eye. *Left eye.*—S. = $\frac{1}{8}$ and colour vision uncertain. He had small patches of ring-formed choroiditis disseminata, quite recent, at different parts of the back of the eye. The papilla was red; retina and nerve otherwise apparently normal. Peripheral vision deficient in patches. Central vision as above. To have $\frac{1}{2}$ gr. of *Biniiodide of Mercury* three times a day, and one grain of *Santonine* every night at bedtime.

July 29th.—Left sight $\frac{1}{8}$, and sees all colours but dark green, which he calls blue. Continue.

August 5th.—A shred of opacity observed in vitreous humour of left eye, close behind the lens. L. S. = $\frac{1}{8}$. Colour vision uncertain, except for blue, which he recognises accurately. Continue the *Mercury*, and take one grain of *Santonine* three times a day. This dose produced the xanthopsia characteristic of *Santonine*.

9th.—L. S. = $\frac{1}{8}$, and colour vision perfect. He was now obliged to leave the hospital. To continue the *Santonine* at home. Owing to the tenderness of his eye to the light, the ophthalmoscopic appearances could only be made out on the day of his admission, and the day following.

Remarks.—The remarks appended to last case will be borne in mind. The sight improved from $\frac{1}{8}$ to $\frac{1}{8}$, and colour vision, at first very imperfect, was quite restored. This latter seems nearly always to follow the *Santonine* treatment.

CASE 7.—Miss S—, aged 24, in good health, but suffering habitually from headaches, at intervals of about a week, and much given to painting and reading, observed for the last few months an increasing deficiency of vision.

October 8th, 1869.—*Left eye.*—S. = $\frac{1}{3}$. Colour vision perfect. Papilla pale and atrophic, but not otherwise abnormal. *Right eye.*—S. = $\frac{1}{4}$ and $\frac{1}{10}$. Papilla confused at its margin from slight reddening of retina. Arteries small and indistinct. Colour

vision perfect. Her left eye had never been a good one, and the vision in it is described by her as being much the same as it usually is.

22nd.—Left eye, S. = $\frac{1}{3}$. Right eye, S. = $\frac{1}{4}$ and $\frac{7}{8}$. Her headaches had been improved, almost entirely gone. Sight is worse in the morning, gets better later in the day, and the patient is now quite satisfied that her vision is as good as ever. To continue the *Santonine* in $\frac{1}{2}$ -grain dose at bedtime, for a fortnight, and to discontinue it; also directed to avoid small print, deficient illumination, and *fine* painting.

Remarks.—This case Dr. Ogston sets down as amblyopia from retinal irritation, and shows well the value of the *Santonine* in improving the vision of the right eye from $\frac{7}{10}$ and $\frac{1}{2}$, to $\frac{7}{8}$ and $\frac{1}{4}$; that is, it is perfectly restored, the latter fraction being above the ordinary standard of sight. The cure of the chronic headaches will also be noticed.

CASE 8.—Wm. B. W—, aged 35, mate of a sailing vessel, contracted syphilis about eighteen months ago. When secondaries supervened, being at sea, he took blue pill regularly in large quantities for many months. After a ducking he first observed trouble in his eyes, with frontal pain and dimness of sight. This rapidly passed off, and for a time he was well. About six months ago his vision began again to fail; this time with no pain. He attended Moorfields Hospital, at which was advised to get rapidly under *Mercury* if he wished to save his sight. On the card from this hospital, which he produced, was inscribed the diagnosis “Syphilitic Hyalitis.”

On the 23rd of August, 1868, when he came under Dr. Ogston's care, his sight with the right eye was No. 19, and with left eye No. 20, Jaeger, at one foot. His right eye bore on the capsule of the lens pigmentary traces of past iritis. No adhesions of iris. In both eyes the opacities of the vitreous body prevented all view of the retina and choroid, but as colour vision was very deficient in him, retinitis was also diagnosed. The amount of colour vision was out of all proportion to the amount of the opacity. He could only see blue. To take *Quinine* and *Iodide of Potassium*.

30th.—Can read nothing with his right eye; with the left reads No. 22, Jaeger. To have *Biniiodide of Mercury* in gradually increasing doses.

September 19th.—R. S. = $\frac{1}{10}$. L. S. = $\frac{1}{12}$.

25th.—R. S. = $\frac{1}{13}$. L. S. = $\frac{1}{3}$.

30th.—R. S. = $\frac{1}{4}$. L. S. = $\frac{1}{3}$.

October 12th.—R. S. = $\frac{1}{2}$. L. S. = $\frac{1}{2}$.

November 5th.—R. S. = $\frac{2}{3}$. L. S. = $\frac{3}{8}$.

December 3rd.—R. S. = $\frac{1}{4}$. L. S. = $\frac{1}{5}$.

January 27th, 1869.—R. S. = $\frac{1}{3}$. L. S. = $\frac{1}{3}$.

March 6th.—R. S. = $\frac{1}{3}$. L. S. = $\frac{1}{3}$.

After this he went to sea, and was not again seen till July 26th, 1869, when he came again under Dr. Ogston's care, saying he was much worse. He had continued the *Biniiodide of Mercury* all this time. L. S. = $\frac{1}{4}$. Discerns blue and green colour. R. S. = $\frac{1}{1000}$. Cannot distinguish colour at all. To continue the *Biniiodide of Mercury* and take also one grain of *Santonine* every night.

August 16th.—L. S. = $\frac{1}{3}$. Still uncertain in judging of colours, especially red and yellow. R. S. = $\frac{1}{1000}$. Colour vision slightly present (for blue).

September 20th.—Has continued the *Santonine* since last report. Notices that it makes him pass his water more forcibly. R. S. = $\frac{1}{100}$, and counts fingers at two feet. L. S. = $\frac{1}{2}$; this reading indistinctly.

November 8th.—R. S. = $\frac{1}{200}$, and counts fingers at four feet. L. S. = $\frac{1}{2}$; reads this now clearly. Sees colours now fairly, mistaking only red, yellow, and mixed hues, as purple and violet.

Remarks.—This is a very interesting case. During the first course of treatment with *Biniiodide of Mercury*, the sight improved very markedly, from being able to read nothing with the right eye to reading No. 3 at one foot distance. The *left* eye also improved till he was able to read No. 2 at one foot, subsequently deteriorating to reading No. 5 at one foot. This improvement under *Mercury* was very satisfactory; but though he had continued the *Mercury* during the four and a half months when he was again at sea, he came back with the right eye so bad as to have $\frac{1}{1000}$ as its formula, and to be totally blind to colour; that is, only a degree better than simply distinguishing light from darkness. After the patient was put on *Santonine* (the *Mercury* being still continued) this eye improved till S. was = $\frac{1}{100}$, and fingers could be counted at two feet; this, however, again falling off to S. = $\frac{1}{200}$, and counting fingers at four feet. The *left* eye, always the best of the two, improved from $\frac{1}{4}$ to $\frac{1}{2}$; colour vision also much better than previously. The improvement in this case may be fairly attributed

to the *Santonine*, as the great deterioration in the right eye had occurred in spite of the *Mercury* being continued from the time of leaving the hospital till his return.

CASE 9.—James W—, admitted to the hospital December 2nd, 1869. *Left eye* atrophied and with no sight at all, tender on pressure in the ciliary region. On the *right eye* there was also a little ciliary tenderness, and muddiness of the iris, but the dioptric media were quite clear, and showed a white papilla slightly cupped (atrophy). R. S. = $\frac{1}{30}$. Colour vision inaccurate; is not sure even of blue. To take one grain of *Santonine* every night.

December 7th.—R. S. = $\frac{1}{30}$. Colour vision perfect.

18th.—R. S. = $\frac{1}{15}$.

20th.—R. S. = $\frac{1}{10}$.

29th.—R. S. = $\frac{1}{8}$.

January 6th, 1870.—R. S. = $\frac{1}{4}$.

10th.—R. S. = $\frac{1}{4}$. Dismissed at desire; but to continue the *Santonine*.

Remarks.—The results in this case are particularly satisfactory; the sight improving in about a month from $\frac{1}{30}$ to $\frac{1}{4}$, while colour vision, which was at first very bad, became perfect in five days. It will be also observed that the colour vision was restored before any improvement in the sight showed itself.

CASE 10.—Mrs. L—, aged 40, applied at the hospital on January 13th, 1870, complaining of deficient vision. *Left eye*, S. = $\frac{1}{30}$. Sees all colours perfectly, except yellow and red, which she is incapable of distinguishing or recognising. *Right eye*, S. = $\frac{1}{1000}$. Colour vision totally abolished. To use *Atropine*; and return to-morrow.

January 14th.—Pupils have not dilated under the *Atropine*. The ophthalmoscopic examination revealed on both eyes, patches, numerous and extensive, of atrophy of the retina and choroid from previous exudative choroiditis. To use *Atropine* every second day (to dilate the pupil), and one grain of *Santonine* daily at bedtime; and to return in a fortnight.

28th.—Pupils still undilated; vision exactly the same. Dismissed incurable.

Remarks.—This case was evidently from the first incurable from previous extensive disease of retina and choroid. The failure of *Santonine*, therefore, was only to be expected.

CASE 11.—W. C—, seaman, applied as an out-patient on December 9th, 1869. *Left eye*, S. = $\frac{1}{8}$. Colour vision deficient except for blue. *Right eye*, S. = 0. Right pupil irregular.

His pupils did not dilate with *Atropine*, but the ophthalmoscope revealed intense silky whiteness of both optic discs, and the vessels were small and chiefly venous. To use *Atropine* locally daily, and to have one grain of *Santonine* every night.

December 28rd.—L. S. = $\frac{1}{12}$; recognises no colour with this eye. R. S. = 0, as before. Pupils are still undilated, though *Atropine* has been used daily. Optic discs distinctly whiter and more atrophic than before; the cupped disc of atrophy being faintly indicated on all the vessels, which are unusually narrow. Sent away, and told that I could do him no good. This man subsequently became stone-blind.

Remarks.—In this case of rapid atrophy of the optic nerve *Santonine* failed, as all other medicines do.

CASE 12.—S. N—, complains, on December 7th, 1869, of deficient sight. Colour vision good in both eyes. With the ophthalmoscope were observable the remains of exudative choroiditis in both eyes, and also atrophy of the retina and optic discs distinctly marked on both sides.

R. S. = $\frac{1}{16}$; L. S. = $\frac{1}{40}$. To take one grain of *Santonine* every night.

Not seen again till January 29th, 1870. R. S. the same as last report; L. S. = $\frac{1}{20}$. To continue the *Santonine* for two months.

Seen again in June, 1870. No further improvement according to his own statement. Not examined by measurement. Not seen again. Improvement in this case $\frac{1}{40}$ to $\frac{1}{20}$.

CASE 13.—Mr. J. B—, student of law, has for four years suffered from weak sight, and pain on reading, so that he has had to give up his studies altogether. He had been treated by one of the physicians in Aberdeen, by tonics, including *Quinine*, without any benefit.

December 8th, 1870.—He has no active disease observable in either eye.

R. S. = $\frac{1}{8}$ and $\frac{1}{4}$; and L. S. = $\frac{1}{8}$ and $\frac{1}{4}$.

The point of nearest vision is three inches and a half in front of each eye. No hypermetropia or myopia.

He complains of pain on reading in both eyes, especially on the

left side, and has been off duty for a year on account of it. Pain most felt at night, when reading by gaslight has been indulged in. Colour vision perfect. No injection of anterior part of eye, nor apparent disease anywhere. To use baths and exercise, and to take five grains of *Citrate of Iron* and *Quinine* three times a day, and to have one grain of *Santonine* every night.

23rd.—Feels himself improved, and suffers much less from pain, although he has been reading moderately.

R. S. = $\frac{7}{8}$; L. S. = $\frac{7}{8}$. To continue the same treatment.

January 19th, 1870.—Is improved. Can now read a considerable time without much fatigue, and at night feels pain, but only at night.

R. S. = $\frac{7}{8}$ and $\frac{1}{8}$; L. S. = $\frac{7}{8}$ and $\frac{1}{8}$. To take two grains of *Santonine* every night.

May 3rd.—Free from any pain or uneasiness whatever; cured. Recommended to use electricity for a fortnight, leaving off the *Santonine*. At the end of this time called, saying he was perfectly well.

Remarks.—In this case it is stated that previously he had been under “tonic” treatment, including *Quinine*, without any benefit. One may also take for granted that *Iron* formed part of this “tonic” treatment, as the combination of *Iron* and *Quinine* would be the first drug treatment which would occur to the allopathic mind in such a case. In Dr. Ogston’s treatment, comparing the results of similar cases in which the *Santonine* was given alone, we may without hesitation put down the successful result to the *Santonine*. The sight from being below the mark ($\frac{7}{8}$) improved till it was better than the standard sight of health ($\frac{7}{8}$), while, as in similar cases, the pain and feeling of fatigue in eyes from reading, &c., were entirely removed.

CASE 14.—Captain M—, complains of deficient vision of six months’ duration. Seen first on January 15th, 1870. Had syphilis some time ago. With the ophthalmoscope, the papilla of the *right eye* is very red, but no signs of retinitis are perceptible. In the *left eye*, nothing could be made out but diffuse opacity of the vitreous. No tenderness of bulbi. Tension normal.

R. S. = $\frac{7}{8}$ and $\frac{1}{8}$; L. S. = $\frac{7}{8}$ and $\frac{1}{8}$. In *right eye* reads $\frac{1}{2}$ with great difficulty, and in that eye colour vision is perfect. In the *left eye* colour vision is deficient, especially for blue. To have one

grain of *Proto-iodide of Mercury* in pill every four hours. Directed to go on to salivation, as he was leaving Aberdeen: To have also one grain of *Santonine* every night at bedtime.

February 15th.—Had been purged and sickened so much by the pills (*Mercury*) that he did not continue them beyond the first two days. But he continued the use of the *Santonine* regularly. R. S. = $\frac{7}{30}$ and $\frac{1}{4}$. L. S. = $\frac{7}{40}$ and $\frac{1}{4}$. Colour vision perfect. To continue the *Santonine*.

He, in spite of advice to the contrary, applied himself to studying and writing constantly for some examination he had to pass, but in spite of this the improvement continued, and six months later his vision was found not to have deteriorated. He continued the *Santonine* up till June, 1870.

Remarks.—This seems to me a very important case. Had the *Mercury* been continued, part at least, of the improvement might have been ascribed to it, but as he only took it for two days, and during that time it sickened and purged him so much that he had to stop it, the influence of the *Mercury* must be eliminated from the question. We have, then, the *Santonine* in a month improving the vision of the *right eye* for near distances from $\frac{1}{4}$ to $\frac{1}{2}$ (the normal standard), and in the *left eye* from seeing no types at all at seven feet distance to seeing No. 40 at this distance, and at the distance of one foot from seeing only No. 4 he could read No. 1 $\frac{1}{4}$. The colour vision also in this eye, though at first deficient, was quite restored; this improvement continuing in spite of steady and hard exercise of the eyes.

CASE 15.—George W—, fisherman. Seen first on the 29th January, 1870. Complains of deficient sight; bulbi natural in appearance and tension. In the *right eye* the colour vision was certain only for blue tints. In the *left eye*, strange to say, the colour vision was good, except for green. The ophthalmoscope showed atrophy of the papillæ on both sides.

R. S. = $\frac{7}{30}$ and $\frac{1}{8}$. L. S. = $\frac{7}{20}$ and $\frac{1}{8}$. To take 1 grain of *Santonine* every night.

February 29th.—R. S. = $\frac{7}{30}$ and $\frac{1}{8}$. L. S. = $\frac{7}{20}$ and $\frac{1}{8}$. He was sent away incurable.

Remarks.—*Santonine* here failed, but both papillæ were atrophied, and the atrophy was progressing; cases such as this are, one may say, not amenable to any treatment.

CASE 16.—John McC—, seen on the 19th of March, 1870.

Deficient sight for some time. In both eyes has punctiform posterior adhesions of the iris to the lens, and deposits on the back part of both lenses.

R. S. = $\frac{1}{2} \frac{7}{10}$ and $\frac{1}{15}$. L. S. = $\frac{7}{70}$ and $\frac{2}{30}$, the eyes being examined under the influence of *Atropine*. Colour vision was perfect in both eyes. To use *Atropine* every morning to dilate the pupil, and to have one grain of *Santonine* every night. This patient did not come back.

CASE 17.—John A—, house-carpenter, aged 38, says that his sight has been failing for some years, and has consulted many medical men upon his case. An ophthalmic surgeon in another town advised him to wash his head with cold water.

April 6th, 1870.—Eyes externally natural.

R. S. = $\frac{1}{12}$ and $\frac{1}{3}$. L. S. = $\frac{7}{40}$ and $\frac{1}{3}$. Colour vision fair. To take one grain of *Santonine* every night.

April 28th.—R. S. = $\frac{7}{2}$ and $\frac{1}{15}$. L. S. = $\frac{7}{20}$ and $\frac{1}{4}$.

May 21st.—R. S. = $\frac{7}{7}$ and $\frac{1}{4}$. L. S. = $\frac{7}{30}$ and $\frac{1}{4}$.

July 2nd.—R. S. = $\frac{7}{7}$ and $\frac{1}{4}$. L. S. = $\frac{7}{30}$ and $\frac{1}{4}$.

July 14th.—R. S. = $\frac{7}{7}$ and $\frac{1}{4}$. L. S. = $\frac{7}{20}$ and $\frac{1}{4}$.

He was not content with this improvement and consulted another ophthalmic surgeon.

Remarks.—This patient's right eye made a marked improvement; the sight of the right eye from being $\frac{1}{12}$ and $\frac{1}{3}$ became normal ($\frac{7}{7}$ and $\frac{1}{4}$), that on the left improved, though to a less degree, from $\frac{7}{40}$ and $\frac{1}{3}$ to $\frac{7}{20}$ and $\frac{1}{4}$. Sufficient improvement, one would think, to please any one who had been ailing for years.

CASE 18.—Elizabeth C—, aged 60, married, had cataract advancing in both eyes for two years, chiefly and primarily in the left eye. On examination there was seen a double senile cataract with the cortical layers a good deal affected on the left eye; the same moderately involved in the right eye.

April 13th, 1870.—R. S. = $\frac{7}{100}$ and $\frac{1}{4}$. L. S. = $\frac{7}{200}$ and $\frac{1}{20}$.

May 4th.—R. S. = $\frac{7}{40}$ and $\frac{1}{3}$. L. S. = $\frac{7}{200}$ and $\frac{1}{15}$.

May 30th.—R. S. = $\frac{7}{40}$ and $\frac{1}{3}$. L. S. = $\frac{7}{200}$ and $\frac{1}{20}$.

June 23rd.—R. S. = $\frac{7}{70}$ and $\frac{1}{15}$. L. S. = $\frac{7}{200}$ and $\frac{1}{20}$. Omit *Santonine*.

Remarks.—Dr. Ogston marks this as a failure; the improvement from $\frac{7}{100}$ to $\frac{7}{70}$, in the right eye, being hardly worth mentioning as a success. Improvement in vision in a case of advanced senile cataract was scarcely to be looked for.

CASE 19.—Mrs. C—, aged 34, married, first seen on February 19th, 1870. In the right eye, from previous iris, had several adhesions of the posterior surface of the iris to the lens, so numerous as almost to constitute total adhesion. There was no irritability of the eye, although she complained of nocturnal headaches, but merely dimness of vision. The other eye was perfectly natural.

On February 21st an iridectomy was performed upwards under *Bichloride of Methylene*, and the result was quite satisfactory. The headaches were cured, and the eye was well in a week; dimness of sight remaining as before. She then went to the country.

On the 13th of April, 1870, she returned, complaining of increasing deficiency of vision, but no pain. The eye seemed normal, when examined externally and with the ophthalmoscope. The sight of this eye (the right) was $= \frac{7}{40}$. To have $\frac{1}{30}$ grain of *Bichloride of Mercury* in pill once a day, and two grains of *Santonine* in the same pill every night.

April 28th.—R. S. $= \frac{7}{20}$. To omit the *Mercury* and continue the *Santonine* alone for three weeks.

May 24th.—R. S. $= \frac{7}{15}$. Continue *Santonine*. This patient did not come back, but on seeing her casually sometime after, she said she saw as well with this eye as with the other.

Remarks.—Under the *Santonine* alone the vision continued to improve (from $\frac{7}{20}$ to $\frac{7}{15}$), and ultimately became perfect. Even during the time when the *Bichloride of Mercury* was given alone with the *Santonine*, the improvement (from $\frac{1}{40}$ to $\frac{7}{20}$), judging from the analogy of other cases, and from the subsequent improvement with *Santonine* alone, may be fairly credited to the *Santonine*.

CASE 20.—John W—, baker, had, in the left lens, a soft cataract, which had come on within a fortnight and developed rapidly. The lens was swelled and the iris bulged forwards.

R. S. $= \frac{7}{15}$ and $\frac{1}{2}$. L. S. $= \frac{1}{1000}$. To use *Santonine* one grain every night for six weeks, and then return. He wrote to say he was no better.

CASE 21.—George J—, at school, complained on May 3rd, 1870, of pain on reading, which had troubled him for two months, and had lately increased so much that, although fond of reading, he had to give it quite up. Eyes natural externally, and painless to touch.

R. S. = $\frac{7}{30}$ and $\frac{1}{2}$. L. S. = $\frac{7}{20}$ and $\frac{1}{2}$. Colour vision perfect. Emmetropic eye. Nearest point three inches distant from eyes, and then he reads No. 1 Jaeger freely. In bad daylight (evening) reads $\frac{7}{10}$. He is advised to go to the country, to avoid reading, to use baths, and exercise out of doors. To take five grains of *Citrate of Iron* and *Quinine* three times a day, and one grain of *Santonine* every night at bedtime.

May 20th.—R. S. = $\frac{7}{12}$ and $\frac{1}{2}$. L. S. = $\frac{7}{10}$ and $\frac{1}{2}$. To have two grains of *Quinine* three times a day, and one grain of *Santonine* every night.

June 2nd.—R. S. = $\frac{1}{2}$ and $\frac{1}{2}$. L. S. = $\frac{7}{2}$ and $\frac{1}{2}$. Cured.

Remarks.—The result here was most satisfactory, the sight of both eyes being improved till it was above the normal standard of health. Of course, the administration of *Citrate of Iron* and *Quinine*, at the same time with the *Santonine*, complicates the result, but this case is so similar to the one already recorded (Case 13) in which “tonics,” including *Quinine*, had done no good, and to others (Cases 3, 7, 24, 41) where the best results had followed the administration of *Santonine* alone, that we may, I think, fairly put down the improvement to the credit of the *Santonine*.

CASE 22.—Mr. Y—, consulted me on June 9th, 1870, on account of deficient sight. He had also liver disease and ascites, for which he had consulted Dr. —, and had been prescribed for by him.

Eye normal. Retina not infiltrated, but markedly atrophic. R. S. = $\frac{1}{10}$. L. S. = $\frac{1}{10}$. Colour vision fair. To have one grain of *Santonine* every night.

He died four weeks later. State of vision not known.

CASE 23.—Mr. Wm. W—, merchant, consulted me on the 25th of June for deficient sight. He had lived hard, and looked as if he had. Admitted that he was a heavy drinker and smoker.

R. S. = $\frac{1}{30}$. L. S. = $\frac{1}{15}$. Fails in discerning red and yellow, especially with right eye. To give up alcohol and tobacco, except two cigars per diem. To take baths freely; an ounce of *Quinine wine* three times a day, and to have one grain of *Santonine* every night.

September 5th.—Writes, “I am much better, and improving daily.” He did not discontinue the drinking and smoking.

Remarks.—Considering that this patient still continued his intemperate habits, and from our knowledge of the action of *Quinine*, I am inclined to attribute the improvement which he felt daily to the *Santonine*, and not the *Quinine*.

CASE 24.—Mr. J—, aged 16, student, requiring to read much in a badly lighted apartment, called on me July 1st, 1870, complaining of inability to read long on account of pain and aching in his eyes, relieved only by rest. Is not now fit for much. On examination eyes are normal; S. $\frac{1}{2}$. To take one grain of *Santonine* every night.

Cured in a fortnight.

Remarks.—The good effect of the *Santonine* here is plain; the hyperæsthesia of the retina was very considerable, though the powers of vision were above the standard.

CASE 25.—Miss J—, aged 24, governess, consulted me on July 4th, 1870. Menstruation irregular for a year past. Had conjunctival catarrh a few weeks ago, which is now cured. She at present complains of pain in the eyes, and difficulty of reading, as well as of dimness of sight. She had been taking *Quinine* and *Corrosive sublimate* by prescription of a leading London oculist. Colour vision perfect. Eyes normal on examination. Slight pigment in left eye, where conus would come.

R. S. = $\frac{1}{7}$ and $\frac{1}{14}$. L. S. = $\frac{1}{30}$ and $\frac{1}{14}$. To have one grain of *Santonine* every night for three weeks, and five grains of *Citrate of Iron* and *Quinine* three times a day.

July 23rd.—Pain gone. R. S. = $\frac{1}{6}$ and $\frac{1}{14}$. L. S. = $\frac{1}{12}$ and $\frac{1}{14}$. Omit the *Santonine*. Cured.

Remarks.—The observations I made on previous cases of a similar nature where *Ferr.* and *Quinine* were given at the same time with the *Santonine*, are verified by this case, as, before Dr. Ogston commenced treatment, she had already been taking *Quinine* and *Corrosive sublimate* with no effect. The good results then, in this case, I, without hesitation, attribute to the *Santonine*. As usual, the pain in such cases disappears entirely, while the sight of the left eye improved from $\frac{1}{30}$ and $\frac{1}{14}$, to $\frac{1}{12}$ and $\frac{1}{14}$; the sight of the right eye being normal at first.

CASE 26.—Eliza McK—, servant, consulted me on July 4th, 1870, for weak sight. She had no pain or any other complaint, except dim sight, which had lasted three months.

R. S. = $\frac{7}{30}$ and $\frac{1}{4}$. Colour vision in this eye good. L. S. = $\frac{7}{30}$ and $\frac{1}{6}$. Colour vision in this eye bad. To have one grain of *Santonine* every night, also a pill containing gr. iiij of *Ferrum redactum*, and one grain of *Quinine* twice a day.

August 4th.—Says she is much improved.

R. S. = $\frac{7}{20}$ and $\frac{1}{3}$. L. S. = $\frac{7}{40}$ and $\frac{1}{3}$. Colour vision the same. Did not return again.

CASE 27.—John H—, seen July 18th, 1870, had on the left eye an old displacement of the pupil from a previous perforating ulcer of the cornea, and prolapse of the iris. On that eye, total amaurosis; retina slightly infiltrated; R. S. $\frac{7}{12}$. Can read with a + 9 (convex) lens; No. 1 Jaeger at one foot distance. Colour vision defective. To use a + 9 lens, and one grain of *Santonine* every night. Did not return.

CASE 28.—James B—, aged 25, compositor, applied at the hospital, April 11th, 1870. He had for years been troubled with pain and uneasiness in the eyes on working at his employment for any length of time, or on reading, or other fine work. Says he had been treated at Montreal for asthenopia. Has a peculiar build of face, and very flat built eyes. A considerable degree of hypermetropia was present as well as deficiency of sight.

R. S. = $\frac{1}{3}$ and $\frac{1}{3}$. L. S. = $\frac{1}{4}$ and $\frac{1}{4}$. Colour vision perfect. A white cloudy patch in the retina, posterior to the vessels, and directly above the edge of the disc. To have one grain of *Santonine* every night.

May 15th.—R. S. = $\frac{1}{3}$ and $\frac{1}{3}$. L. S. = $\frac{1}{3}$ and $\frac{1}{4}$.

30th.—R. S. = $\frac{1}{3}$ and $\frac{1}{4}$. L. S. = $\frac{1}{3}$ and $\frac{1}{4}$. He then left off the *Santonine*, and used a + 12 lens (convex).

He was next seen on June 13th. R. S. = $\frac{1}{4}$ = $\frac{1}{3}$. L. S. = $\frac{1}{4}$ and $\frac{1}{4}$.

He was seen again in October; he was free from pain, but the sight much the same as when last seen.

Remarks.—The pain felt in such cases, as before remarked, seems unfailingly to disappear. The sight also during the time he used the *Santonine* improved considerably—in the right eye from $\frac{1}{3}$ and $\frac{1}{3}$, to $\frac{1}{4}$ and $\frac{1}{4}$, and in the left eye from $\frac{1}{4}$ and $\frac{1}{4}$, to $\frac{1}{3}$ and $\frac{1}{4}$.

CASE 29.—James S—, aged 68, fisherman, came to the hospital on June 13th, 1870, complaining of deficient sight. There are

peripheral opacities in the form of streaks and clouds in both lenses.

R. S. = $\frac{1}{3}$, and L. S. = $\frac{1}{3}$. To take one grain of *Santonine* every night for a month.

July 13th.—R. S. = $\frac{1}{3}$. L. S. = $\frac{1}{3}$. Opacities unaltered. Dismissed.

Remarks.—In this case of partial cataract in an old man, no improvement resulted from the *Santonine*.

CASE 30.—Elizabeth R— was admitted into the Hospital in March 1870, with nearly total posterior adhesions of the iris to the lens, in both eyes, from old syphilitic iritis. An iridectomy was performed upwards on each eye. The operation was normal, and the recovery rapid.

May 5th.—The vision was measured and found to be R. S. = $\frac{1}{70}$, L. S. = ∞ . To have one grain of *Santonine* every night.

13th.—R. S. = $\frac{1}{40}$. L. S. not stated.

20th.—R. S. = $\frac{1}{30}$. L. S. = $\frac{1}{2000}$.

27th.—R. S. = $\frac{1}{20}$. L. S. = $\frac{1}{2000}$.

June 3rd.—R. S. = $\frac{1}{20}$. L. S. = $\frac{1}{1000}$.

She then left for Newcastle, and was not seen again.

Remarks.—In this case the improvement was very considerable, from $\frac{1}{70}$ to $\frac{1}{20}$ in the right eye, and in the left from ∞ to $\frac{1}{1000}$.

CASE 31.—John W—, aged 48, was admitted into the hospital May 15th, 1870, with atrophic painful bulbus in left side. Right eye amaurotic. R. S. = $\frac{1}{3}$. The left bulbus was enucleated, and he got gr. j of *Santonine* every night.

June 28th.—R. S. = $\frac{1}{100}$. Dismissed incurable.

CASE 32.—John W. C—, aged 21; admitted into hospital May 13th, 1870, with specific chronic irido-choroiditis. Was put on *Bichloride of Mercury* and tonics. After irritation had subsided an operation for adherent iris was performed on both eyes; result successful.

May 21st.—L. S. = $\frac{1}{3}$. R. S. = $\frac{1}{200}$. To take one grain of *Santonine* every night.

27th.—L. S. = $\frac{1}{3}$. R. S. = $\frac{1}{100}$.

June 3rd.—L. S. = $\frac{1}{3}$. R. S. = $\frac{1}{100}$.

11th.—L. S. = $\frac{1}{3}$. R. S. = $\frac{1}{100}$.

He then left, and was not again seen.

Remarks.—The improvement in this case was from $\frac{1}{8}$ to $\frac{1}{3}$ in left eye, and $\frac{1}{20}$ to $\frac{1}{10}$ in the left eye.

CASE 33.—Alex. K—, aged 34, saddler; admitted to the hospital February 21st, 1870, with total double posterior adhesion of iris and deficient sight. An iridectomy upwards was performed on each eye. As the sight had not improved much (he had a hazy opacity in posterior layers of lens) he was on the 19th March put on one grain of *Santonine* every night.

On the 24th March he said his sight had considerably improved, and on measuring it it was found $\frac{1}{8}$ in each eye. Continue *Santonine*.

April 5th.—S. = $\frac{1}{8}$.

13th.—S. = $\frac{1}{2}$.

23rd.—S. = $\frac{1}{3}$.

30th.—S. = $\frac{1}{3}$. Dismissed, with powders to be continued. Not seen again.

Remarks.—In this seemingly unpromising case there was a wonderful improvement in the sight. It is unfortunate that no measurement was made before he got the *Santonine*, but after it had “considerably improved” to the patient’s own perception, the sight was only $\frac{1}{8}$. It must have, therefore, at first been considerably worse, but after a month’s treatment by *Santonine* it had improved to $\frac{1}{2}$.

CASE 34.—Mary C—, aged 50, applied at the hospital on the 9th December, 1869, in consequence of deficient sight, which had been felt for some time. In the *right eye* everything seemed normal, except congenital central excavation and red disc. Lens clear. Colour vision fair on this eye. *Left eye.*—Cornea and lens clear; extensive cataract in posterior capsule; colour vision absent.

R. S. = $\frac{2}{4}$. L. S. = ∞ . To have one grain of *Santonine* every night.

January 11th, 1870.—R. S. = $\frac{2}{4}$. Colour vision perfect.

February 1st.—R. S. = $\frac{2}{4}$. L. S. = $\frac{1}{10}$.

March 1st.—R. S. = $\frac{2}{3}$. L. S. = $\frac{1}{5}$.

May 11th.—R. S. = $\frac{2}{3}$. L. S. = $\frac{1}{5}$. No further improvement.

Remarks.—In this case the sight of the right eye improved from $\frac{2}{4}$ to $\frac{2}{3}$, colour vision becoming perfect; and in the left eye,

a most unpromising case, from the presence of extensive capsular cataract, the sight rose from ∞ to $\frac{1}{30}$ —a wonderful improvement.

CASE 35.—Mrs. D—, whom I had seen twelve months before for double, unripe cataract of a year's standing. On the 18th March, 1870, her state was as follows:—A nearly ripe senile cataract in the left eye and a nucleolar cataract on the right eye, the peripheral layers being clear, so that the total extent could be easily seen. She was ordered one grain of *Santonine* every night.

March 25th.—Believes she sees a little better. To continue the *Santonine*.

April 20th.—R. S. = $\frac{1}{30}$. L. S. = $\frac{1}{30}$.

May 17th.—R. S. = $\frac{1}{30}$. L. S. = $\frac{1}{30}$.

June 14th.—R. S. = $\frac{1}{20}$. L. S. = $\frac{1}{20}$.

July 12th.—R. S. = $\frac{1}{20}$. L. S. = $\frac{1}{20}$.

August 9th.—R. S. = $\frac{1}{12}$. L. S. = $\frac{1}{20}$.

September 6th.—R. S. = $\frac{1}{12}$. L. S. = $\frac{1}{20}$. To leave off the *Santonine*.

Remarks.—This may be considered a remarkable improvement in a case of so clearly marked double cataract. Unfortunately, again, the sight was not measured on first taking the *Santonine*, but after a month, when the sight was already improved perceptibly to the patient, the sight was R. S. = $\frac{1}{30}$ and L. S. = $\frac{1}{30}$. The rise of the sight to R. S. = $\frac{1}{12}$ and L. S. = $\frac{1}{20}$, only, therefore, indicates a part of the whole improvement.

CASE 26.—Hugh J—, labourer, applied at the hospital in August, 1870, for deficient sight. He had deficient hearing also, and symptoms of cerebral disease of the same standing as his deficient vision. The eyes were examined, and inflammatory exudation into the anterior layers of the retina, and slightly in optic disc, was discovered. He was then transferred to one of the physicians.

On August 31st, 1870, his cerebral symptoms being much the same, he expressed himself anxious that some treatment should be directed specially to the eyes, and he applied again to me; his sight was then R. S. = $\frac{1}{200}$. L. S. = $\frac{1}{100}$. He was ordered two grains of *Santonine* every night.

September 6th.—R. S. = $\frac{1}{30}$. L. S. = $\frac{1}{40}$. This improvement did not last long.

12th.—R. S. = $\frac{10}{100}$. L. S. = $\frac{10}{100}$. And as he was leaving the hospital on that day, and leaving town also, the medicine was discontinued.

Remarks.—A more unpromising case could hardly come under treatment, the deficient vision being unmistakably of cerebral origin; and it only shows the remarkable power of *Santonine*, that in such a case the sight could, under it, even temporarily improve from $\frac{10}{100}$ to $\frac{1}{3}$ on the right eye, and from $\frac{10}{100}$ to $\frac{1}{4}$ on the left.

CASE 37.—Archibald O—, gardener, aged 43, applied at the hospital on August 13th, 1870, complaining of deficient sight, which had been failing for six months.

R. S. = $\frac{2}{30}$. L. S. = $\frac{2}{30}$. Colour vision perfect. To have one grain of *Santonine* every night.

August 27th.—R. S. = $\frac{2}{3}$. L. S. = $\frac{2}{12}$. Continue.

September 24th.—R. S. = $\frac{2}{3}$, L. S. = $\frac{2}{3}$. Continue.

Not seen again, owing to my leaving the hospital. (Dr. Ogston at this time resigned the office of ophthalmic surgeon, owing to his being appointed one of the ordinary surgeons to the hospital.)

Remarks.—The improvement in this case is very great: from $\frac{2}{30}$ to $\frac{2}{3}$ on the right eye, and from $\frac{2}{30}$ to $\frac{2}{3}$ on the left.

CASE 38.— —, applied at the hospital on August 22nd, 1870, complaining of deficient sight. On examination by the ophthalmoscope nothing wrong is detected. Colour vision uncertain.

R. S. = $\frac{10}{100}$ and $\frac{1}{10}$. L. S. = $\frac{1}{40}$ and $\frac{1}{4}$. To have one grain of *Santonine* every night.

September 17th.—R. S. = $\frac{1}{40}$ and $\frac{1}{4}$. L. S. = $\frac{1}{20}$ and $\frac{1}{3}$. Continue *Santonine*.

Not seen again, as I left the hospital.

Remarks.—The improvement here is well marked, and the probability is that had Dr. Ogston continued the treatment, and been able to watch it, the result would have been still better.

Since writing the above, Dr. Ogston told me that he afterwards saw this patient, who said his sight was quite restored.

CASE 39.—W. C—, applied at the hospital on August 23rd, 1870, for retinal deficient sight. Colour vision perfect.

R. S. = $\frac{1}{12}$. L. S. = $\frac{1}{13}$. To have one grain of *Santonine* every night.

September 16th.—R. S. = $\frac{1}{20}$. L. S. = $\frac{1}{12}$.

Seen no more, as I left the hospital.

CASE 40.—A woman came to the Aberdeen Dispensary complaining of inability to do sewing work or reading for any length of time, in gaslight, without aching pain in the eyes. The conjunctiva became red after some time of fatigue to the eyes; otherwise well. To have *Santonine* gr. $\frac{1}{20}$ three times a day.

This woman did not return. The probability is that she was better, as dispensary patients, as a rule, if not better, return at least once.

CASE 41.—Mr. C—, aged 35, complained of having, for upwards of a year, been troubled with the following symptoms. After reading for some time, especially if reading aloud, his eyesight becomes dim, the letters of the book or newspaper become hazy and indistinct, so that he has to stop reading for a time. Rubbing the eyes seems to clear the sight for a few minutes at a time. In his office, if specially busy and anxious to get over any piece of business requiring writing and examination of books, the same symptoms come upon him; at other times his eyesight is excellent. Fancying that his sight was getting naturally weak, he had taken to using spectacles. He had no pain or aching in the eyes or head, and was otherwise in good health. I told him to leave off the use of spectacles, and to take one grain of *Santonine* every night. I saw him about three weeks afterwards, when he told me he had only taken six of the powders, but was now quite free of any of the symptoms formerly complained of.

Remarks.—The result here is as satisfactory as could be wished, this and other similar cases showing, beyond dispute, the power of *Santonine* in such cases.

Analysing these cases, we find the following results:

1.—Total cases treated, 42; cured or improved, 31; failures, 7; unknown results, 4.

2.—The seven failures are as follows:—

- (1). Syphilitic neuro-retinitis, with total blindness; incurable from the beginning (Case 5).
- (2). Old retino-choroiditis; incurable by everything else (Case 10).

- (3 & 4). Progressive rapid atrophy of optic nerve ; incurable by all other means (Cases 11 & 15).
- (5). Rapidly formed soft cataract (Case 20).
- (6). Senile cataract (Case 29).
- (7). Amaurosis ; incurable by all other means (Case 31).

These seven cases marked "failures" could not be expected to be otherwise. They were incurable by all other means, and to them *Santonine* was only administered by way of experiment, and not with anything like hope of a good result. Were it not for the desire to give a thoroughly truthful account of the experiments, chronicling *every* case when the medicine was given, they might have well been omitted.

3.—Two cases out of four of cataract were improved in vision, one slightly, and the other greatly (Cases 18 & 35). Even this result is remarkable.

4.—In all the remaining cases of organic disease of the back parts of the eye (choroiditis, retinitis, and more or less complete atrophy of the optic disc, some of them of long standing), there was improvement in some to a remarkable extent.

5.—Of sixteen cases of deficient sight, when no organic alteration was perceptible (amblyopia), nine were improved, most of them to a great extent (Cases 2, 3, 17, 23, 26, 28, 34, 37, 39) ; six were cured completely (Cases 7, 13, 19, 21, 25, 38), and one did not return (Case 27).

6.—Of eight cases where there was marked hyperæsthesia of the retina with pain on reading, &c., seven were cured ; the pain, &c., being completely removed, and ability to read, &c., without fatigue restored (Cases 3, 7, 13, 21, 24, 25, 41) ; and one did not return (Case 40).

7.—In two cases (one of cerebral amblyopia, and the other of amblyopia from retinal irritation, Cases 2 & 7), where headaches were much complained of, the headaches were cured. In the latter case (No. 7) the headaches were periodical, coming about once a week.

8.—In all the cases (excluding the seven incurable cases marked "failures") where deficient colour vision was

present, there was, with one exception (Case 26), a most satisfactory result, four having their colour vision quite restored (Cases 6, 9, 14, 34) ; one (Case 8) much improved, and one (Case 27) did not return.

9.—In one case (No. 2) there was complete paralysis of the right motor oculi nerve of six months' standing, and this was nearly cured.

10.—In six cases *Mercury*, generally in the form of the *Biniiodide*, was given at the same time as *Santonine*. The reason for this is explained at the commencement of this paper ; besides the reason there given, three of the six cases were syphilitic. It is important to see clearly what in these cases was done by the *Santonine*, as, on a cursory notice, the credit might be given to the *Mercury* and not to the *Santonine*. Of the three syphilitic cases, the first (Case 5) was a case of neuro-retinitis with total blindness, and incurable from the first. It may, therefore, be excluded. The second (Case 8) a case of retino-choroiditis, after improving remarkably under *Mercury* alone, deteriorated again till, in one eye, the patient was barely able to distinguish more than light from darkness ($S. = \frac{1}{1000}$) ; this deterioration occurring while the *Mercury* was being taken as before. On being put on *Santonine* (the *Mercury* being still continued) the sight improved to $\frac{1}{100}$ in this eye, and in the other from $\frac{1}{4}$ to $\frac{1}{8}$; colour vision also greatly improving. The third case (Case 14) is particularly interesting, as the *Mercury* was only taken for two days, during which time it so sickened and purged him that he had to leave it off. It may therefore be at once eliminated from the question ; after which we find the most remarkable improvement going on under the *Santonine* alone, in spite of constant and hard exercise of the eye. Of the three remaining cases, the first (Case 4) was one of diffuse retinitis, when *Mercury* was given for one month along with *Santonine* and then discontinued, the improvement going on under *Santonine* alone. In the second (Case 6), a case of old choroiditis, both medicines were given together the whole time ; and in the third (Case 19), a case of amblyopia, where *Bichloride of Mercury* was given for a fortnight along

with *Santonine*, and then discontinued, the improvement went on under the *Santonine* alone, the patient subsequently reporting herself as perfectly well.

Excluding, then, the incurable case, we find of the five other cases improvement clearly traceable to the *Santonine* in four. The impression produced on Dr. Ogston's mind, as he informs me (comparing the results of former cases treated by *Mercury* alone with those here recorded treated by *Mercury* and *Santonine* together, and with those treated by *Santonine* alone), is that, in the above-mentioned cases, the improvement is far greater than he could have expected to result from the use of *Mercury* alone.

11.—There were three cases in which *Iron* and *Quinine*, and one case in which *Quinine*, were administered along with the *Santonine*. It is important to settle the part which each of these medicines play in the cures, as was done with the *Mercury* in the last paragraph. Three of them are cases of hyperæsthesia of the retina with amblyopia (Cases 13, 21, and 25), and those three had *Iron* and *Quinine*. The first (Case 13) had already been treated by "tonics," of which *Quinine* is especially mentioned as one, and I may take for granted that *Iron* was another, with *no benefit*; I may at once, then, put down the cure to the *Santonine*. The same may be said of Case 25, where *Quinine* and *Corrosive sublimate* were previously taken with no benefit; while in Case 21, although there are no such data from which to judge, yet, comparing it with the other two cases, and with other similar ones (Cases 3, 7, 24, and 41) where equally good results followed the treatment by *Santonine* alone, there is no doubt, I think, that the cure was the result of the *Santonine*. The case in which *Quinine* alone was given along with the *Santonine* was one of alcoholic amblyopia (Case 23). His dissipated habits being still kept up, I think no one will doubt that, from our knowledge of the action of *Quinine*, the improvement can be credited only to the *Santonine*.

There can, I fancy, after a careful examination of the cases recorded in this paper with their analyses, be no room for doubt left in any one's mind that *Santonine* possesses

a remarkable power over many cases of inflammatory and atrophic alterations in the retina and optic nerve producing deficient sight. Many of these cases here recorded were very unpromising, and such as are little amenable to other treatment. It seems, then, to be superior in the treatment of such cases to the remedies ordinarily employed. I may state that Dr. Ogston is quite of this opinion, and places more reliance on it than on any other drug. It is hardly to be expected in a tissue where inflammatory action, even slight in degree, is so invariably followed by secondary contraction of the connective and atrophy of the nervous elements, that any drug we may ever fall in with should suffice for a perfect cure, or anything approaching to such. The utmost that can be expected is that the secondary atrophy should be checked, and the maximum of sight preserved.

When there is no inflammation or atrophy of the retina or optic nerve, but simply hyperæsthesia of the retina, *Santonine* acts beautifully, the relief to the pain and aching in the eyes, and the restoration of the patient to the power of using his eyes comfortably in reading or otherwise, being very rapid. Its remarkable power in the restoration of defective colour vision has already been noticed.

The method adopted in the foregoing cases of testing improvement or otherwise in the sight, namely, by careful measurements by the aid of Snellen's types, at frequent intervals, and at the same time of day (in order to ensure, as nearly as possible, the same illumination), is very important. The statements of patients are not always to be relied on, as they may imagine the results to be other than they really are, according as their temperaments are sanguine or the reverse.

Having now concluded the statement of the curative results obtained by *Santonine* in different forms of eye-disease, I come to a part of the subject which is peculiarly interesting to homœopaths, namely, the mode of action of the drug in question. Does it act homœopathically or not in diseases of the eye? I think I shall be able to show that it does so.

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For the eye and brain symptoms I am indebted to the interesting paper of Dr. Edmund Rose, of Berlin, on *Santonine*, translated in the *British Journal of Homœopathy* for 1869. Also I shall include the provings of *Cina*, which we already have, since *Santonine* is the alkaloid extracted from *Cina*.

Into the alterations in colour vision produced by *Santonine* I need not occupy valuable space by entering particularly. They are now too well known to require this, and for those who wish to refresh their memory on this point, I refer them to Dr. Rose's paper. It is, therefore, sufficient for my purpose to mention them. Dr. Rose refers this colour blindness to the retina, and in doing so states the important fact that *Santonine* produces hyperæmia of the retina. I shall quote this passage. "When we are able to perceive a violet colour, but, from the yellow sight (of violet blindness) are not sensible to light that otherwise seems violet, we must refer the affected portion of the organ of sense to the exterior extremity of the apparatus of the optic nerve, since the disorder has been proved to be a nervous one." Here a note is inserted as follows: "That the cause (of yellow vision) was not in the dioptric apparatus will appear by comparing Virchow's *Archiv*, vol. xviii, p. 81. There is adequate proof that the yellow sight is always the first symptom, yet even in fatal cases the papilla of the optic nerve always appeared white, never yellow, when observed with the eye speculum." I may here also state that Dr. Alex. Ogston found the same thing, in numerous experiments on animals with the drug, made for the purpose of ascertaining the mode of production of the *Santonine* yellow vision. He says, in our joint paper in the *British and Foreign Medico-Chirurgical Review*, April, 1871, "It became evident that the seat of this action lies in the retina or brain, since the other parts of the eye preserve their normal tints when the coats of the eye are removed to a small extent behind, so as to admit of the vitreous, lens, aqueous, and cornea being observed by transmitted light. No yellow pigmentation of the macula lutea was ever observed."

Continuing Dr. Rose's remarks, he says, "We refer it to the retina so much the more because, on the one hand, this disorder shows itself to be quite dependent on the admission of light, and on the other hand, in some of our strong poisonings great hyperæmia of the retina was clearly exhibited by the aid of the speculum. Veins as well as arteries (but especially the latter) were evidently distended; so that the two seemed alike, a result which is so much the more certain, because the rabbit always employed for that purpose had been diligently examined with the ophthalmoscope for fourteen days, so that we could accurately draw the course of its retinal vessels out of our head. Thus we arrived at the conviction that partial colour blindness is caused by the change of pressure to which the retina is exposed from the increased contents of the blood-vessels."

Then in *Jahr*, we find under the eye symptoms (pathogenic) of *Cina*, "aching in the eyes, when fatiguing them by reading." "Confusion of sight on reading, which disappears on rubbing the eyes." "Pupils dilated." "Weak sight, with photophobia and pressure on the eyes, as if sand had been introduced into them." And in *Lippe's Text Book of Mat. Med.* we find "pain in the eyes when using them at night by the candlelight."

The elective affinity of *Santonine*, then, for the eye, especially the retina and optic nerve, and its homœopathic action in cases of inflammation of retina and optic disc, in cases of hyperæsthesia of the retina, as well as in cases of deficient colour vision, is perfectly clear. The symptoms in *Jahr*, above quoted, "confusion of sight on reading, which disappears on rubbing the eyes," is exactly that which was complained of, and so rapidly cured, in Case 41.

Here I may mention some very interesting experiments by Dr. Alex. Ogston on the production of cataract by *Santonine*. Dr. Ogston found, in the experiments before mentioned, that it several times happened, especially when young kittens were employed, that within a few minutes after the animal was killed, a dense cataract developed itself in the lenses of both eyes. Within half an hour these parts

became quite opaque, the opacity remaining very marked after the removal of the lens from the eye. It seldom occurred to any extent in the eyes of adult animals, nor has it ever been observed to occur during life. In consequence of this cataract-producing power of *Santonine*, it was tried in the four cases of cataract already recorded. Improvement occurred in two of them to some extent, but none of the cases were good ones on which to test the power of *Santonine*, as the cataracts were nearly ripe, instead of commencing. It is possible that, in a case of a commencing cataract in a young person, the administration of *Santonine* might check its further progress.

Besides the action of *Santonine* upon the eye, especially the retina and optic nerve, it has unmistakeable action on the brain and cerebral nerves.

We have, 1, headache produced. In Jahr we find "numbing pressure as from a load, which weighs upon the head, especially when walking in the open air." "Tearing, drawing cephalalgia, aggravated by reading or meditation." "Dull pains in the head, with eyes fatigued, chiefly on waking in the morning." Dr. Rose also states that in eight cases out of thirty, there were "abnormal feelings and pains in the head."

2. We have nausea, retching, and vomiting produced. Cases of this are mentioned in Dr. Rose's article, pp. 218, 220, 221, *British Journ. of Hom.*, 1869. He also states (p. 239) that nausea and vomiting occurred in fourteen out of thirty cases. These symptoms, if carefully examined by comparison with others, will be seen to be not gastric, but cerebral. Dr. Rose is quite of this opinion. He says (p. 238, loc. cit.), "Lastly, I compare with the hallucinations (of sight) the nausea that ordinarily came on, which, in a high degree of narcosis, may proceed to repeated voluntary vomiting. As in the case of the senses, I believe we may distinguish two kinds of vomiting in persons who generally vomit with difficulty, viz. according as the stimulus affects the stomach or the brain. The one comes on according to the individuality, with various long and severe warnings, under the usual appearances in the *primæ viæ* (flow of saliva,

eructation, pressure on the gastric region, &c.), as in vomiting from gastric catarrh, or from the irritation of *Tartar emetic*. Here (in the case of *Santonine*) the vomiting is always conducted differently. Without any warning it commenced with me quite suddenly, and without the slightest pain; whereas the other brain symptoms always attain their maximum at the *same time*. This kind of brain sickness is met with on other occasions; for instance, during mental over-exertion, advanced narcosis from *Opium*, &c.

8. We have spasms of cerebral nerves. Dr. Rose (p. 283, loc. cit.) says that *Santonine*, in large doses, "killed by continued spasms. Even then I observed some cases in which these tetanic fits were confined to the nerves of the cerebrum, and before the spinal cord was implicated, the narcosis passed off. In all the poisoning cases that did not run their course too rapidly, every one may convince himself of the truth of my assertion, that at first spasms of the cerebral nerves alone occur, *i. e.* in muscles of the eye, ears, nose, nape, &c., and not till long after do they extend over the body. In medium doses the optic nerve and par vagum suffer constantly; at times, also, individual nervous centres in the brain. It will be borne in mind that I mean the centres of the five senses, *with the remarkable exception of the sense of hearing.*"

Dr. Rose also gives several examples of smell and taste being morbidly altered by *Santonine* (pp. 218, 220, 221, 235, loc. cit.). Hallucinations of smell, he states (p. 239), occurred in six out of thirty cases, and hallucinations of taste in five.

The foregoing facts, then, conclusively prove that *Santonine* acts specifically on the brain, and the individual cerebral nerves, including the optic, as well as the retina, and they sufficiently clearly account, on the principle of similars, for the cure of the headaches complained of in Cases 2 and 7; for the improvement in sight occurring in a case of cerebral amblyopia (Case 2), and in a case of well-marked cerebral disease (Case 36), and for the nearly complete cure of paralysis of the motor oculi nerve in Case 2.

It would be foreign to my present purpose to follow out

the physiological action of *Santonine* on other parts of the body. It is sufficient to have brought to light a most valuable and hitherto unknown addition to our homœopathic remedies for eye-disease.

EXPERIMENTAL RESEARCHES ON THE NATURE AND CAUSES OF CATARRHUS ÆSTIVUS (HAY- FEVER, OR HAY-ASTHMA).

By CHARLES H. BLACKLEY, M.R.C.S. Eng.

(Continued from page 286.)

CHAP. III.—EXPERIMENTS WITH THE PRESUMED CAUSES OF HAY-FEVER.

§ 78. THE object aimed at in these experiments has been to single out the agent which I believed was, in my own case, the principal if not the only exciting cause of the disease ; but I have also wished to show the ground I have gone over, as well as to indicate the point I have arrived at, in the hope that some one else may be induced to take up a similar line of investigation, and thus assist in correcting or in strengthening the conclusions I have come to.

The plan adopted has been, where practicable, to make each of the supposed causes the subject of separate as well as of combined and repeated experiment, and by these means to endeavour to eliminate such as have no power to produce the symptoms of the disorder.

§ 79. In the early part of the course it was deemed absolutely necessary to make each experiment as distinct as possible, so as not to allow the results of one to interfere with those of another ; but in the latter part, when experience had been gained, and had shown that this rule need not be strictly adhered to, the experiments were permitted to follow each other more rapidly. This was often done where it was desirable to see what effect could be

produced by the re-application of any particular substance when convalescence had not been fairly established after a former experiment.

Various circumstances which were to some extent unavoidable contributed to render the course of experimentation apparently somewhat irregular. The nature of some of the agents, combined with the accidental way in which these had to be obtained, made it often impossible to conduct the experiments in as systematic a manner as might have been wished. This irregularity, however, had one advantage, namely, that it has brought out the results of such observations in strong contrast with those which were made in a more regular manner and under more normal conditions.

§ 80. The agents which have been named as the exciting causes of hay-fever admit of several modes of classification. One of the best of these would be to place these agents in three categories, according as they are mechanical, chemical, or physiological in their action ; but here we are met with the difficulty of not being able to determine in all cases to which of the three classes an agent may belong. After close observation for a long period I am not able to decide which mode of action takes the lead in the case of the substance which I believe brings on the disease in my own case.

For practical purposes a very simple method of classification will perhaps answer better than a more elaborate one in enabling us to decide which are the exciting causes of the disorder. By this method of arrangement we place these causes in two divisions. In the first are included those substances which are more or less under the control of the operator, and which can be generated at pleasure or can be gathered and stored for future experiment. In the second division we place those agents which cannot be produced artificially and which are not capable of being controlled or altered in any way when generated naturally.

In the first division will be found *benzoic acid*, *coumarin* (the substance which gives the odour to newly-made hay), other *odours of various kinds*, *ozone*, *dust*, and *pollen*. In the second division we place *solar heat* and *light*.

§ 81. In respect to the facility with which we can conduct experiments with these two groups of agents there is a wide difference. In one case we can have these made under circumstances of our own choosing if not of our own creating; but in the other we shall generally have to trust to observations merely, and these will often have to be made in the presence of disturbing elements which are difficult to detect, and frequently when detected are incapable of being set aside. These circumstances must exercise a considerable influence upon the manner of conducting the experiments, and upon the deductions we make from the phenomena observed, but at the same time need not prevent the conclusions we arrive at being sound and trustworthy.

The account of the experiments will be given in much the same order in which the agents are mentioned in the two classes into which they are divided, viz. :—

- A. Experiments with *benzoic acid*.
- B. Experiments with *coumarin*.
- C. Experiments with *odours* of various kinds.
- D. Experiments on the action of *ozone*.
- E. Observations on the effects of *dust*.
- F. Experiments with *pollen*.
- G. Observations on the influence of *light* and *heat*.

A. *Experiments with Benzoic acid.*

§ 82. The experiments with this substance were tried in three different ways. 1st. By exposing the acid to evaporation, at ordinary temperatures, and inhaling the vapour.* 2nd. By applying a watery or spirituous solution of the acid to the mucous membrane of the nares. 3rd. By subliming the acid at high temperatures and inhaling the fumes.

* Seeing that *Benzoic acid* does not sublime at less than 293° Fahr., it seems useless to try experiments with it at ordinary temperatures; but as according to some authorities in chemistry it contains a small quantity of essential oil, which accompanies the acid during the sublimation, in the process of manufacture, it is not impossible that this oil may be given off at ordinary atmospheric temperatures, and that it may assist in producing the symptoms of hay-fever.

§ 83. In the first form of the experiment the acid was spread thickly on glass plates containing a superficial area of one hundred square inches, these being carefully weighed before being exposed. The room in which the plates were placed was a small room about 15 feet by 12 feet, and was kept at a temperature varying from 65° to 75° Fahren.; the average being about 68°. The room was kept closed for ten hours at a time, and after being so closed I entered it and spent a couple of hours in it on three separate occasions, so as to breathe the vapour if any had been given off from the acid. No effect whatever was noticed, which could be fairly attributed to the presence of the acid in any of the three trials. The plates were weighed again at the conclusion of the experiments, and were found not to have diminished in weight after a lapse of forty-eight hours.

§ 84. In the second form of the experiment cold distilled water was saturated* with the acid. A small strip of lint steeped in this solution was applied to the mucous membrane of one nostril, and was kept in this position for an hour. Another solution was made by dissolving a quantity of the acid in hot distilled water.† This was applied to the nostril at a heat of 120° Fahren. in the same way as the one above had been applied. Another solution was made by adding two drachms of proof spirit to eight drachms of water, and dissolving in this mixture twenty grains of the acid. This also was applied to one nostril in the same manner.

In order to be able to distinguish the effect of the alcohol from that of the acid a mixture of proof spirit and water (minus the acid) was applied to the other nostril immediately after the conclusion of the last experiment. A slight burning sensation was felt in each nostril after the conclusion of the experiments, and the mucous membrane was found to be slightly reddened in each case, but there was no difference perceptible between the action of the mixture containing the acid and that which was composed of alcohol and water. These experiments were repeated

* Cold water takes up $\frac{1}{250}$ th of its weight of the acid (Miller).

† Boiling water takes up $\frac{1}{35}$ th of its weight of the acid (Miller).

several times, but no effect which in any degree resembled the phenomena of hay-fever was seen in any of the trials.

§ 85. In the third form of the experiment, 3j of the acid was placed in a crucible, and was held over the flame of a Bunsen's burner, so as to cause the former to sublime more or less rapidly. At first the heat was applied gently, so as to allow the vapour of the acid to be only just perceptible, but in later trials the heat was increased until it brought out dense fumes.

During the progress of these trials I was present, and, indeed, manipulating all the time, but had no sensations like those of hay-fever. I had some dryness of the throat, with a feeling of irritation about the larynx, and a slight disposition to cough; but these sensations were not so marked in my case as in the cases of other persons who were in the room with me during a part of the time the experiments were going on,* and who had never been the subjects of hay-fever. It is almost needless to say that in each of these trials I inhaled the vapour of the acid freely, taking especial care in some of the later experiments to inspire through the nostrils only.

§ 86. From the uniformly negative results of all these experiments with benzoic acid, when applied in the various ways I have described, I think I am warranted in concluding that it has no power to produce any of the symptoms of hay-fever in my case. Moreover, unless it can be shown that the acid exists, in the grasses in which it is found, in combination with some base or some other body which renders it much more volatile than it is in the uncombined form, on theoretical grounds only we are forced to the conclusion that it cannot possibly be a cause of hay-fever, since the heat which is required to volatilise it is beyond anything which exists naturally in the atmosphere in any part of the world.

* In the case of one of those who was present with me the dense fumes of the acid brought on a slight cough with a feeling of suffocation, as if there was some little tendency to spasm of the glottis; but this passed rapidly off on going into the open air.

B. Experiments with Coumarin.

§ 87. This substance is an odoriferous principle found in the plants of several of the natural orders. It is one of those singular bodies which boil at high temperatures* only, and yet readily give off odorous vapours at ordinary atmospheric temperatures. Its formula is, according to the old notation, $C_{18}H_6O_4$. It is, as before stated, found in several of the grasses,† but it is most easily obtained from the Tonka bean‡ (*Coumarouma odorata*). A tincture made from the powdered bean in the proportion of one part, by weight, to ten parts of proof spirit gives a solution which has a strong odour of newly made hay.

The experiments with this substance were made, in my case, by placing ten drops of this tincture on a porcelain plate, and exposing this to evaporation in an apartment which was kept closed during the period of the experiment, except when I entered it or left it.

In about fifteen minutes after the tincture was exposed to the air the room was filled with a strong odour of newly made hay, and though the quantity of the solution was comparatively small, it was sufficient to cause the air of the room to be permeated with its characteristic odour for quite thirty-six hours. During this time I entered the apartment, and remained in it a couple of hours at a time several times, taking care now and then to move about, so as to inhale as vigorously as I should have done if I had been walking in the open air. Several other persons were present for different periods during the time named, but neither in their cases, nor yet in my own, was there any effect pro-

* Coumarin fuses at 122° , and boils at 518° , Fahr. (Miller).

† *Anthoxanthum odoratum*, *Holcus odoratus*, *Hierochloa borealis*, and one or two other grasses. It is also found in the *Myroxylon toluiferum*, *Melilotus cerulea*, *Melilotus officinalis*, and other species of *Melilotus* (*Leguminosæ*), also in the *Asperula odorata* (*Rubiaceæ*), in the *Prunus Mahaleb* (*Rosaceæ*) in the *Orchis fusca*, *Angrecum fragrans*, and *Nigritella alpina* (*Orchideæ*), and in the *Herniaria glabra* (*Portulacææ*).

‡ By some German authors Coumarin has been called "Tonka camphor."

duced beyond the perception of the somewhat agreeable odour of newly made hay.

§ 88. Three such experiments as that described above were made upon myself in different years, and at different times in each year, but in no one of them was there the slightest approach to any of the symptoms of hay-fever.

Four experiments with this same substance were tried upon patients Nos. 6 and 7 (§§ 69—70). The first of these trials was made upon patient No. 6 in 1870. With the exceptions that the apartment was not kept closed during the time the tincture was exposed, and the circumstance that the patient did not remain quite so long at one time in the room, the conditions were just the same as they had been in my own case. Another experiment, with exactly the same quantity of material, and under pretty much the same conditions, was tried in the early part of this year (1871). In both these cases there were several other persons, who never suffered from hay-fever, in the room during the whole time the tincture was exposed, but in no case were there any unpleasant symptoms produced. The patient, as well as several of the other persons present, very quickly detected the characteristic odour of the coumarin; but in no case were they aware of the object of the experiment, nor yet of the nature of the substance employed.

§ 89. Two experiments were also tried with patient No. 7, but under somewhat severe and more trying circumstances. In the first of these trials the quantity of tincture exposed for evaporation was the same as in the other cases, but in an hour after the experiment had been commenced the quantity was doubled. The vessel containing the tincture was, during a good part of the time, not more than four feet from the patient, so that the odour was very strong in her immediate neighbourhood. An important circumstance in this experiment was, that the patient remained in the same room night and day during the whole time the experiment was going on. In this case also the patient was not in the least aware of the object of the experiment, nor of the nature of the odoriferous agent used. No symp-

toms which could fairly be attributed to the presence of the coumarin were developed.

§ 90. The entire absence of results in this series of observations make it absolutely certain that in these cases, as well as in my own, coumarin has no power to produce any of the symptoms of catarrhus æstivus.

c. *Experiments with odours of various kinds.*

§ 91. In addition to the trials made with coumarin, I have also experimented upon the effects of many other volatile bodies, amongst which I may mention *Paraffin oil*, *Camphor*, *Oleum terebinthinæ*, *Oleum menthæ piperitæ*, *Oleum juniperi*, *Oleum rosmarinæ*, *Oleum lavendulæ*, &c. I have also tested the odours given off by the flowers and herbs of wild and cultivated plants, such as the *Chamomilla matricaria*, *Rosa canina*, and other species of roses; *Viola odorata*, *Lilium tigrinum*, *Lilium album*, *Cynoglossum*, and many other flowering plants which it would serve no purpose to enumerate here. I have also tried the odours given off by several of the fungi.

§ 92. The experiments with *Camphor* were tried much in the same way as those with coumarin, as was also those with some of the volatile oils. In all cases they were made sufficiently exact to permit me to say decisively whether any of them had the power of producing the symptoms of hay-fever. The experiments with the vapour of the oil of turpentine were more severe and extensive than those with any of the other volatile bodies; but this was not because I believed it was more capable than the other bodies of generating the specific symptoms I was seeking for, but because the opportunity for testing it somewhat extensively, without any trouble or inconvenience to myself, came in my way. The necessary conditions were, in fact, ready made to my hand at any time when I chose to avail myself of them. I had the opportunity of visiting an establishment where a considerable quantity of copal varnish was used. In a room set apart for the purpose, from one to two thousand superficial square feet of varnished paper were

often exposed at one time. Ordinarily copal varnish does not contain much oil of turpentine, but in this case it was the custom to add twenty or thirty per cent. of the oil in order to facilitate the working, and to help the drying. This, of course, was evaporated in the drying, and the atmosphere of the room was, as a consequence, highly charged with the vapour of the oil of turpentine. I have frequently entered the room, and have breathed the air for half an hour to an hour at a time. I have also taken the opportunity of doing this at the time I have been suffering from hay-fever, as well as when I have been quite free from it, but have not noticed any difference in the effect produced.

§ 93. The experiments with paraffin oil were tried under somewhat similar conditions to those last named, but were not so frequently repeated, nor yet could I say that the atmosphere of the room was so highly charged with the vapour of the oil as in the other case.

The volatile oils all produced head symptoms more or less severe in character, in some cases scarcely to be felt, but in other cases becoming rather unpleasant when long continued. The effects of the oil of turpentine were always the most marked, but this was probably owing to the fact that a much larger quantity of the vapour was inhaled than of any of the other substances. In no instance, however, were there any symptoms set up which in the least degree resembled those of hay-fever.

§ 94. The odour of the *Chamomilla matricaria* had a marked effect both upon myself and others. The plant had been gathered fresh in considerable quantity, and spread out in the room which we occupied as a dining room during one of our seaside visits, so that we inhaled the volatile principle given off pretty freely. Severe aching pain across the forehead, with nausea, dizziness, and pain at the epigastrium, were the principal symptoms, and these became so unpleasant on the second day after the plant had been placed in the room that I was glad to have it removed. There were, however, none of the symptoms of hay-fever produced.

§ 95. The inhalation of the odour of one of the microscopic fungi (*Chætomium*) also produced rather unpleasant symptoms

with me, but these were not at all like the symptoms of hay-fever. The spores of another of the microscopic fungi (*Penicillium glaucum*) I have reason to believe will, when brought into contact with the respiratory mucous membrane, generate symptoms not unlike those of hay-fever in some respects, but differing materially in others—being much more like those of ordinary influenza.*

The sensations caused by these two agents were so unpleasant that I have never cared to reproduce them. In the case of the first-named fungus I inhaled the odour given off by the plant, on two occasions, in an experimental way. In the case of the second I was the subject of an involuntary experiment, which gave me so much trouble and in-

* I had noticed many years ago that the dust from straw sometimes brought on attacks of sneezing with me, and that this seemed to occur more frequently when we had had a long spell of wet weather. I determined to try what fungi could be generated on damp straw. For this purpose wheat straw, slightly moistened, was placed in a closed vessel, and was kept at a temperature of 100° Fahr. In about twenty-four hours a small quantity of white mycelium was seen; this increased slowly for three or four days, and in a short time after was followed by the appearance of minute greenish-black spots dotted here and there along the surface of the broken straw, apparently coming out more readily on the inner than on the outer surface. This, I found on examination, was the *Penicillium glaucum*. After a few days another crop of dark-coloured spots were seen, but these became almost jet black, and had quite a different contour. These I found to be the *bristle mould* (*Chaetomium elatum*).

The spores of these two fungi were sown again separately on straw which had been placed on separate vessels after having been subjected to the action of boiling water for a short time. A separate crop of each fungus was thus obtained.

The odour of the *Penicillium* produced no perceptible effect upon me, but the odour of the *Chaetomium* brought on nausea, faintness, and giddiness on two separate occasions. By inhaling the spores of the *Penicillium*, in the involuntary experiment of which I have spoken, a severe attack of hoarseness, going on to complete aphonia, was brought on. This lasted for a couple of days, and ended in a sharpish attack of bronchial catarrh, which almost unfitted me for duty for a day or two.

This experiment would seem to some extent to agree with the observations of Dr. Salisbury, of America, who states that he has seen the mycelium generated on damp straw produce many of the symptoms of measles among the troops engaged in the American war (the specific rash amongst other symptoms). (Vide *American Journal of the Medical Sciences*, July, 1862.)

convenience that I have not wished, voluntarily, to subject myself to these again.

§ 96. The symptoms developed by the inhalation of some of the odours I have mentioned were sufficiently well-marked to make this a subject worthy of close investigation, but, as we have seen, it cannot be said that in any case the phenomena produced bore any resemblance to those of hay-fever. The action of the spores of *Penicillium* comes near to that of the exciting cause of this disease; but when the former comes to be thoroughly tested, I believe it will be found to produce symptoms of a much more acute and dangerous character.

From the results of these few experiments it is impossible to say whether the effects produced with the agents I have mentioned are due to idiosyncrasy merely; or whether the liability to be affected by them is due to a constitutional condition which is pretty widely spread, is not at present easy to decide. In the case of the vapour of those bodies which are allied to the hydro-carbons, the liability to be acted upon by these will probably be very widely spread; but in the case of the spores of fungi, it is possible that the phenomena they exhibit may be restricted to comparatively few individuals; but, as I have said above, it is a subject which calls for much more careful and extensive investigation than it has yet received.

D. *Experiments on the action of Ozone.*

§ 97. It is to M. Schönbein, of Bâsle, that we are indebted for some of the most important and earliest researches on ozone. Since the publication of his memoir on ozone, in 1840, the subject has received a considerable amount of attention; but we have not as yet obtained a test which can at all times, and under all circumstances, be relied upon as an unfailing indication of its presence.

It is now generally believed that ozone is only an *allotropic* condition of oxygen, but Andrews and Tait, who have experimented very extensively with it, at one time

thought it probable that it was a combination of oxygen and some other body.*

It is generated artificially in various ways, which I need not here stay to describe.

§ 98. M. Kosmann, of Strasbourg, made a number of experiments for the purpose of ascertaining the difference between the amount of ozone produced naturally in close proximity to growing plants, and away at a distance from any vegetation. From the results of these experiments he concludes that ozone is formed in greater abundance in the neighbourhood of growing plants than it is away from vegetation; but as his experiments were performed in the open air, without any apparatus to exclude the atmospheric ozone derived from other sources, his conclusions should be accepted with caution.

§ 99. An interesting and important series of observations were made by the late Dr. Daubeny, of Oxford.* He found that, in addition to the other known modes in which ozone is generated, it is also produced by the action of sunlight on the green parts of growing plants.

By the use of an ingenious apparatus which he invented, the air was made to pass over growing plants, after the former had been deprived of any ozone which it had in it, and Dr. Daubeny found that these give off or generate ozone, when exposed to the action of sunlight, but not always in the same ratio.

§ 100. Professor Mantegazza, of Lombardy, has also shown, by his experimental researches on ozone, that this substance is generated by the oxidation of the essential oils of many plants. It was produced in large quantity from the essences of mint, turpentine, cloves, lavender, bergamot, anise, juniper, lemon, nutmegs, &c., when solar light and atmospheric oxygen were allowed to act upon them. It was also found that these substances have the power of ozonising a large amount of oxygen even when they are present in comparatively small quantities—so small, indeed, that in some cases where a vessel had been perfumed with an

* Vide *Philosophical Transactions*, 1860.

† Vide *Journal of the Chemical Society*, January, 1867, pp. 1—28.

essence, and afterwards washed with alcohol and perfectly dried, it was found that if it retained the least trace of the odour of the essential oil with which it had been perfumed, it was still capable of developing ozone. Flowers which have a strong perfume were found to develop ozone in closed vessels, but those which were deficient in perfume developed it only in very small quantity or not at all. In most cases the direct rays of sunlight are required to generate ozone, but in some few diffused daylight is sufficient, but scarcely any of the essential oils will develop it in total darkness.

It is also very readily produced by the action of sulphuric acid on permanganate of potash, or peroxide of barium; and it is supposed that whenever oxygen is being disengaged from a base, ozone may be generated, but that this is not perceptible in all cases, inasmuch as it may be immediately taken up again by some other oxidizable substance present; but whatever is the source from which it is obtained it has much the same qualities.

§ 101. It is generally admitted that where there is a large amount of ozone in the atmosphere there—*cæteris paribus*—the best conditions for the enjoyment of health are to be had. Schönbein, however, found, during the course of his experiments, that when air highly charged with ozone was inhaled it brought on “a painful affection of the chest—a sort of asthma with a violent cough, which obliged him to discontinue, for a time, his investigations. Reflecting on this circumstance, he began to suspect that certain catarrhal disorders might be caused by atmospheric ozone. He got several physicians at Basle to compare their lists of catarrhal patients with his tables of atmosphero-ozonometric observations, and he and they were struck by the unusual number of catarrhal cases, on the days, or during the periods when M. Schönbein’s papers (test papers) showed that ozone was unusually abundant in the air.”*

From the results of these observations it would appear that ozone will, when present in considerable quantity,

* Watson’s *Principles and Practice of Physic*, 4th ed., pp. 47, 48.

produce symptoms which have some resemblance to those of catarrhus æstivus.

§ 102. It will have been seen, from what I have said before (§ 53), that I do not conceive it to be possible, for ozone, in the quantity in which it is ordinarily found in the atmosphere, to bring on hay-fever. It would therefore seem to be inconsistent in me to be seeking for an effect which I believe cannot be produced. Whilst, however, I hold that we have abundant evidence to show that, in the quantity in which it is ordinarily met with, ozone will not produce hay-fever; we have little or no evidence to show that it may not do this when present in a larger quantity.

In this disorder, as in many others—and even in some of the so-called zymotic diseases—the *quantity* of the exciting cause may have almost as much influence in determining the occurrence of an attack as the *quality* has, and as I was investigating the effects of *all* the presumed causes of hay-fever, it seemed in this case also to be necessary to show by actual experiment that ozone has no such effect as that which it has been supposed to be capable of, even when present in what may be called maximum quantities, such as are often found on or near the sea.

§ 103. For the purpose of determining this question I instituted a series of experiments on the action of this subtle agent, and, on account of the important bearing the subject has upon the study of other diseases of the respiratory organs, I decided upon making an effort to ascertain the relative amount of ozone present at various points of the scale.*

* The first test papers tried—made on Schönbein's method—were procured from one of the London makers. These yielded very unsatisfactory results. If half a dozen slips were exposed for a given time, and placed under exactly the same influences, I very rarely found that these slips were of the same tint at the termination of the experiment; and frequently it happened that no two of them would be alike. Another trial which I made with test paper which I prepared myself, according to Schönbein's method, was more satisfactory, but still was far from being as exact as was deemed to be necessary in experiments of this kind.

I do not know what may be the experience of meteorologists in this matter, but it would appear that that of the late Dr. Daubeny was in some respects

The first experiments were tried at Grange, on the north-western shore of Morecambe Bay, Lancashire, during the latter end of August and beginning of September, 1865. Schönbein's scale and test papers were used, but I was at first not very successful with them. As I was merely testing for the effects of ozone upon the respiratory organs, and for the purpose of ascertaining the amount of ozone present, I did not pay much attention to barometrical indications, to hygrometric conditions, or to temperature.

I was quite free from any sign of hay-fever at the time.

As I have hinted above, I was not then very successful in my attempts at ascertaining what *relation* the amount of ozone indicated by any point of the scale bore to that of any other point; but I think I was able to decide that at the highest point of Schönbein's scale (10°) it had not, in my case, the slightest perceptible effect upon the respiratory organs.

§ 104. The papers were exposed in a garden overhanging the shore and running down close to high-water mark, so that when the tide was in they were within about thirty yards of the water and fully fifteen yards from any building. In this situation the slips of test paper were fairly exposed to the full force of the sea breezes, but care was taken to shelter them from the direct rays of the sun.

Twelve experiments were tried on six different days, similar to my own. In the concluding remarks in the paper to which I have already alluded he says—"I cannot rely upon different samples of either paper (Schönbein's or Moffat's) yielding, under the same circumstances, exactly similar results, and, therefore, am loath to confide in their indications as furnishing corresponding measurements."

By adopting a different method in preparing the test paper, I have obtained results which are much more uniform; and by constructing a scale on a given principle I have been able to make some approach to accuracy in determining the *relative* amount of ozone in the atmosphere, as shown at various points on the scale.

It would be out of place here to attempt to give the details of this method, but I hope to have the opportunity of doing so at another time and in another place; but I may here state that, according to the method I have used, I find that if the first perceptible effect on a slip of test paper be taken as *unit*, it will require about 715 times the quantity of ozone to produce the highest effect on the scale.

commencing on August 27th, and terminating on September 2nd. The papers were exposed for twelve hours at a time, viz., one set from 9 a.m. to 9 p.m., and another set from 9 p.m. to 9 a.m. the following morning. The periods were arranged in this manner in order to give an opportunity for my being in the open air during the greater part of one of the periods, and also for the purpose of comparing the amount of ozone registered during the day and the night.

I was present during the time of the experiments four days out of the six, and, except during meal times and sleeping hours, was pretty constantly in the open air and for the most part on the sea shore close to the water's edge.

§ 105. The total quantity of ozone registered in the twelve experiments was 93° (Schönbein), the mean for twelve hours, being 7.75° . The highest point attained in this series of observations, in twelve hours, was 9° , and the lowest 6° , and from a comparison of the total quantity registered during the nights and the days it was found that there was scarcely any difference between the two. The total amount registered during the six days was 47° , and the total amount for the six nights was 46° .

The average amount of ozone present was comparatively large, especially if we take into account that the period of exposure was only twelve hours, but it must not, however, be supposed that because twelve hours gives a mean of 7.75 , an exposure of twenty-four hours would give double that amount on a scale. My experiments have shown unmistakeably that it requires a vastly greater amount of ozone to increase the depth of the colour in a slip of iodised paper from 9° to 10° , than it does to change it from 1° to 2° ; or, in other words, the higher we go in the scale the greater is the amount of ozone needed in moving from one degree to another. A similar set of experiments was made in the same spot in 1866, with much the same results.

§ 106. Another set of experiments was made at Southport, on the Lancashire coast, during the months of February and March, 1866. Here, however, I was not able to pursue the inquiry in so systematic a manner as at

Grange, in consequence of my stay being much shorter at each visit, the longest period being not more than thirty-six hours. This was, however, generally sufficient for me to take an observation, and to enable me to note the effect, if any had been produced, upon me, when there was a large quantity of ozone present.

The amount was generally large when a sea breeze was blowing, and almost invariably low when a land wind prevailed. The highest point attained several times exceeded the highest degree ($= 10^{\circ}$) on Schönbein's scale; and when this happened to be the case the wind was generally very strong.* On such occasions I found that ozone could be detected in close proximity to the backs of the houses facing the main street of the town. The farther I went inland the longer time did it take to produce a given effect on the test paper, and, as a matter of course, the nearer I approached to the sea the more rapid was the effect, providing no building intervened. The maximum effect was got at the end of the pier when the tide was coming in, and when a steady and tolerably strong breeze was blowing in from the sea.

§ 107. A few observations were also made at Blackpool during the latter part of October, 1869, but those which I consider to be the most valuable and the most conclusive were made at Filey Bay, on the coast of Yorkshire,† in the month of July, 1870. Here we have an expanse of sea from three to four hundred miles in a straight line, so that when a sea breeze is blowing, it has a long journey to make without touching land; consequently, whatever action the ocean may exercise in generating ozone, we may expect to have the full extent of this action exhibited here.

§ 108. On many days during my stay at Filey, the tem-

* It has often occurred to me that all ozone observations ought to be combined with the use of an anemometer, so as to be able to make due allowance for the varying rates of the wind.

† About seven miles south of Scarborough. Apart from scientific considerations it is a pleasant and quiet spot to spend a summer holiday at, but for the archæologist, the geologist, or the botanist, it is a neighbourhood which is replete with matters of interest.

perature was very high, so that the place and the season were favorable for observing the effect of heat as well as that of ozone; and, to use the words of Bostock, "the situation might have been chosen for the purpose of experiment" (which, indeed, it had been, so far as ascertaining the quantity and effects of ozone were concerned); for I also found the place favorable for determining other questions connected with the study of hay-fever, in relation to which a geographical position of a certain character, such as we find here, is absolutely needed before any approach can be made towards deciding these questions.

§ 109. A glance at the map of Yorkshire will show a narrow strip of headland* to the left of Filey Bay, which runs out to seaward, half a mile or so in length. This forms the northern boundary of the bay. At the extreme point of this headland is a low reef of rocks,† which are left high and dry when the tide is out, but which are for the most part covered with water when the tide is in, and especially at spring tides.

This was an excellent spot for experimentation, and was a favorite resort during my stay. I found here a large amount of ozone at all times—larger than at any other place I had visited—but this was no doubt in part owing to the character and position of the spot selected for the experiments, and also in some measure to the force and direction of the wind during the time these were in progress. It several times happened that five or six hours' exposure would produce a depth of colour on the test papers equal to 7° (Schönbein), and in some cases, when the test was exposed for twenty-four hours, the colour was beyond the highest point (10°) in Schönbein's scale.

§ 110. One experiment was tried on the water, the test paper being a portion of the time three or four miles away from the shore. In two hours this gave a colour equal to 5°, but in a test paper exposure for the same time outside the house we occupied in one of the streets of the town (at right angles with the shore) the test only reached 3°, although the wind was blowing in from the sea all the

* "The Car Naze."

† "The Brig," or Bridge.

time. It is scarcely necessary to say that I was present during the time when many of the experiments were in progress, and frequently it happened that I was at the extreme point of the reef of rocks I have named for several hours at a time, so as to place myself fairly under the influence of the abundant supply of ozone found at this place.

§ 111. I have previously mentioned (§ 97) that ozone is formed by permitting sulphuric acid to act upon permanganate of potash (or, according to the modern nomenclature, potassium permanganate). For any experiment in which it is desirable to try the action of this body upon the respiratory organs, with some degree of certainty that we have no other disturbing influence present, this is one of the best means of generating it in a ready manner. A comparatively small quantity of the two agents named, if placed together in a jar or wide-necked glass bottle, will continue to give off ozone* for several hours, and at the commencement of the experiment will give off sufficient to colour a test-paper, placed over the mouth of the jar, up to 6° or 7° (Schönbein) in a couple of hours.

Several experiments were tried on the effect of ozone generated in this manner. The gas was inhaled as it formed, and the odour denoted that the quantity was, for the space it occupied, much larger than in any other experiment I have ever tried, except, perhaps, where a current of electric fluid is thrown silently on the mucous membrane of the nostrils.†

Whilst describing these experiments, I have purposely refrained from giving the results in any case, for the reason that the same statement will serve for all.

From the details and the dates I have given the reader will have seen that the observations were made at all times of the year; in the autumn at Grange, when the hay season

* And also oxygen in its ordinary state.

† This is easily done by having a sharply pointed wire connected with the prime conductor of an electrical machine. If the wire is insulated by cementing a piece of glass tubing over it, it may be held in the hand of the operator and the current of electricity will pass off silently, in the form of a luminous brush or cone, which has its apex at the point of the wire, whenever the machine is put in operation.

was quite over, and at a time when I scarcely ever had any of the symptoms of the disorder lingering about me; in the winter and spring at Southport, at a time of the year when I never remember the disease troubling me; and at Filey, in the middle of the summer, when I was still suffering a little from hay-fever. In not one of these trials with atmospheric ozone could I say that it had any unpleasant influence upon me. In the experiments made with that which was generated artificially the only effect produced was a slight sense of dryness in the throat, but there were not any symptoms of hay-fever set up, and I cannot say that at any time my experience corresponded with that of M. Schönbein. Perhaps it might be that in my case a much less quantity of ozone was inhaled than in M. Schönbein's; nevertheless, I am satisfied that I inhaled a larger quantity, at times, than is ever met with in the atmosphere in the same volume of air, and I think it is fair to conclude that it cannot at any time bring on this curious disorder with me.

§ 112. In addition to the experiments already detailed, two different sets of observations were made for me by patients who crossed the ocean to Australia in the latter part of 1866 and the early part of 1867.*

The variations of temperature and other meteorological conditions were also noted, but as these have no direct bearing upon the subject we are considering, I shall not specially refer to them, but shall merely give such facts as relate to the quantity of ozone observed on each voyage, and to some of the circumstances which seemed to influence this. It will be well to observe here that the numbers will not always exactly agree with Schönbein's scale, as the test-paper was not made in the same way as his, or with the same proportions of the re-agents. Nevertheless, the

* Neither of these patients was affected with hay-fever, and the experiments cannot, therefore, have any direct bearing upon my own case; but as ozone has been so often mentioned as a possible cause of the disease, it seemed to be an opportunity for trying what quantity is met with on the ocean, which ought not to be let pass without endeavouring to glean a little information, and thus to throw some fresh light on those very rare cases of the disease which are said to occur whilst the patients are on the sea.

numbers given will suffice to give a tolerably fair idea of the amount of ozone met with.

§ 113. The first patient went on board the vessel he sailed in on November 6th, 1866. He commenced his observations on the 20th Nov. Ninety-two observations were taken during the voyage. The test-papers were exposed for twelve hours at a time, viz. from 10 p.m. to 10 a.m. on the following day. The first observation was made on Nov. 20th, when the vessel was about one hundred miles from Lizard Point, and the last one on Feb. 20th, 1867, when off Cape Otway (Victoria). The total amount of ozone registered during the voyage was 572° , or a mean of 6.2173° for each twelve hours. The highest amount was 10° , and the lowest 3° . During the whole of the experiments ozone was never absent.

As in my own observations at Grange and elsewhere the time of exposure—twelve hours—should be taken into account in estimating the quantity of ozone met with. When this fact is borne in mind the figures show that the amount registered is comparatively large.*

§ 114. The second patient sailed from Plymouth on November 22nd, 1866. One hundred and twelve observations were taken, commencing on November 28th, 1866, and terminating on February 4th, 1867. The result of these experiments agrees in the main with those given above, and particularly in the circumstance that the quantity of ozone

* In comparing the amounts registered in different directions of the wind, some rather curious results were brought out; the two highest mean quantities obtained being with the wind at opposite points of the compass, viz. from S.S.E. and N.N.W. The observations made while the wind was blowing from the first-named quarter gave a mean of 8° , while those made whilst it was blowing from the opposite quarter gave a mean of 7.6° . With the wind from the N.W. the mean was 7.18° , and whilst blowing from the S.E. it was 7° . This sort of balancing or compensating action seemed also in some degree to hold good in the lower numbers; as, for instance, in those with the wind from the N.N.E. where the mean was 5.4° , whilst in those with the wind from S.S.W. it was 5.6° .

Whilst, however, the above results were brought out by comparing *sections* of the observations, a comparison of the whole experiments did not seem to show that in any one particular direction of the wind there was a larger amount of ozone present than in others.

registered on damp or wet days was always much greater than on dry and fine days.* As in the other case, also, ozone was never found to be entirely absent.

§ 115. These experiments show pretty conclusively that if atmospheric ozone is a cause of hay-fever in any case, the disorder ought to show itself more or less frequently whenever the patient ventures near the sea, and as ozone seems to be always present on the open sea, the disease should never be absent whenever he is at a sufficient distance from land to be free from the influence of land breezes. We ought also to expect that if the disease is in any case produced by ozone it should vary in intensity with the rise or fall in the quantity of this agent. How far this is shown to be the case I shall, in reviewing the testimony of other authors, have to consider further on.

E. Experiments with dust.

§ 116. In speaking of dust as a cause of hay-fever most authors have used the designation "common dust." In the strict sense of the term, however, there is no such thing as common dust. A careful examination of the dust of any district will show that, in addition to those matters which may with propriety have the name "common" applied to them, it contains ingredients to which this cannot be applied, and the nature of which will to a large extent depend upon the season, upon the geological character of the district, and upon the nature of its botanical productions. The *number* as well as the *kind* of organic germs found in the dust of any district will also largely depend upon the

* In the first set of experiments forty-five damp or wet days gave a total of 332·5°, whilst forty-seven dry days gave a total of 239·5°. During the first-named period the wind came from almost all points of the compass, with perhaps, on the whole, a slight bias to the south; whilst in the last-named period the wind was quite as variable, but came a little more from the north or north-east than from any other quarter.

For an interesting account of experiments on the relative amounts of ozone with various directions of the wind on land, I must refer my readers to the paper, by the late Dr. Daubeny, which I have already mentioned (*vide* note to § 99).

meteorological conditions which prevail in that district, so far as regards heat and moisture.

When I come to give an account of experiments tried for the purpose of obtaining information on this point I shall, I think, have abundant evidence to show that the statements made above are, for the most part, borne out by the results of a long course of observation. For the present I shall content myself with mentioning, and with offering one or two remarks on, the incident which first drew my attention specially to this phase of the question.

§ 117. I have several times noticed that dust could at certain times of the year produce some of the milder and less marked symptoms of hay-fever, but there was this peculiarity about these attacks, that generally they came on only during the time that hay-fever prevailed (and then as exacerbations) or immediately after the hay season was over, but never during winter or early spring.*

There was also another peculiarity which these attacks had, namely, that they were more fitful and more ephemeral, coming and going in a more irregular and transitory manner than the ordinary attacks of the disease ever do when they have once set in. At first I was considerably puzzled and was unable to account for the fitful appearance and departure of the symptoms. I also noticed that the attacks were more frequent whenever I had to pass through any dusty lane in the country, even when the hay had been all gathered in. I was consequently inclined to think, as Dr. Phœbus and others have since thought, that common dust was one of the causes of the disorder.

§ 118. In one of the earlier years of my attacks,† when I was just getting free from the disease, about the middle of July, I was out in the country and had to walk through a lane which was apparently not often used for the passage of vehicles. A carriage which passed me at a rapid rate raised a cloud of dust in which I was, for a time, completely

* With some exceptions, which I shall name hereafter.

† As I had not at that time made up my mind to follow out any systematic course of observations on the subject, I made no notes of the occurrence, and cannot now remember the exact year.

enveloped and compelled to inhale pretty freely before I could get out of it. A very violent attack of sneezing immediately came on and continued at intervals for about an hour. As I had to pass over the same road on the following day, I determined to see if the same result would follow by disturbing the dust voluntarily. I found that I could bring on the symptoms in this way to the fullest degree of severity.

§ 119. The first examination of the dust under the microscope, and which was made with that which had been scraped from the road, did not show anything very special. A second examination of the upper layer of dust* was more successful, and revealed to me the presence of bodies which I now easily recognise as the pollen grains of the grasses.

So far as I can now recollect, the weather during this season had been very favorable for the rapid growth and flowering of grasses—first a few hours of rain then a day of sunshine—and when this got to be nearly ready for cutting, and before the period of flowering was gone by, the weather had settled down so as to give three or four weeks without any rain.

§ 120. With the help of subsequent experience it is not difficult to see why such a season as I have described should have given rise to a condition of things which would quite account for the symptoms from which I suffered, but it must be confessed that it is not easy to say why I had the attack more severely in this particular spot than I had in any other under similar circumstances. Perhaps if I had then been as well acquainted as I now am with all the various channels by which the *cause* may reach a patient, in out-of-the-way places and, as it were, at out-of-the-way times, I might have been able to explain the matter. At

* This was got by coating an ordinary microscopic slide with a film of glycerine, and by pressing this upon a layer of dust, which seemed not to have been previously disturbed for some days. A thin layer was in that way taken up. In the first examination the dust was placed on the slide in a dry state, but in the second it had a small additional quantity of glycerine added to it, and a glass cover was placed over it. This was probably the principal cause of my getting so much better results in the second examination.

that time, however, I could only do as some writers on hay-fever have since done—speculate on the causes of the phenomena.

§ 121. Before I conclude my remarks on this part of the subject I must allude to one circumstance which has several times attracted my attention, and which will serve as one example among many others which might be given of the accidental and apparently causeless manner in which the symptoms of the disease may be developed. It has several times happened that an attack of the malady has followed a ride in a railway carriage even when the train has been going into a part of the country where the hay-making has been finished, and where, so far as could be seen, there could not be any possibility of the attacks being caused by hay or by grass in flower. I am now satisfied, however, that these attacks were generally brought on by getting into carriages which had come from places much more north than Manchester, and where the hay-making is much later. For a similar reason I think the disorder may be brought on earlier than usual by a patient travelling in and inhaling the dust in carriages which have come from parts of the country where the grass ripens earlier than it does in the part where the patient may live. That this is not a matter of mere speculation my own experience abundantly proves.

The incident I have related above along with the other circumstances alluded to caused me to decide upon investigating the matter as fully as my opportunities would permit, but before I could do so satisfactorily it seemed necessary to make some experiments upon the action of pollen on the respiratory organs.

(To be continued.)

FORCE, PROTOPLASM, AND STIMULUS.

By Dr. DRYSDALE.

*Continued from Vol. XXVIII, p. 654.*CHAP. III.—ON THE NATURE OF LIFE (*continued*).

§ 66. But the question of the stimulus is far from being so simple. On the contrary, it involves the previous discrimination of the action of external agents upon dead from that upon living matter.

Great as was the genius of John Brown in emancipating his mind in 1780 from the all-prevalent notions of life as a substantial entity, and raising the stimuli to the position of an essential part in what he showed to be an action or process ; yet he did not and could not penetrate the whole truth as to their operation. And when the system of medicine into which his theory had been prematurely elevated was exploded, the fundamental truths contained in his doctrines fell into discredit, and they have remained forgotten or undervalued, especially by physiologists. Fletcher, however, was fully penetrated by them, and never fails to keep them in the foreground, in both his physiology and pathology. To practical physicians, both in respect to hygiene and therapeutics, the stimuli are of the greatest importance as the chief agencies we have to any extent under our control for health or disease. Nevertheless, how little is known of the nature of their action upon living matter, as distinguished from the operation of the same substances on dead matter of apparently the same chemical composition ! Although we are so familiar with the facts that we scarcely think any explanation is required, who can explain the action of a grain of sand upon the eye, or any mechanical irritant whose weight is insignificant and whose chemical composition is absolutely unaltered, while it may excite frightful inflammation and ultimately death of the part or even of the whole body ? What force has been communicated equivalent to all that effect ? Or when a

mere drop of serpent poison, hardly distinguishable by chemical tests from mucilage, in a few minutes or hours alters the whole blood, and penetrates the vital powers even to death; to what change of the living matter the least like any known chemical change can that be attributed? In the time of John Brown it might have been a great step to refer these phenomena to the exhaustion of excitability from excess of the other factor of life, viz., stimulus. But the demands of science are greater now, and since we accept the principle that the phenomena of life are the product simply of the properties already potentially existing in unorganized matter and the forces, we are bound to show an analogy, at least, between vital and ordinary chemical action, in respect to the stimuli as well as other agents. Except as regards the nature of force, the fundamental principles of chemistry were much the same as now when Fletcher wrote: in what sense, then, could he still define life to be an action depending on two factors, viz., irritability on the one hand, and stimuli on the other? In the first place although the problem is generally so stated for practical reasons, the matter is much more complicated, and the more full definition of irritability is given as follows: "Vitality or irritability is the property which characterises organized beings of being acted on by certain powers otherwise than strictly mechanically or strictly chemically."* Thus, we

* As precision is of essential importance, I give here Fletcher's glossary of the chief terms used by him:—

"*Organ*.—A part of a plant or animal more or less distinct from the rest, and destined to perform, either alone or in conjunction with others, some specific function.

"*Organisation*.—The *process* by which a being possessed of organs is formed (*e.g.* the formation of a germ).

"*Organized*.—Possessed of such structure.

"*Organogeny*.—The process by which the several organs become perfectly distinct from each other (*e.g.* the development of a germ).

"*Organic*.—Appertaining to organized beings.

"*Vitality* or *Irritability*.—The property which characterises organized beings of being acted on by certain powers otherwise than either strictly mechanically or strictly chemically.

"*Life*.—The sum of the actions of organized beings, resulting directly from their vitality so acted on." (p. 2.)

have irritability on the one hand, and all other agents which act upon it on the other, which may be comprised under pabulum, conditions, and stimuli; and, as many agents can act in two or all three of these capacities, the problem of analysing their influence becomes very complicated. The category of conditions is brought in because there are agencies, such as moisture and temperature, which may be neither stimuli nor pabulum, but, on the other hand, they may stand in the same relation to vital as to chemical or mechanical action. In that case the only specific relations, as before said, in which external agents stand to living matter will be as stimulus and pabulum.

Likewise, as vitality is merely a property developed by the peculiar combination of the elements of the irritable matter, and is not dependent on the addition of any new principle, essence, force, or power, added to matter, its nature and laws must be in a wide sense chemical or physical, and Fletcher must be counted as one of the upholders of the physical theory of life as opposed to the vital principle school. Yet from his conception of the utterly peculiar nature of the living matter, as already explained, he must be looked on as one of the most extreme and strict of vitalists, and allowed to use the word vital as implying something totally distinct from chemical. We may, if we please, adopt the word metabolic,* or that of vito-chemical, to express this

* This word was invented by Schwann, and as it is adopted by Sharpey, and thus become a current physiological term in all English-speaking countries, I may be allowed to use it without affectation of novelty in words. But some words of explanation are needed, as I shall use it to express Fletcher's idea of the constitution of matter in the living state. In introducing this word, Schwann says, respecting the formation of cells, "Those phenomena may be arranged into two natural groups: first, those which relate to the combination of the molecules to form a cell, and which may be denominated the *plastic* phenomena of the cells; secondly, those which result from chemical changes either in the component parts of the cell itself, or in the surrounding cytoblastema, and which may be called *metabolic* phenomena (τὸ μεταβολικόν), implying that which is liable to occasion or suffer change." (Schwann and Schleiden, p. 198).

These two powers are inseparably connected, for the plastic or formative power, which causes formation and growth of peculiar matters, "presupposes another faculty of the cells. The cytoblastema in which the cells are forme

relation to ordinary chemical action, but, at any rate, we must claim the right to speak of vital, on the one hand, and chemical on the other, as practically distinct without continual explanation.

§ 67. A hypothesis such as Fletcher's is, in fact, essential if we expect to stand against the arguments of the vital-principlists, because sooner or later on the ordinary physical theory of life you come to a point where all similarity to chemical action fails. And that point is very soon reached, for it is the distinguishing mark of chemical combination, that in all cases each of the agents disappears, and a product with new properties is formed. In the combination of a metal both the metal and the oxygen disappear, and the oxide is found in their place. Moreover, they disappear in an exact proportion; an acid is neutralised by an alkali; both disappear in exact proportion, and a new

contains the elements of the materials of which the cell is composed, but in other combinations; it is not a mere solution of cell material, but it contains only certain organic substances in solution. The cells, therefore, not only attract materials from out of the cytotlastema, but they must have the faculty of producing chemical changes in its constituent particles. Besides which, all the parts of the cell itself may be chemically altered during the process of its vegetation. The unknown cause of all these phenomena which we comprise under the term metabolic phenomena of the cells, we will denominate the *metabolic power*" (p. 197).

The word plastic was originally Aristotle's, and was applied by him to that power which determines the association of the component parts of organs during the development of each, according to the harmonious laws we see in operation. With him it therefore meant originally the germinative or germinal power. The same power, after receiving many names, has been extended to the process whereby an old part is repaired, or a new part formed in after life. Fletcher calls "plastic" that power which, as well as giving form to renewal of its own tissues, elaborates the germ into the complete individual (II, 23), while at the same time he says these are all merely modifications of vital action. So, under the word plastic are included the varieties plastic and germinative. We have thus three principal varieties of faculty in vital action, which, though all inseparable from life as a whole, yet may be conveniently distinguished. I shall here use the word metabolic to express the changes of matter which occur in the living state, and also speak of metabolic affinity, metabolic change, metabolic action, the metabolic state, &c., as contrasted with the analogous but totally distinct adjective chemical as applied to those substantives. The words plastic and germinative or germinal stand for themselves alone as there is nothing analogous to them in nature.

substance—the salt—is formed. On the other hand, when living matter is placed in contact with a chemical compound fit for pabulum, the latter is certainly decomposed, and a variety of products make their appearance, but the former—the agent of all these changes—is not consumed in the process; on the contrary, unlike catalytic agents, it even grows, and that not by accretion from the deposition of similar matters from solution around it like a crystal, but from within outwards by the decomposition of nitrogeneous matters and the selection of the elements it requires; more than that, it reproduces similar forms, and even develops itself into different forms, which have different powers of chemical change of the pabulum; in short, it displays the marvellous powers of a living being which it would take volumes to describe.

§ 68. No attempt to explain this complete antithesis between chemical and vital action by the influence of any known physical force, or any imaginable force correlative with the known forces, upon the albumenoids or other proximate principles, has had even the shadow of plausibility. Nor do the truly philosophical among physiologists, who hold what is called the physical theory of life, rest their conviction on the sufficiency of any of these attempts, but they look forward to the time when future discoveries will clear up the darkness in which the subject is shrouded, and in the meantime they reject the teleological view of a principle, essence, or power added to the proximate principles when organized and withdrawn at death which can enable them to perform all the wonderful functions of life, as inconsistent with sound philosophy, and calculated to stop all progress in science. The hypothesis of Fletcher—viz. that this living matter is a compound wholly different from, although isomeric with, the *sum* of the proximate principles contained in it after death, and in virtue of that peculiar mode of combination possessing the property of vitality—it is, doubtless, impossible to submit to the test of direct experiment, but it must rest on deductive reasoning and collateral evidence. For it is not that no test has been as yet found which will react with albumen as with living matter, and

vice versa, but no such test can possibly be found.* All tests which act chemically must react by double decomposition and mutual combination with the living matter, an action which we have just said never takes place with the living matter till it has been destroyed by excess of its own peculiar action. Chemical tests can, therefore, teach us no more than destructive distillation or any mode of destructive analysis. The true tests of the nature of the living matter are the reactions it displays with external agents in the form of pabulum and stimuli, and these reactions are nothing else than *life itself*. But as these reactions can only take place within narrow limits of conditions, and as the reproduction of the living matter itself is one of the chief functions of these very reactions, it follows that comparatively small variations of the conditions, or of the quality or quantity of the stimuli or pabulum, will produce destruction of the living matter, *i. e.* death and resolution of its elements into the proximate principles. It seems, therefore, to require an effort of the imagination to conceive that the substances which we now easily recognise to be albumen, myosin, and the like, are not the same which we saw a few minutes before in active life, and which we ceased not to watch during the transition. And it seems to require no effort of the imagination to perceive that, if they be the same, some essence, principle, or influence must now be lost, which enabled them to resist all their natural affinities, and gave them totally new powers. But does not the argument from imagination tell

* We shall frequently have occasion to notice the coincidence between the opinions and principles held and taught by Fletcher before 1836, and those beginning to prevail in the present day; but in no case is it so remarkable as with respect to the doctrines of Dr. Lionel S. Beale, who, by his histological and physiological discoveries, has realised several of the hypotheses of Fletcher in a surprising manner, as we shall see. In Beale's *Todd and Bowman*, 1866, we find, "It is, perhaps, as impossible to conceive a *living chemical compound* as it is to conceive a *living elementary atom*" (p. 103). "The chemical composition of living matter is unknown, and probably unknowable, for it is impossible to analyse matter that is living. To assert, therefore, that living matter is 'protein' or 'albumen,' is to assert that which never has been and never can be proved, and all arguments based upon such assertions must be discarded." (P. 65, Beale *On Life Theories and Religious Thought*, 1871).

really on the other side? Is it not simply in accordance with common sense to deny the name of albumen to a substance which answers to none of its tests, and which can do a hundred things of which albumen is notoriously incapable? And does any one now look for the escape of any active principle when a chemical process comes to a stand, as in the days when Phlogiston or the matter of fire was held to be a substance?

§ 69. The distinction of the state of the living matter from that of the proximate principles is so important, that at the risk of repetition some further statement of Fletcher's views must be given in addition to what was said at §§ 63 and 64. It might be said there is nothing peculiar in them, as it was often said that organic chemistry was very defective, and that many of the so-called principles were not educts but products, and, therefore, gave no true information as to the state of combination in the living body. This is certainly true in a sense, but by none was the distinction made so absolute, and above all, was it held, as by him, to be the sole cause of vitality. The greater number, as Chaptal, Prout, Thomson, Murray, Henry, Mayo, &c., held that chemical laws were still in force, although guided to produce compounds impossible out of the body, by the "vital principle," "organic agent," "superior agent," "directing principle," &c. And Henry expressly states that in dead animal matter the proximate principles are in a state identical with that in which they exist in the living structure. Against these views Fletcher argues that a power of such a nature must be entirely *sui generis*, and asks, "Is it conceivable that a power confessedly *different* from common chemical affinity should, while it continues to exercise a paramount influence on any substance, maintain it in a state *identical* with that of a common chemical compound" (p. 185.)

Among other expressions respecting the nature of vital affinity, he says, "it does not combine them [the elements] at all in the chemical sense of the word, but merely holds them associated together in an ever varying form of exist-

ence, of which the chemists seem to have no idea" (p. 135).*

He thinks that "the process of secretion, by which the ultimate ingredients of all vegetable and animal compounds, whatever they may be, are brought together, is, perhaps, an infinitely more subtle and searching power than that of common chemical affinity; and that there exists between the two only a very faint and remote analogy" (p. 140.) Nevertheless, it is not of a nature quite foreign to the properties of matter as the vital-principilists maintain, for he says (p. 136), "that the proximate principles are the results of the combination of certain elements effected by secretion as a process, although not identical with, still analogous to common chemical affinity."

Now, we must remember that the living matter owes its powers entirely to its ingredients, and the mode of combination in which they exist in it; therefore, as different living parts of the same animal and different animals and plants vary in their vital qualities, and even the same part varies at different times, we must expect the proximate principles into which it is resolved at death to vary in proportion and even in number. Likewise, as will be noticed further on, a large proportion of the bulk of organized beings is not really living, but consists of chemical compounds as we find them after death, and we have thus always before us a mixture of dead and living matter; so the problem to be solved by organic chemistry, viz. which are the proximate principles or the chemical compounds if not actually composing it, yet nearest to the living matter? is an extremely difficult and complex one. Accordingly, we find great difference of opinion to prevail among chemists as to the constitution and atomic weights of the albuminous matter. Nor is the analysis of any organ, tissue, or part, anything

* "Of the relations which these elements bear to one another in the living matter, we know indeed nothing; but since every kind of living matter exhibits the same characters, it seems probable that during this temporary living state the elements do not exist in a state of ordinary chemical combination at all. Their ordinary attractions or affinities seem to be suspended for the time." (*Beale's Todd and Bowman*, 1866).

like complete, and we always find a percentage of "extractives" and other little known matters. Great as has been the progress of organic chemistry since Fletcher's time in 1836, not the slightest step has been made towards filling up the gap between the chemical proximate principles and the living matter. A glance at the chief points in the present state of organic chemistry will illustrate this and speak strongly in favour of Fletcher's position.

§ 70. "By the term proximate principle is to be understood a vegetable or animal compound formed by the union of certain elements, characterised by distinct chemical properties, and by its union again with others contributing to form the several tissues and fluids of organized beings." By this definition we must be prepared to admit, and afterwards we shall admit, that a great portion of the actual substances composing the body while living are merely chemical compounds, hence the question comes to be whether there are any histological elements whose composition is such that life may be manifested in them while still cognisable as chemical compounds? We may, therefore, pass by the great bulk of binary and ternary compounds with the remark that they are certainly what they seem, and, although not much in that direction has been achieved by art as yet, we can conceive it possible that they may be all compounded in the laboratory chiefly by oxidation from the more complex organic bodies from which they are derived in the living being, although by a very different process indeed.

Some also may now be built up synthetically in the laboratory from the ultimate elements themselves, and, in addition to stearin and urea, which were made in this way before 1836, we have now cyanide of potassium from charcoal, nitrogen, and potassa, and this, when decomposed by water, yields ammonia. Allantoin, formic, oxalic, glycolic and many organic acids, and various other substances generally derived from the organic kingdom, have been produced artificially from the inorganic elements. And the numbers that have been produced artificially from other organic bodies is so great that it would be tedious to enumerate them. In respect to these, however, it is remarked

by chemical physiologists at the present day that they all belong to the secondary organic compounds, such as urea, uric acid, hippuric acid, &c., which are pretty far down in the scale of destruction by oxidation of more complex matters. It is also remarked that in the long list of intermediate products which intervene in the living animal between its complex food and the simple excreta, viz., carbonic acid, water, and urea, the composition of the majority of these is only very imperfectly known even now, and, in fact, can only be stated as known with tolerable certainty as they approach to the simple excreta. We may thus pass over the secondary organic compounds, such as the long list of fatty acids in the vegetable and animal kingdoms: the glycolic, lactic, oxalic, and other acids, the whole carbo-hydrates—grape and milk sugar, inosit, and glycogen—the neutral fats, the amid substances, as urea, glycin, leucin, cystin, tannin, xanthin, kreatin, and the pigments. All these are derived from the far more complicated group which may alone fix our attention in this question, viz., the albuminates, comprising albumen, globulin, fibrin, and myosin (with syntonin); the albumenoids, namely, mucin, gelatin, chondrin, keratin, elastin, fibrin, protagon, hæmoglobin, and the ferments.

With respect to this last group in particular the observation applies that their chemical composition is only imperfectly known and their constitution or the arrangement of their constituents atom is wholly unknown. Likewise the very existence of many as distinct chemical substances is not settled, as they are mostly uncrystallisable and hardly to be obtained in a pure state. On this point Dr. Beale remarks, "it is doubtful if many of the substances which have received special names, as albumen, fibrin, and the like are really definite chemical substances of fixed composition" (*Todd and Bowman*, p. 8.) In illustration of this we may remark that Fletcher continually speaks of the proximate principles *par excellence*, as sodo-albumen, ozmazome, gelatin, and fibrin. We have at this day a very different list, viz., that given above, and sodo-albumen is now only represented by the casein of the serum which occurs only in

the blood and the spinal fluid. Osmazome has now disappeared from the list, and its very name does not occur in the index to the best works on physiological chemistry. It is nothing but a mixture of several educts and products of animal flesh. The once famous organic radical of the albuminates, protein, is now no longer heard of. The cerebrin, cerebrie acid, lecithin, and the phosphorated fats, which for long were said to be the proximate principles of the brain, are said now to be all merely chemical derivatives of protagon. The composition of the albuminates as to per-centage of the ultimate elements is almost exactly the same, and, although they differ much in properties, yet they yield many similar products to chemical processes, and on the whole the tendency of the elements of organic bodies to assume new combinations is very great, showing that the affinity holding together these complex substances is only feeble. For example, the serum-albumen digested with hydrochloric acid becomes converted into syntonin, and with potash into casein. Fibrin digested with hydrochloric acid yields syntonin, which is soluble, but you cannot get back the fibrin from that or any other solution. The probability, therefore, is that we have not yet heard the last word of chemistry on the proximate principles, and that new ones will in future be added to, or take the place of, the old. Does not all this point to the probability that the chemical proximate principles are, as it were, chemical compounds in the nascent state or the earliest on the border line from a totally different state, viz., the vital or metabolic? Now, as the solids and fluids, slowly formed by the vital action of nutrition and secretion, may be looked on as chemical compounds, we must look towards the structureless products of comparatively sudden death as the most likely source of information respecting the actually proximate principles. Among these some spontaneously undergo changes palpable to our senses at or soon after death, the most notable of which is the coagulation of the blood and stiffening of the muscles, which have attracted attention from the earliest times. Let us, therefore, test this question by going into some detail respecting the nature

of the fibrin and the myosin which are the efficient agents in those phenomena.

§ 71. In opposition to the opinions almost universally prevalent at the time, Fletcher considered "the coagulation of blood a purely chemical phenomenon under all circumstances" (p. 44), and that "its non-coagulation, while still circulating, is obviously to be ascribed to the perpetual molecular changes which it is undergoing, and which was incompatible with the full development of that principle on which its coagulation depends" (p. 114). That fibrin, being a merely chemical compound, did not exist as a constituent of the living matter, and that all the then (and even still somewhat) prevalent theories of its organisable nature were false and the appearances leading to such conclusions deceptive. He did not, of course, anticipate without experiment, the exact state of the facts now known, but he held the *liquor sanguinis* to be a non-vital chemical mixture, containing fibrin in some state in which it was never "sufficiently long identical" owing to the perpetual and very rapid vital changes going on in the capillaries to allow it to coagulate.

It is interesting to notice that in 1845 the now illustrious J. R. Mayer gave a somewhat similar explanation of the resistance to chemical change displayed by easily decomposed fluids surrounded by living tissues. It is not from any influence of a supposed vital force, he says, but is owing to the constant changes produced by secretion and absorption (*Mechanik der Wärme*, p. 79).

With these plainly expressed views of a strong vitalist like Fletcher's, in 1836, before us it is difficult to understand the air of triumph with which Bence Jones brings forward the actual experiment, in his *Lectures on Force* (1868), showing the formation of fibrin from its chemical elements, and commenting on it as a sure sign that the reign of pure chemicalism is at hand. But let us go into the subject in detail.

§ 72. It was found by J. Müller that mixing fresh frog's blood with a weak solution of sugar retarded the coagulation so much that the *plasma* or *liquor sanguinis* could be filtered in a fluid state free from the corpuscles, after which it coagulated as usual. Kühne

regards this as simplifying the question by cutting off all the supposed influence of the globules. We have now a fluid in the somewhat paradoxical condition of containing a substance which has the singular property of separating itself spontaneously. The plasma is far from being a saturated solution, especially as regards the fibrin, which amounts to little more than 1 per cent., and which it can never dissolve again. Light was thrown on the subject in 1845 by the discovery by Dr. A. Buchanan of the "coagulant power" of some of the constituents of the blood on the fluid of hydrocele, and the hypothesis began to prevail, especially through the influence of Virchow, that no fibrin existed in the solution, but only a fibrino-genic substance; and finally Dr. A. Schmidt demonstrated that such was the fact, and that fibrin does not pre-exist in the blood as such, but is formed by the union of two chemical constituents of that fluid. It is, therefore, strictly a chemical compound, and this view has been universally adopted. The discovery of the fibrinogen and the paraglobulin as the generators of fibrin cleared up some of the most puzzling facts noted by the older chemists, more especially those observed by Denis of the action of the *Sulphate* and the *Muriate of Soda* on fresh blood. And now the chemical process of formation can be performed by extracting the two constituents separately and mixing them; and the product differs in no way from natural fibrin. But the details of the process interest us as bearing on the influence of vitality. The question of why the plasma coagulates being answered, there remains that of why it does *not* coagulate in the living body, and the important point to know first is whether the same causes which prevent natural coagulation in the fresh drawn blood act equally on the artificial process. The facts on this head have been brought together by Kühne (*Physiol. Chemie*, p. 171). We must recollect that the fresh drawn blood, besides the mere chemical principles, contains living white corpuscles, which, in spite of the opinion above given of Kühne, are held by Beale to have an influence on the formation of, or in fact do form, fibrinogen.

The effects of temperature.—The plasma may be frozen fast, and when thawed is still fluid. It appears even that a temperature of about 32° F. may keep the plasma fluid for an indefinite time. But with increase of temperature the time necessary for coagulation steadily diminishes, and at ordinary blood-heat and a little above it takes place very rapidly. Now the effect of cold in suspending vital action without destroying it is well known, but exactly the same effects of temperature are met with in the artificial formation of fibrin from fibrinogen and paraglobulin. The effects of acids, alkalies, and neutral salts in retarding or favouring coagulation is exactly the same with the natural blood and the artificial mixture. The retardation by *Carbonic acid* and the promotion by the access of oxygen follow the same rule, and this last is important, considering the powerful action usually

attributed to oxygen in vital processes. The proofs that the formation of fibrin is chemical and not vital being complete, there still remains much difficulty in understanding the nature of the process. For a quite disproportionally small quantity of paraglobulin is required to convert a large quantity of fibrinogen into fibrin. The two are extremely like each other in chemical composition and in their reactions. It was remarked by Schmidt that both are alkaline in their separate solutions, but on their union the serum becomes more alkaline, as if they had combined with a certain quantity of alkali like weak acids, and the formation of fibrin thus resembles the precipitation of *Silicate of Alumina* from the solution of *Aluminate* and *Silicate of Potash*. Fibrin does not exist in the living organism in health, though easily produced by injuries and diseases, and an important use of it is to limit hæmorrhage where vessels are divided: and besides, according to Beale, "it forms, when effused in internal parts or on the surface of wounds, a temporary tissue, a cementing or protecting substance, or bond of union between separated parts which serves as a nidus for the development of the masses of germinal matter which are to take part in the formation of a higher, more elaborate and more durable texture." Blood clots always contain living white corpuscles at first, and when they have appeared to be organised and take on living functions, it is always owing to the growth and development of these masses of living matter.

With respect to the nature of organisable lymph Fletcher, of course, denies that it consists of mere fibrin deposited by coagulation, but is secreted, *i.e.* formed by a vital process by the adjacent vessels; and while not denying the outgrowth of vessels from the old parts also, he represents it as "containing within itself the germs of its future nerves and vessels, which being developed first in its substance extend gradually to the neighbouring parts and thus connect them with the newly organised mass. The most philosophical view of the subject appears to be to regard organisable lymph as a kind of germ, and its organisation as depending on the same laws as those by which that of the embryo is effected." (Fletcher's *Pathology*, 184.)

It is impossible to go into such a large subject here, but as bearing on the vitality of fibrin the general opinion is in accordance with the above, and although in most examples of inflammatory lymph fibrin and corpuscles occur together, yet it is to the latter alone the vital properties are attributable.* Indeed, inflammatory lymph is divided by Mr. Paget into the fibrinous

* "In the repair of a divided tendon, masses of germinal matter probably result partly from the multiplication of the adjacent germinal matter of the tissue itself, and partly from the white blood-corpuscles which have escaped from the divided vessels, or from masses of germinal matter descended from these." (Beale's *Oxford Lectures*.)

and corpuscular varieties according to the proportions in which they are mixed. The origin of the corpuscles is not uniform as in some cases the migration from the blood-vessels is admitted; and in others the formation by growth from the adjacent parts is still looked on as the chief source, and in fact must always be so if we consider the subsequent development of the product.

With respect to the origin of the fibrinogen, the following remark by Dr. Sharpey is important, as it shows that no light can be thrown upon its formation by chemistry which the apparent resemblance of the fibrino-plastic agent to a ferment might lead us to expect. "It is not that the latter converts albumen into fibrin, for after a certain amount of fibrin has been coagulated from the venous fluid no farther addition will generate more, although abundance of albumen remains; and again, a given quantity of fibrino-plastin will not coagulate with equal rapidity and intensity any amount of fluid containing fibrinogen. In short, the fibrino-plastic substance seems not to operate as a ferment or by catalysis, but by combining with the other necessary ingredient." (*Quain's Anatomy*, 1867, p. xxxix.)

With respect to the origin of the fibrinogen Dr. Beale thinks it possible that it is formed by the vital action of the living white corpuscles, and the smaller colourless corpuscles allied to them. These gradually diffuse the fibrinogen through the plasma, and during coagulation the red corpuscles are seen to undergo change during which probably the globulin which they contain escapes. (*Todd and Bowman*, p. 123.) This does not fully meet the objection at first stated (p. 514) against the influence of the corpuscles, although in that experiment the globules may have already escaped and gone through the filter. But in favour of it we have the facts that, *ceteris paribus*, the total blood coagulates more quickly than the plasma deprived of the corpuscles, for although the latter contains enough of fibrinoplastin to convert the whole fibrinogen, yet the change takes place more rapidly in presence of the greater abundance of paraglobulin which the red corpuscles afford, as they are the most richly supplied with that substance of any part of the body. It is also found that the plasma of blood that has been frozen and thawed coagulates even more rapidly than blood containing the corpuscles in its complete state. In this case the colouring matter has escaped into the plasma and with it paraglobulin. (Kühne, p. 222.)

The fibrinogen occurs in the blood, the lymph, the chyle, and all the serous fluids except the cerebro-spinal. It is most abundant in the pericardial fluid and among pathological formation in the fluid of hydrocele. The paraglobulin, which is distinguished from globulin by some chemical signs and which alone is fibrino-plastic, occurs in the corpuscles, plasma, and serum of the blood, the chyle, the cornea, and aqueous humour, and pathologically in the urine and pus.

To finish the subject of their non-combination in the living

body it may be noticed that Brücke examined exhaustively all the conditions applicable to the question, and came to the conclusion that the sole cause was the contact with the living vessels or tissues. Even within the larger vessels blood coagulates around foreign bodies introduced into them; and it was finally shown that it does so under all circumstances when the vital actions of the solids upon it were cut off. None of the chemical reasons, such as the rapid destruction of the specific power of the paraglobulin by ozone (never yet shown to exist in the blood), are admitted by Kühne to be satisfactory, and the direct experiments of Magendie, Brown-Séguard, and A. Schmidt, showing the reproduction of the fibrinogen, are in favour of the influence of vital changes being the cause. A. Schmidt's experiment was as follows:

"The heart of a living turtle being separated from the body was allowed, by the continuance of its pulsation, to exhaust itself as far as possible of blood. It was then injected with serum so as to free it entirely from red blood. It was then filled with defibrinated turtle blood; hereupon the heart, which had been motionless when empty of blood, began immediately to pulsate again, and the first portion of blood that flowed out of the artery coagulated immediately. We thus see, firstly, that fibrinogen is again produced in the defibrinated blood by the action of the pulsating heart; secondly, that this is thrown out as fibrin with unusual quickness for turtle blood, as soon as the blood is withdrawn again from the heart; but if the artery is tied up and the blood retained longer in the pulsating heart, it becomes gradually as slow in coagulating as that of ordinary turtle blood. To all appearance the pulsating heart or the contracting muscle destroys again the fibrinogen which it produces." On this Kühne remarks, "The last fact deserves particular attention, for when we think that the fibrinogen is continually produced and again consumed, and that for the separation of it as fibrin a certain time is requisite during which the action of the paraglobulin must continue, so it is only at the death of the vascular walls, when the fibrogen-metamorphosis ceases, that the opportunity is given for any remnant of this body to unite with the other fibrin-generator and form a clot." This is exactly in accordance with the view of Fletcher, who points out the continual and rapid change that the blood is constantly undergoing. In fact, he calculates that the mass of the blood does not retain its identity longer than three minutes and a half. The existence, therefore, of any individual particles of fibrinogen is merely temporary. It agrees also with the relative diffusion-power of the two substances, for in this respect the paraglobulin far surpasses the fibrinogen, which latter does not diffuse towards albumen, hæmaglobin, or paraglobulin. Now, the corpuscles are much more permanent than the fibrinogen, and as there is no fear of its passing into them the rapid change of it prevents coagulation. Nevertheless,

although so much has been done we can hardly say that the non-coagulation in the living state has been yet fully explained. This is also the opinion of the chemicalist, Bence Jones, who calls the chemical formation of fibrin an "unfinished discovery," and, oddly enough, answers the above questions very much in the same way as the physiologist, Fletcher. He says, "the fibrinoplastic substance is rapidly acted on by ozone. The fibrinogenic substance also is rapidly changed after it is formed, so that possibly the necessary quantity of these substances is not given the necessary time to act upon one another for the production of coagula" (p. 58).

From all this we have learned that fibrin is a dead chemical compound, derived from two other equally dead chemical compounds, but we have learned nothing of any possible chemical origin of these latter, nor have we advanced one step nearer to any substance which possesses the powers of living matter described in § 67. Nor do we see that fibrin has any better claim to be capable of putting on vital functions or to be more "organisable" than hair, horn, or cuticle. Before proceeding farther with the general question, let us consider the analogous proximate principle of the muscles, viz., myosin, which is supposed to display vital contractility and the response to stimuli.

§ 73. In the same way as with blood action, Fletcher disposes of all theories of muscular action which presumed the existence of fibrin in the living muscle, while at the same time he explained the *rigor mortis* by its first formation. He controverts the theory of Humboldt, Cuvier, Rudolphi, and others, that contraction of muscles during life can be in any degree dependent on coagulation of fibrin, and maintains that none of that principle exists in the living muscle, and it can therefore have no share in any of its functions. At the same time he shows that the theory prevalent since the time of Hunter and Nysten, that the *rigor mortis* is in some way dependent on muscular contraction is erroneous, and that, in fact, that phenomenon really depends on the development for the first time, and speedy coagulation of fibrin in the muscles. His conclusion is, that "the muscles stiffen, owing to the development in them of fibrin, which they did not previously contain. It is the distinguishing property of fibrine to undergo spontaneous coagulation; but no such coagulation takes place in muscles till they are deprived of their vitality, when they rapidly become stiff; and it is to this cause, and not to the

contraction of the muscles, that the rigidity of the limbs, which soon succeeds death, and continues till the putrefactive process has commenced, is to be attributed" (p. 130).

At the same time, to meet the difficulty that a purely chemical cause of *rigor mortis* like this should be prevented by causes operating vitally, such as violent stimulation and over driving, he says that can only be referred to a "deterioration of the muscular tissue, which is resolved into fibrin after death, only when in a state of integrity." These propositions he established by a train of argument unnecessary to reproduce, but which it will be more interesting to illustrate by a short review of the progress of discovery on the subject since.

§ 74. In 1836 the chief constituent of the muscles was universally considered to be identical with the fibrin of the blood, but some years afterwards Liebig finding its reactions different, called it muscular fibrin or syntonin. But since then it has been found that this is not the first product of the muscular plasma on coagulation, and that it differs from that in several particulars. Syntonin is formed by dissolving the coagulum in muriatic acid, but that solution does not coagulate on boiling; it does not dissolve in solutions of common salt; it does not decompose the peroxide of hydrogen, which property is possessed in a marked degree, not only by myosin, but by fibrin and its two generators; it can also be produced from other albuminous bodies, such as fibrin, casein, white of egg, and, as a matter of fact, all the albuminous bodies of muscle can be changed into syntonin, and dissolved by dilute hydrochloric acid. In short, the syntonin is nothing but a product of the action of dilute hydrochloric acid on myosin and other albuminates, which latter are thereby changed into it, and cannot be got back again from the solution, so it does not pre-exist even in dead muscle, and we may dismiss it at once from the proximate principles. The knowledge of its precursor myosin we owe to the indefatigable physiologist Kühne, who gives the first complete account of it, and names it in his *Untersuchungen über das Protoplasma*, 1864. The question of the myosin is more complicated than that of the fibrin, because the muscular plasma from which it spontaneously separates by coagulation cannot be obtained except under conditions which also retain the vitality of the parts, and as yet no chemical generators of it have been found from which it can be formed purely chemically. It is treated of by Kühne as the characteristic constituent of all the contractile substances, both animal and vegetable, which are distinguished by the power of spontaneous

coagulation after the cessation of their movements, and which alone in nature contain albuminous compounds, which coagulate at the comparatively low temperature of 35° to 50° Cent. To such substances he, with most of the German school, restricts the term protoplasm. These are the whole substance of many infusoria, such as amœbæ, the soft parts of the rhizopods, the contents of many vegetable cells; the white corpuscles of the blood, lymph, and chyle; the corpuscles of connective tissue of mucus, saliva, pus, &c., all which are capable of apparently spontaneous movement, also the fluid contents of the sarcolemma or muscular plasma.*

Dr. Beale's more general application of the term protoplasm or germinal matter to the living matter *par excellence* will be followed here. The substance from which the myosin spontaneously separates is the single-refracting matter contained in the sarcolemma in its purest state, but it is very difficult to obtain except by methods which also retain vitality. These are drying and cold. The muscle of the frog may be quickly dried before coagulation, and the muscular substance from the dried portion may be dissolved out, and will then coagulate spontaneously. This does not happen with blood or lymph. Now, we know that many low forms of living animals, such as the rotifers, may be dried for a long time without destruction of their vitality. The other method is by cold, which we know also preserves the vitality in a state of suspended animation, and many animals, fish especially, may be frozen and restored to life afterwards. So we must notice that in the following processes we must for a time not only get the precursor of myosin, but some part of the matters operated on must be in the living state. Kühne takes the muscles of frogs, carefully freed from blood, by injecting a $\frac{1}{2}$ per cent. solution of common salt till it returns colourless by the veins. They are then submitted to various manipulations at a temperature of -7° C. when hard frozen, and finally a fluid is obtained on thawing, which can be filtered, and will then display the properties of the muscular plasma. He has found that uninjured muscles exposed to a cold of -7° to -10° Cent. for three hours, when thawed in a room at 15° C. (64° F.), remain irritable on an average for six hours, so we may presume that these fluids retained their normal properties. The muscular plasma thus obtained is a yellowish, syrupy, alkaline liquid, but it is difficult to free it from

* Dr. Beale says (*Protoplasm*, p. 15) that Kühne considers all contractile material to be protoplasm, and that he applies that term "to the contracting muscular tissue which exhibits structure, as well as to the structureless." Now I think that it is scarcely a correct description of Kühne's statement that a substance which after death becomes myosin is contained in the single-refracting contents of muscular fibres, and in all other organized substances which exhibit movements. That is simply a statement as to facts, whatever be the cause of those movements.

portions of fibrous matter, and it quickly chokes the pores of the filters. It is therefore quite possible, although Kühne does not advert to the question, that the finer particles of living matter, as spoken of by Beale in the blood plasma, may be present, and we cannot say what part they may play in the subsequent change that takes place. That change is spontaneous coagulation and separation of the myosin, which is analogous to fibrin, although differing in some particulars in its reaction with acids, alkalies, and salts. The formation of myosin takes place very slowly at 0° Cent., but almost instantly at 40° Cent. It is also produced immediately by dilution with pure cold water, so it can be got pure by dropping into very cold water, when each drop falls to the bottom in the form of a firm elastic ball of myosin. If dropped into a 0.1 per cent. solution of hydrochloric acid it coagulates at first, but is again dissolved before it reaches the bottom, but it is now no longer myosin, it is converted into syntonin.

To follow now the changes that take place in the death of muscles, we will take for granted that the above plasma is really the fluid part of the contents of the sarcolemma, which is not a solid fibre at all, as already stated at § 23 (p. 52). The first step is the gradual disappearance of the contractility, so that the strongest stimuli produce no longer any shortening; then, 2nd, appears the stiffness shown by loss of flexibility and elasticity; 3rd, the acid reaction; 4th, the opacity, now the *rigor mortis* is at its height. When these are contrasted with the state of the contracted living muscle, the difference is plain. The latter is transparent, elastic, soft and alkaline generally, and if the sarcolemma be torn, the contents are pressed out; this last does not happen in the death rigidity. After going through all the phenomena and conditions of the state, Kühne concludes that the *rigor mortis* depends on the development and coagulation of the myosin from the plasma, which is fluid in the living state. He says (*Physiol. Chemie.*, p. 282), "An end was put, by the reflections of Brücke, to all former views which looked on the death-contracted muscle as eminently living, in as far as it was thought to be tetanically contracted."

Hermann also (*Grundriss der Physiol.*, p. 220) says, "The circumstance that a shortening of the muscles took place in *rigor mortis*, as in their ordinary activity, nourished false views on the nature of the stiffness for a long time. It is only since the supposition (Brücke) of, and the proof (Kühne) of, a coagulable body in the muscle that the doctrine here given has become generally taught."

These authorities seem not to be aware that the arguments of Fletcher were quite as cogent long before.

The muscles do not stand alone as to stiffening at death, for a certain amount of that takes place in many tissues, owing to the protoplasm they contain. In all these contractility and the spontaneous movements cease, and coagulation takes place at a tem-

perature between 35° and 50° Cent. (78° and 112° F.). Kühne considers it probable that all varieties of the contractile protoplasm occurring as part of the higher animals, and when it forms the whole animal, as in the *amœbæ*, &c., contain myosin. It is also found in the axis cylinders of the nerves and in pus. We have thus a substance that comes very near to the living matter, while at the same time we see undoubted proofs that it does not exist till after death. But it is even doubtful whether its immediate precursor in the muscular plasma is not also a mere chemical compound, like the fibrinogen, so it may not stand next to the living matter after all. Is the myosin, in fact, like the fibrin, a compound of two generators? In favour of this supposition Kühne gives several chemical reasons which need not be repeated, as they are not conclusive, and certainly these substances, if they exist, have not yet been isolated. The history of the muscular plasma is not finished with the separation of the myosin, for muscle-serum, which remains, still contains albuminous bodies of interest. At temperatures above 0° Cent. it soon becomes acid, and at 45° Cent. again deposits a coagulum, which is not myosin. After filtration there still remain at least two other albuminous bodies, viz., the potash-albuminate or casein, and the serum-albumen, which is identical with that of the blood. Besides these, it contains the usual salts, the phosphate and lactate of potash. All this gives us very little information as to the state of the muscle-plasma before the formation of myosin, and while it is either itself in the living state, or mixed with living matter in the process of extraction.

As yet we have seen no substance capable of responding to the action of stimuli, nor seen reason to suppose that any part of the function of force-extrication can be chemical rather than vital or metabolic. Nevertheless, there are physiological grounds for supposing that a large part of the process may be physical and chemical. It is too large a subject to go into here, but we may with advantage touch on some of the points which show how far chemical explanations can go, and where the different order of things we call vital or metabolic must come in, to all appearance. All agree that the mechanism of muscular motion is purely physical, in as far as it is an approach or change of direction of material parts, just as shortening would be produced by a similar change in any similar thing out of living beings. Fletcher, in reviewing the theories of the day, concluded that the most probable of them was the zigzag bending of the muscular fibres. Now, since the solid fibres are abolished,

the shortening is considered to be produced in what is still a similar way, viz. by the changes of position of the diadclasts,* which are the component parts of the muscular prisms, rows of which, enclosed in sheaths with the plasma, constitute the muscular fibres. The source of the force producing these movements is, without question, that of chemical attraction set free by the satisfaction of stronger affinities from complex substances passing down to simpler combinations. So far we may speak with tolerable certainty, but it is believed on good grounds that direct oxidation is not the chemical change which sets force free; and also there is a complete gap in our knowledge between the origin of the force and its application; electricity is held by some good authorities to be the intermediate agent, but nothing is positively known, except that it is certain that it is not heat which is converted into work (Voit, Clausius, Fick).

Here we must be excused for going farther into details, for we stand now on the boundary line between vital and chemical action, for contractility has always been considered one of the cardinal attributes of life alone. Now, since the discovery of the law of conservation of force, we know that there can be no such thing as a property of contractility in the sense of a capability of producing motion without expenditure of an equivalent of force whether the matter be living or dead; and that in the living matter there is no other source of force than that of chemical attraction set free as said above. There is no doubt this doctrine has tended much to the revival of purely chemical theories of life, but without reason in my opinion, for it is never pretended that vital affinity could create force any more than chemical affinity, both being merely properties (§§ 60, 61, 64); and on any analytic change of molecular aggregation the amount of the outcome of force depends solely on the less amount of potential energy in the new compound than in the original one. Hence it makes no difference whether that change takes place while the atoms are under the influence of vital or of chemical affinity as far as the amount is concerned. The problem set before us in muscular force-extrication is this: we have a complex unstable substance with high potential energy, viz. blood, and oxygen likewise with high potential energy in the capillary vessels at one end of the scale; while at the other in the muscle we find products far down in the scale of potential energy or totally spent; viz. lactic and carbonic acids, while the difference of force is set free in the forms of work and heat.

* Brücke, in Stricker's *Histology*, i, p. 240.

Whether the intermediate process be chemical or vital, or partly both, can make no difference in the quantity of force set free. It is, perhaps, most likely that the whole process takes place within the inscrutable domain of the metabolic affinity, in which case all the chemical products—both those existing in the muscles during life, and those which only come into existence after death—would spring directly from the living matter.

We have an example of decomposition of pabulum by purely vital action with evolution of actual energy as heat in the growth of the yeast-cell. This is a simpler process, as oxygen takes no part in it, and there is no mechanical motion produced; so, in view of the greater complication and need of more rapid action, it may be that the evolution of force proceeds from a chemical compound formed by the living matter—a secretion, as it were, containing the oxygen synthetically combined, and ready to split up with evolution of force.

In this case the myosin, &c., may have chemical progenitors. Such are presumed in the chemical hypothesis of Hermann, given at § 23* (p. 51), of which a few words more may be said. The hypothetical substance "inogen" there spoken of, whose splitting up into myosin and the spent products carbonic acid and lactic acids, is presumed to furnish the force extricated as heat or work, cannot well be like either of the fibrin-generators, because the myosin in Hermann's theory plays the part of a chemical ferment, causing the oxygen and certain non-nitrogenous matters of the blood to unite into the form of inogen and immediately decompose, while it returns to the state of myosin, just as sulpho-vinic acid is supposed to be the intermediary in the catalytic formation of ether by sulphuric acid and alcohol. Now, we know that is not the case with the generators of fibrin, which simply combine, although in unequal proportions, and neither can be got back from the product. We have, however, no example in chemistry of such a complicated process as the inogen formation, and certainly not, as before said, § 23, under stimuli; and the supposition of a myosin formation at all before death goes against what has been said of the cause of death rigor, and brings back Hermann's theory to that of Humboldt and Cuvier. Hermann, indeed, is obliged to suppose that in natural muscular action the myosin does not really coagulate, but remains in a first or nascent stage, in which it is merely gelatinous, and not optically demonstrable (p. 226); and from this it may pass back over and over again into "inogen" like a ferment. As one point in favour of this first stage of myosin formation being a part of natural contraction is the fact alleged on Hermann's own authenticity, that "a muscle cut out of the living animal produces the same total amount of carbonic acid, whether it directly stiffens into death or previously has been excited to contractions, with formation of

* Vol. xxviii, p. 299, of this Journal.

carbonic acid ; thus the more carbonic acid there is produced by contraction, the less is produced by stiffening'' (p. 225).

The same has been observed by Ranke with respect to the lactic acid. These are certainly in favour of the theory that some substance is formed as long as the muscle lives, whose splitting up produces carbonic and lactic acids, and thereby extricates force, either as heat or work, but they do not prove myosin to be thus formed.

One thing stands fast, viz. that no process of direct oxidation takes place during muscular action. This has been settled by the elaborate experiments of Voit, who shows on the large scale that, although the source of muscular force may be the formation of the non-nitrogenous products derived from fats and sugars, yet the albuminates must take an essential part in the process, "with their important function of condensation of oxygen, without which no combustion of fat and no extrication of force is thinkable ; hence the quantity of the albuminous matters determines, also, in this case the amount of the work."* The formation of some substance by oxidative synthesis is, therefore, a necessary preliminary to force extrication. Now, it seems not improbable that the splitting up of some merely chemical compound of high potential energy is the immediate source of the work and heat of muscles, but, as before said, I cannot but believe that we have now got to the limit of chemical action, and the formation of that substance is the work of metabolic or vital action.

Hermann says that the splitting up of his "inogen" is prevented by sudden strong heat or mineral acids, but the substance is at the same time destroyed. This is in allusion to the fact that by throwing a portion of living muscle into boiling water or by strong mineral acids it loses the power of forming carbonic acid, it does not become acid, nor does it stiffen. We have here the counterpart of Fletcher's statement that the non-stiffening from excessive stimulation is owing to *deterioration* of the living matter and its too rapid death to form the products of its gradual death. We must remember that our knowledge of what exists in the plasma before death is merely speculative. We know that in the contraction of muscle carbonic and lactic (properly paralactic) acids are produced, but from what matters they proceed we do not know. There are likewise a great variety of extractives, but it is not known in what quantities they exist in the living state, and although Voit proves that the use attributed by Liebig to the kreatin is exaggerated, if not quite erroneous, still it is a defect in Hermann's or any similar theory that nothing is said of them at all. I am not unwilling, as a vitalist, to admit the partial application of some chemical process as above, but I think all will agree in Hermann's own conclusion respecting chemical theories as applied to the whole process.

* *Ueber die Entwicklung der Lehre von der Quelle der Muskelkraft, &c.*, von Carl Voit, p. 14.

"There is as yet entirely wanting the more profound knowledge of muscular action, especially of the connection between change of form, heat formation, and variation of electric currents among each other, and with the chemical splitting-up processes. All theories hitherto put forth are faulty" (p. 240).

At any rate, by the experiments of Pettenkofer and Voit we have got rid of the coarse chemical notion of combustion of the fats and sugars, or the muscular tissue itself, producing heat which is again converted into work as in a steam-engine.

We may, indeed, take Voit's statement of the essentiality of albuminous matters for the combustion of fats and sugars simply to mean that such must be performed by a vital process, for such processes are only displayed by compounds into which nitrogen enters, and therefore a due proportion of nitrogen must enter into the diet. To enter into the process by which the fixed proportion of nitrogen is retained in the system belongs to the question of general nutrition, and, although bearing on the question of muscular action, would lead us too far from our subject to be entered upon here.

The action of a number of chemical substances as stimuli in inducing contraction and coagulation on application to the cut ends of muscular fibres and not to their nerves, seems to Kühne to show that the whole process is chemical. This will be considered under the general action of stimuli; but, in the mean time, it is not difficult to suppose that many chemical substances would cause the splitting-up of a supposed "inogen," and that the living matter might, under stimulus of the nerves, secrete some ferment which might have a like effect. On the other hand, it is also quite as conceivable that all those agents act on the living matter, and that the myosin coagulum which appears on their operation is the result of over-stimulation and death. Myosin may, no doubt, be formed and removed even in the living state just as fibrin, but then it is a pathological product. The insuperable difficulty for the chemist lies in the *formation* of all those substances engaged in the work of contraction which do not exist in the blood, which is the sole source whence they are derived, not to speak of the formation of the muscular apparatus itself.

We have already said that Hermann's hypothetical inogen has not been isolated, because it splits up by every chemical treatment, and in another place (p. 211), after enumerating the chemical constituents of the dead and rigid muscle, he says, "As the process of coagulation as well as the contraction is accompanied with changes in it which are still unknown, and as the uncoagulated muscle-plasma cannot be analysed without producing contraction or coagulation, so the above-named constituents of the coagulated muscle cannot be looked on as the constituents of unaltered living muscle."

It is superfluous to enter on the chemical history of albumen, gelatin, protagon, or any more of the proximate principles, as it

must be sufficiently obvious that, in spite of the advance of organic chemistry, its stand-point in respect to Fletcher's theory of life is exactly the same now as it was in his day. To return to the more general question.

§ 75. In no substance chemically definable have we yet seen any true attributes of life, and many of the chemical school will grant that those we do know are products of the chemical processes used, or of changes that have taken place before analysis could be begun. But these persons mostly imply that in time, by increased skill, we shall at last get to some ultimate substance nearly pre-existing in the living matter which shall be capable of vital functions and at the same time cognisable by chemical reactions. Also which by combination and splitting up shall be capable of forming the long and complicated series of substances forming the tissues and fluids by whose really physical and chemical properties a great number of the subordinate functions of living beings are performed. Liebig, for example, in his recent papers on fermentation, after pointing out that a number of important processes (such as the primary stages of digestion) are really the work of purely chemical ferments, goes on to say that these ferments may possibly be formed by the chemical reactions of other unknown compounds; and thus by a long series of combinations which chemistry will one day reveal to us we may be able to explain the formation of the tissues and the functions of life without having recourse to what he sneers at as the antiquated notion of vitality or vital force, as he sometimes calls it, as if the two expressions meant the same thing. Vitality is, however, a property and not a force—two very different things, as it has been attempted to be shown at §§ 61, 62.

This by the way; but if such a time is coming it must be very slowly, for we do not seem to have made much progress in the last forty years. We have, it is true, seen the genesis of one supposed simple proximate principle, viz., fibrin; but if we can do nothing more than find two chemical progenitors for each, and even two for each of those again, it would not help us much as long as we cannot separate and recombine them, and combine the different

generators with each other, so as to form different substances, all of which are the ordinary details of chemistry. Nay, even if we can so far imitate vital action inasmuch as we can make compounds by processes in which one substance is not consumed, *e. g.*, as in catalysis and chemical fermentations, we must remember that we have not yet made chemically any of the organic ferments such as pepsin, ptyalin, and the like. I shall afterwards consider catalytics under the head of stimuli, but in the mean time it may be said their resemblance to vital agents is only superficial, and they all act by double decomposition and combination, like all other chemical agents; but here it may be said if they remain stationary they do not *grow*. No catalyser or ferment grows or reproduces itself in the process any more than spongy platinum. The living cells are self-forming, self-producing, and self-regulating. No machine makes itself. I am speaking here, of course, only of the chemical ferments such as diastase, emulsin, pepsin, &c., not of the physiological ferments like yeast, which last are vital agents. In short, to meet the difficulties of the first formation of organic substances the chemist has nothing but speculations and hopes with the slenderest foundations of facts as yet established.*

§ 76. The opinion that in the living state matter was withdrawn from chemical affinity, and that the proximate principles did not represent the actual state of combination of the elements in the living body, was not peculiar to Fletcher, but held pretty extensively by physiologists, such as Rudolphi, Barclay, Adelon, Prichard, Tiedemann, and others, and one great chemist Berzelius. From Adelon, Fletcher quotes, "Les molécules qui forment les solides du

* "The lifeless passes through no definite stages or states of being; the living invariably does so. The lifeless catalytic body does not necessarily alter in chemical composition during its action; the living one is always undergoing change in its active state. The first cannot be said to *form new material*, the last always exhibits this property. If the catalytic platinum could take up and convert the materials around it into *platinum*, and give rise to something differing in composition and properties from itself, as well as from the matters around it, which it had taken up, an analogy would exist between the phenomena above mentioned and catalysis." (Beale, *Medical Times*, 10th March, 1866).

corps humain, étant associées en vertu d'une affinité spéciale, dite vitale, et que les chimistes n'ont pas en main, comment ces chimistes pourraient ils prétendre faire une analyse de ces solides? Ils ne font que les détruire." But it is to be remarked that the majority of the above belong to the vital principle school, and that their real meaning probably differed very little from that of Prout, Thomson, Henry, and the others who expressed it as chemical affinity modified by a superior power. There is necessarily much vagueness to be expected in the language and ideas of theorists, who have always a *deus ex machina* at their beck and call, like the vital principle, ready to cut every knot they cannot untie; and I cannot find that any one expressed with decision and clearness the hypothesis of Fletcher above given (§ 67, 68 69). Since the promulgation of the present doctrine of force, however, it must be confessed that the view of an essential and irreconcilable difference between vital and chemical affinity has lost ground, and the great majority at present are inclined to the purely chemical theory of the constitution of organised matter. When we look back on the history of physiology, we observe that at every important discovery in physics and chemistry there has been a recrudescence of physical and chemical theories of vital phenomena, but that after a time vital physiology has surely regained its ground. I am persuaded the same will happen again ere long, and in the mean time, Dr. Beale whose discoveries in histology entitle his word to the highest respect, has stepped forward as the champion of pure vitalism. In the point now under consideration he repeats the words almost of Fletcher and Adelon, denies the existence of albumen or anything like it in living matter, and says the chemist can tell us nothing about the molecular condition or chemical composition of the germinal (living) matter, for the instant he commences his analysis the living matter has ceased to be living matter, and he is merely dealing with the products resulting from its death. (*Oxford Lectures*, 1868.)

It has always appeared to me a very singular thing that the vital principle school should go out of their way to raise

the organic elements out of ordinary chemical affinity, because with an agent of such transcendent powers at command it would be quite as easy to perform all the vital functions with albumen, fibrin, &c., as with new substances held together by a new affinity. And if the organic elements are really emancipated from chemical, and subject to a new affinity—and therefore must have new properties which would be presumedly capable of performing new functions impossible to chemical compounds—where is the use of the vital principle? Again, for the more modern school, who can bring themselves to imagine that any mere force—measurable as *m. v.*², and correlateable with heat and mechanical motion—will confer upon any substance whatever the distinguishing powers of life, they may just as well accept albumen and the rest as the materials for that force to act upon; which they accordingly do, and accept the ordinary proximate principles, or some other to be soon found, as the actual substances which fulfil vital functions. But in accounting for the striking differences between their conduct in and out of the body, they fall into extreme vagueness as to conditions and modifying circumstances, and if you probe the matter to the bottom you always find that the supposed vital force is nothing else than our old friend the vital principle with a new name. As examples, we find this shown in the expressions of men of the highest merit as experimenters, such as Kühne and Parkes. The former, after describing the movement of the amoebæ, in answer to an objection how it is that “such a small drop of albumen as an amoeba” can withstand the gradual diffusion into the water, says, “But the amoeba-substance is capable of, at the same time, assimilating nourishment and rejecting useless remains. It thus regenerates itself, and it is this maintenance of equilibrium of bodily substance by assimilation and excretion which we call life; this self-conservation against external influences, this struggle for existence, belongs to the definition of life.”*

This last part of the sentence has a teleological ring about it that puts all the pretensions to exact chemical science out

* *Das Protoplasma*, p. 39.

of court. People never talk of acids, alkalies, or salts, "struggling for existence." Dr. Parkes, in his recent lectures on the elimination of nitrogen,* says, "If we have no idea why an increment of carbon to a compound of carbon, hydrogen, and oxygen should give it a different consistence, weight, colour, and taste, or the power of crystallization and accretion, how is it possible we can form any conception what arrangement of particles shall evolve contractility, sensation, and the power of assimilation and growth?" This implies that some arrangement of particles does produce vitality, and the passage might have been written by Fletcher, as, indeed, fifty similar passages are written by him. But then Parkes goes on to illustrate the matter thus:—"A particle of an albuminate—a dead particle, as we term it—passes into the body, is attracted by a living structure, and then becomes gifted by the contact with the marvellous power of contractility and sensation." Granting that this is merely an illustration, similar to the vague resemblance, noticed by Owen, between magnetic and vital action, yet he nowhere gives us to understand that albumen is anything but albumen when it takes part in vital changes, or that these are other than chemical. Likewise, he speaks of "the force which absorbs nutriment into the germinal cell, and eventually differentiates it into tissues and organs."

The idea here is evidently that of Schwann, who says, "We must ascribe to the cell membrane, not only the power in general of chemically altering the substances which it is either in contact with or has imbibed, but also of so separating them that certain substances appear on its inner and others on its outer surface" (p. 199, *Schwann and Schleiden's Researches*). This faculty Schwann then proceeds to trace to something like galvanism. Now, we must recollect he wrote in 1839, before the true nature of force was known, and heat, light, and electricity were held to be imponderable substances, and it was impossible to make the distinction we now do between property, force, and power. From what has been said at § 61, 62, it cannot be admitted that any force, in the proper sense of the term, can have

* *Lancet*, March and April, 1871.

the above power. Apparently, also, Dr. Parkes is not satisfied with the position, for he is obliged to fall back on the darkness of the subject, and concludes by doubting "whether some greater mystery still is not behind the veil, and whether human knowledge will ever suffice to rend it." In this last clause all will, I imagine, agree; but why should vital action of matter be a greater mystery than chemical action, which is already pronounced to be inscrutable? Is not the additional difficulty raised by those who insist that the chemical affinity is still existing, and is controlled, counteracted, and directed by a something called force, whose powers have no parallel in the physical forces of which we are cognisant?

With a similar inconsistency, Cl. Bernard expresses the opinion that some day all the phenomena of living beings will be reduced to physio-chemical considerations, and immediately goes on to say that they are all subordinated to a *force vitale créatrice* or *idée directrice*, whose nature, when we look further, can be nothing else than the vital principle of Barthez, the inventor of the term, but not of the thing, if an imaginary substance can be called such, for it is nothing but the Psyche of Plato. And so we shall find with the theories of all those who assert the purely chemical nature of vital phenomena, and do not perceive the necessity of a distinct and peculiar affinity for their production.

§ 77. As might be expected, Fletcher gives no countenance to the idea of any intermediate stage between the vital and chemical state of matter—any stage, as it were, common to both which would permit the gradation of one into the other. There is no such thing as vito-chemical in the sense of partaking of both states. On the contrary, the division is sharp, abrupt, and absolute, and between them is an unfathomable gulf. This applies equally to the smallest particle of living matter which forms part of organised beings, as between its state while living and possibly an instant afterwards when dead, and to the organic contrasted with the inorganic kingdoms of nature.

He quotes with approbation the comment of Dr. Johnson, that there are "infinite vacuities" in the scale of animated

creation, wherever it begins or ends. He rejects as altogether false and unfounded Raspail's classification of organic compounds on the principle of "their being either already organised, tending to become organised, undergoing organisation, or organic." The expression *organisable* is used frequently in a false and misleading sense, applied especially to the albuminates, as if they were capable as such of taking on vital action. Gavarret (*Phénomènes Physiques de la Vie*, 1869) speaks of "the albumenoids as the true *organisable* substances from which are borrowed the fundamental elements of all organisation," p. 269). The albumenoids are no nearer life than the inorganic materials, so the above cannot be admitted, except in so far as they form a pabulum more easily assimilated by some organised beings. On this subject Dr. Beale is entirely in accord with Fletcher, and his frequently repeated and strong expressions must be familiar to almost every one. I may, however, quote this sentence from the Oxford lectures of 1869 :

"Between the *living* state of matter and its *non-living* state there is an absolute and irreconcilable difference ; that, so far from our being able to demonstrate that the non-living passes by gradations into, or gradually assumes the scale or condition of, the *living*, the transition is sudden and abrupt, and that matter already in the living state may pass into the non-living condition in the same sudden and complete manner."

In other places I think Dr. Beale lays unnecessary stress on the fact that the chemists have never yet succeeded in making anything the least like albumen ; so can they ever hope to make a living cell ? It is quite true they have not, and it is, no doubt, most probable they never will, but still it is possible. We know that diamonds, jacinths, sapphires, emeralds, and other precious stones, have never been made by the hand of man, but nobody doubts they are merely chemical compounds, and if we shall make them one day we may also make albumen. We can afford to grant that, and yet the purely chemical theory of life would not be advanced one jot. It is more important to notice the argument

which is almost universally relied upon by the chemicalist school, viz. that if we can make artificially the organic compounds, the fact is in itself a sufficient proof that the process of their formation in the body was merely chemical and not metabolic. On this point Fletcher observes, "We know, indeed, that binary compounds, such as carbonic acid and water, may result equally well from common chemical affinity as from secretion; and there is nothing, therefore, irrational in the idea that ternary and quaternary compounds might do so likewise" (p. 125). He then simply states the fact of the double origin of some, and possibly of all, the organic compounds, and dismisses with ridicule the inference founded on their chemical origin that we shall ultimately make living beings in the laboratory. But since then we have some direct testimony of the existence of the metabolic state from chemistry itself. We have the evidence of a slight difference in constitution of some chemical substances when resulting from vital action and when prepared in the ordinary way in the laboratory. For example, it was discovered by Pasteur that asparagine possesses the property of rotating the plane of polarisation of polarised light, which is likewise possessed by its derivatives, aspartic and malic acids, which are formed from it by subtracting one and two atoms of ammonia respectively. These two still possess the power of rotation, but the transition from malic to maleic and paramaleic acids causes the rotation-power to cease. The paramaleic acid is also called fumaric, as it is found in fumitory. M. Desaignes discovered that aspartic acid could be made artificially from fumarate of ammonia. Pasteur then tried whether the artificial aspartic acid, i. e. that derived from a so-called inactive or non-rotating body, possessed the rotating property like the aspartic acid derived from the active body (asparagine), and found that it did not. Thus, an organic compound, made artificially and apparently differing in no respect in the great bulk of chemical relations, cannot necessarily be assumed to be identical with that derived from the organic kingdom.* This is noticed

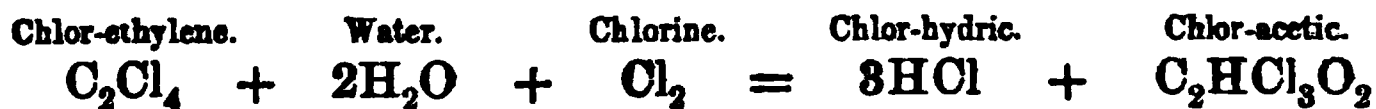
* See Biot's report on Pasteur's aspartic and malic acids, *Philosoph. Mag.*, iv, 4, p. 275.

by Professor Stokes in his address to the British Association, 1869, who says that the statement still remains true that no substance active in the above respect has been formed artificially from substances not possessing that property; therefore, the artificial substances cannot be said to be absolutely identical with the natural ones, *i. e.* those formed through vital processes. He suggests a physical explanation of the fact, but it still remains true, and, as far as it goes, tends to show that the metabolic process is distinct from the ordinary chemical action by which the same substance may also be formed. A second and stronger testimony to the same effect is the extreme difference of the mode and conditions under which a substance capable of being made both naturally and artificially is produced. Of this I have collected numerous instances, but it is unnecessary to accumulate proofs, so I shall confine myself to the instances brought forward by Dr. Beale. After remarking that the chemists are prone to speak of the synthetic actions of plants as only transformations such as they themselves perform in the laboratory, he contrasts the two modes thus:

A "cell, or rather transparent structureless matter does, without any apparatus or machinery whatever, and at a lower temperature and in a marvellously small space and with marvellous speed, much that the chemist cannot achieve with all sorts of complex apparatus, space millions of times as great, and time millions of times as long, at his disposal, and communicates its wonderful powers to new matters into the bargain."

"The *plant* may consist of a minute particle of *living*, clear, transparent, structureless, soft, semi-fluid matter, which, under certain conditions, takes up the water and carbonic acid, and the complex organic bodies are formed or form themselves! Now, let us see how the chemist proceeds to form, by the synthesis of its component elements, such a comparatively simple organic substance as acetic acid. Dr. Odling tells us how Kolbe effected the formation of this substance from carbon by a series of inorganic reactions:—'Disulphide of carbon, CS₂, was first obtained by the combustion of charcoal in sulphur vapour. This compound was acted upon by chlorine at a high temperature, whereby it was converted into chloride of sulphur and chloride of carbon, CCl₄. Then, by transmission through red-hot tubes, this last product was *transformed*, with evolution of chlorine, into the so-called sesquichloride of carbon, 2CCl₃ = Cl₂ + C₂Cl₄, and eventually into the so-called bichloride of carbon or tetrachlor-ethylene, C₂Cl₄ = O₂Cl₄."

In the course of his examination of this tetrachlor-ethylene, Kolbe observed that by exposure to chlorine in presence of water it was decomposed into a mixture of hydrochloric and trichlor-acetic acids; thus :



Then, by subjecting this trichlor-acetic acid to the action of nascent hydrogen, he successively converted it into dichlor-acetic acid, $\text{C}_2\text{H}_2\text{Cl}_2\text{O}_2$, monochlor-acetic acid, $\text{C}_2\text{H}_3\text{ClO}_2$, and finally into normal acetic acid, $\text{C}_2\text{H}_4\text{O}_2$. What can be more conclusive of the presence of some power in the plant, in obedience to which the elements combine with one another to produce the required compound? The highly complex substance is formed very quickly under conditions so different from those necessary for its production in the laboratory as to render it certain that the synthetic processes must be effected in a totally different manner in the two cases. I should have concluded that the chemist would have been led to reflect upon the wonderful and mysterious agency at work in the simple living matter of the plant, by which the same compounds are produced in a manner *so totally distinct from* that in which he is able to produce them. When the chemist has succeeded in forming a little clear transparent stuff like that in the plant, which will take up water and carbonic acid and *transform* organic compounds, it will be quite time enough for him to call the plant a 'machine' or a laboratory; and talking of the 'fiction of vital force' and *artificial processes* of oxidation 'more or less similar to the *natural processes* taking place in the animal body,' ought we not, for 'more or less similar to,' to substitute '*totally different from*'?"—(*Medical Times*, 1866, p. 362.)

"How are we to account for the fact of distinct classes of substances being separated, or separated and altered, by the agency of one and the same cell or nucleus? The oily, saccharine, and albuminous constituents (butter, sugar, caseine) of milk have not been discovered in the blood, and there is only one kind of cell to form these three classes of substances. Does the same cell-wall or nucleus separate from the blood, at the same time, oily, starchy, or saccharine and albuminous matters, and convert these into the particular constituents of milk, or are they separated one after the other" (*Beale's Todd and Bowman*, p. 109).

One would think that such examples were sufficient to show the existence of some radical distinction in the two processes, but Professor Stokes, in allusion to similar facts, still asserts that the natural organic compounds were "formed by the play of ordinary chemical affinity, not necessarily nor probably by the same series of reactions by which they were formed in the laboratory, where a high temperature is

commonly employed, but still by chemical reactions of some kind." As if those words of simple assertion could explain away the insuperable difficulty given even by temperature alone.

Professor Wyville Thompson follows in the same strain when he says, "It may be fairly questioned whether the chemical relations of the component elements of an organised body are in any way directly affected or controlled by life. It has become quite conceivable that a constant adjustment and readjustment of membranes and colloid diaphragms, in the presence of powerful catalytic agents, may possibly explain the maintenance of almost any chemical conditions, however complicated." (*Nature*, 25th May, 1871.) He ought to go a little more into detail. Are we to understand that a portion of clear structureless stuff, $\frac{1}{50000}$ of an inch in diameter, such as, say, any part of the secreting protoplasm of the liver, is provided with all the above complicated apparatus to enable it to secrete from the blood the complex fluid bile, not one of whose constituents pre-exist in the blood? Surely Beale's complaint is most apposite here, viz. why do the chemical school always fall back on the very complicated structures of complete individuals high in the scale as a base for theories? Again, what analogy is there between the mechanical catalysis, as we may call it, of spongy platinum, which merely brings oxygen within molecular distance, and the above? Or even between it and chemical catalysis, as in the manufacture of chlorine by sulphate of copper and hydrochloric acid, where the action is simply that of definite chemical combination through a definite intermediate compound, or if the latter is not actually formed, the definite affinities tending to it are engaged? Compare that with the extreme complexity of the metabolic action of the living matter of a milk or bile cell, which not only transforms the pabulum into such a variety of matters at one moment, but makes and renews itself. This last power the author admits to be inexplicable by chemistry; why, therefore, attempt to glide over the immense distinction between the metabolic or vital and any known chemical action by stretching the already vague word catalysis beyond any signification warranted by facts?

Now, the tendency of this is not merely curious or speculative, but is in the highest degree practical; for the question can never be of the action of the causes of disease or of remedies in a chemical sense, nor can we speak of parts, organs, or tissues, which are more or less organised, and to a greater or less degree susceptible of being acted on chemically. On the contrary, the whole living being must be divided into two parts, sharply and distinctly defined—one which is wholly chemical, and the other wholly vital, while, in respect to the latter, no knowledge of the chemical properties of any substance can give us the slightest clue to the behaviour of the living matter exposed to its influence. Hence the futility of all chemical theories of pathology and therapeutics noticed in § 64, except in the very limited sphere in which we can act on the merely chemical part, without at the same time exposing the vital part to the same agent. This subject is too large to be even touched upon at present, so we may pass to the extremely interesting question whether it is possible, as yet, to point out anatomically that sharp dividing line, and thus separate to our perception the living from the dead parts of which, in various proportions, all organised beings are made up.

CHAPTER IV.

THE ANATOMICAL SEAT OF THE LIVING MATTER.

§ 78. We now come to one of the most remarkable anticipations, by deductive reasoning, of a great discovery, subsequently made by others, which the history of science can display. We have repeatedly said that the proximate principles do not exist in the living state, but only come into existence on the rearrangement of particles which takes place at death. But where is that death, and in which part anatomically does that change take place at the moment of general death of the individual? Are we to imagine that an organised being—say a man weighing 150 lbs.—is composed of a mass of matter in the unknown metabolic state of affinity in which it differs chemically entirely from what

it shall be within a few minutes, or at most, hours, after death? This is not by any means Fletcher's meaning; on the contrary, he holds that during life the greater part of the framework of our bodies is in the same condition, chemically and physically, as we find it after death. In the first place, although all organised beings consist of both fluids and solids, he denies vitality to the fluids properly so called *per se*, going very fully into the question of the vitality of the blood and deciding against it. No objection will be made to such a decision now as respects the fluid part of the blood, and since the discovery of the living nature of the white corpuscles some of the apparent arguments for the vitality of the blood as a whole are explained and the possession of that property by the solids alone vindicated. The whole fluids (and all crystallisable substances) being thus subtracted from the living matter, a large part of the ternary and quaternary chemical principles are thus admitted to exist in the living body. Of the solids there are some tissues and parts, such as hair, epidermis, horn, nails, &c., which are admitted by all to be the chemical compounds we see. There remain, then, the solids composing the proper tissues and parts which, as a whole, take part in vital function. Then he says, "Admitting that irritability or vitality, general and specific, is a property of the organised solids alone, it becomes a question of the highest interest whether it be directly inherent in each of the organised tissues, either of plants or animals, or whether it merely appears to be possessed by them all in virtue of some one which is universally distributed over the organised being and inextricably interwoven with every other" (ii, p. 55). This question he examines particularly in respect to the two which display vital properties in the most palpable manner, viz. the muscular and nervous tissues. It must be remembered that with Fletcher irritability is synonymous with vitality, and is the property on which, with concurrence of stimuli, conditions and pabulum, all vital actions depend, including development and renewal of itself as well as all other tissues, and the fluids that are not possessed of this property. His reasoning is based chiefly on arguments

from comparative anatomy and physiology, and he notices that, although quite the lowest tribes of animals and all plants are destitute of a distinct nervous and muscular system, yet they distinctly show the possession of irritability, not only by movements but the whole range of the essential vital functions, such as growth and development, and that, therefore, the irritable matter exists in them in a diffused form, and that all possess, in a rudimentary form, a certain degree of the faculties which only become perceptible in the higher animals through development and specialisation. That "the splendid human organism itself consists merely of the same organs, regarded fundamentally, as exist in the polype, the differences consisting in their different degrees of elaboration" (p. 87). While, again, "the tendency to concentration or centralisation in this respect is, in general, in the direct ratio in each of the advancement of its organism, or its rank in the scale of organised beings"* (p. 19). By an interesting train of reasoning he comes to the conclusion that irritability (vitality) does not reside in the muscles, nor in the nervous system as a whole, but exclusively in the grey matter of the ganglionic nerves alone, and that it is only in virtue of the possession of this matter universally diffused and intimately interwoven with its texture that any tissue or part possesses vitality. Hence, he "must deny any direct participation in irritability or vitality" to "those peculiar aggregations of matter which go to form respectively the cellular, dermoid, mucous, serous, fibrous, vascular, osseous, cartilaginous, or muscular tissues" (ii 92). The grey matter of the spinal cord and brain have likewise vitality and the superadded, but localised, faculties of sensa-

* Mr. Darwin (*Descent of Man*, p. 211) attributes to Von Baer "the best definition ever given of advancement or progress in the organic scale; and this rests on the amount of differentiation of the several parts of the same being, when arrived, I should be inclined to add, at maturity." Nothing can better show the unmerited oblivion into which Fletcher's *Physiology* seems to have fallen, than the circumstance that Mr. Darwin is apparently not aware of its existence. Nevertheless, those chapters on concentration as an evidence of advancement contain matters of great interest on this subject, and they attracted attention at the time when the book called *Vestiges of Creation* was in vogue.

tion and thought respectively. Thus, with the exception of the grey matter of the brain, of the spinal marrow, and of the ganglionic nerves, the whole structure of the highest organised beings is actually composed of dead and now merely chemically combined matters. Here, then, is ample scope for the display of those mechanical and chemical actions which are certainly largely represented in the functions of living beings, without trenching on the truly vital actions which a large school are inclined to thrust away altogether. We may, without difficulty, now perceive how the bones give firmness and support; how the teeth grind the hardest substances; how the fibrous, elastic, and connective tissues perform their respective physical functions; how the muscles form an apparatus admirably adapted for the physical conditions of motion in particular directions, while probably both a chemical and a vital process are involved in the transformation of force; how osmosis, chemical fermentation, interchange of oxygen and carbonic acid by the hæmoglobin, and all the various processes, strictly physical and chemical, of animals and plants are performed in harmony with vital action. Anatomically it may be said that this doctrine is erroneous as applied to the ganglionic nervous system, as it is usually described, but it really applies to a hypothetically supposed, universally diffused, grey matter, which was, of course, not seen by him or any one else then, nor were the means existing of discovering such matter at that time. His view of the actual ganglionic nervous system, as presented by systematic anatomy, was much in harmony with the most modern conception, in particular that the fibres consisted mainly of filaments derived from the cerebro-spinal system, and especially an extension of Charles Bell's respiratory system of nerves, which, according to Fletcher, constituted a complete system of nerves of organic sympathy corresponding to the reflex action now usually spoken of. Fletcher's respiratory system has not yet received the attention it merits from physiologists, but it cannot be entered upon here. As the grey matter alone is with him the sole seat of vital properties, and these, of course, are inherent and incommunicable, the diffused ganglionic matter is that which

is of importance, and the fibrous part of the ganglionic system probably belongs entirely to centrifugal and centripetal fibres of communication for the conveyance of stimulus. From the restriction of all vital properties to the grey matter alone it follows that all fibres of nerves, of whatever denomination, must both begin and end in grey matter, either the diffused ganglionic nervous matter of the tissues, or the grey matter of the brain and spinal marrow; this also will be found in harmony with Beale's discoveries of terminations of nerves which are still contested by the German school.

The extreme precision of Fletcher's ideas in separating irritability from sensibility and the conveyance of any communicable influence whatever distinguish his view of the ganglionic nerves from that which represents them as nerves of sympathy, as their original name implies, and also as a whole the nerves of organic life, such functions really implying both susceptibility of perceiving and the power of generating and of conveying stimuli; in fact, the process of reflex action, which can only be performed by a complicated apparatus. It is, therefore, in relation to the admixture of fibres of different portions of the nervous system that the ganglionic system, as it appears in non-microscopic anatomy, is to be judged, and, if so, its size corresponds to its function, which would not be the case if all we saw were the sole seat of vitality for the whole body. We must also notice what Fletcher gives as one of the proofs of its function as the seat of vitality, viz. its early appearance and great predominance of size in the foetus and also in the young over the same animal as life advances, exactly as we would expect of a substance through which formation of tissue and organs as well as all other vital functions is performed. Whatever we may think, in respect to systematic anatomy, of Fletcher's theory of the functions of the "great sympathetic," there is no doubt that his hypothesis of the physiological nature of the ganglionic nervous matter is very remarkable and well worthy of attention. It amounts simply to this, that living beings are living beings solely in virtue of their possession of a "pulpy,

translucent, homogeneous matter, yielding, after death, fibrin" (p. 106), which is so universally distributed and interwoven with all organised tissues as to form a considerable part of their bulk.

§ 79. We have here a physical basis for vital action, for it was scarcely conceivable that anything exhibiting structure or firmness of texture beyond that of a mere pulp could possess the requisite degree of molecular mobility for the display of the rapid and almost continual changes which go on in a substance whose molecular actions are so complex. But although the irritable matter possesses certain characters in common in all parts, not only of the same animal, but in all animals and plants, thus manifesting a generic unity, yet, as before said, there are differences in the properties of each animal and plant, and each part of all individuals, implying a corresponding difference in the composition of the irritable matter—facts embodied in the doctrine of "specific irritability," which asserts that "every organised solid is not only the seat of irritability in general, but has a kind of irritability peculiar to itself" (p. 52); which specificity applies to all its faculties, viz. the plastic, metabolic, and germinative. Further, all changes of the vital properties must be rigidly traced to corresponding changes in the quantity and composition of the irritable matter.

"From what has been above said of the nature of irritability and irritation—or irritability called into action by the requisite stimuli—it will readily appear that the practice of speaking of irritability or vitality as something substantial, something which may be *per se* accumulated, diminished, exhausted, is erroneous; but the error amounts merely to this, that, for shortness sake, we speak of the *property* as directly so affected, whereas it is, in fact, the *substance* of which this property is the attribute that is so, the property, however, manifesting, of course, corresponding changes, since the degree of irritability in any part will necessarily be in the direct ratio of the quantity and quality of the irritable matter which it contains" (p. 125, ii b).

Every vital action presumes a metabolic change in the irritable matter, and when that change results in the formation of tissue or secretion there must be a transformation

of matter from the metabolic to the chemical state, and thus consumption of the irritable, living, or diffused ganglionic nervous matter must take place. I may add that the same, with satisfaction of stronger affinities, must take place when any extrication of force takes place, either for work or heat or the internal stimuli. At the same time, as the living matter is consumed, it is continually renewed from the blood, or other nutrient fluid, in the same proportion as it is consumed, or in greater proportion, if growth of living matter is going on. When these processes are exactly balanced health is the result, but any excess, deficiency, or perversion of the pabulum or stimuli disturbs the balance, and disease, and beyond certain limits death are produced. These changes take place in the irritable matter itself, independently of a vascular system, both as respects the stimuli as well as the pabulum, but the action of the stimuli is more perceptible in the higher animals, where they can control the supply of pabulum; accordingly, Fletcher states it as a principle that the formation of the living matter itself, and all secretion and nutrition depending on it, are, *cæteris paribus*, in the direct ratio of the quantity of blood in the capillary vessels. These last, therefore, play the most important part in changes of nutrition and secretion and in inflammation, although still all vital action in them is exclusively seated in the living matter interwoven with the tissue proper. Quantitative variation, either by excess of production or by exhaustion from over-consumption of the living matter, has thus for its chief factor the abundance of the supply of nutrient matter.

Mere change of quantity, however, is not the only variation that can take place in the irritable matter, but also change of quality from the action of excessive or abnormal stimuli or insufficient or abnormal pabulum. In this case disease and death are said by Fletcher to result from deterioration of the ganglionic nervous matter in any tissue or part when local, or the whole body when general. This *deterioration* of the irritable matter from excessive stimuli plays an important part in Fletcher's system when he comes to

pathology, for it applies not only to the metabolic faculty in altering secretion and nutrition, and thus giving rise to a variety of morbid products all of which display the character of lowered vitality; but also to the germinal facility in producing living matter of a degenerated character.

This last is very important, and rests on the principles insisted on frequently, that secretion, nutrition, and germination are all intimately connected as varieties of vital action, that new formations and growths are not deposited from the blood, but consist in "a proper germ or matter secreted by the vessels, and containing within itself the rudiments of its own organism" (p. 142). Again, "the organisation of the embryo was the work of its parent, its organogenesis is its own, and parts which it received from the former in a state of ambiguity and diffusion itself renders concentrated and determinate." (II, p. 24.) That the germinal faculty is not confined to the reproductive organs of the individual, has been stated at the note to § 66, p. 504. The dependence of the germinal faculty on stimuli, just as all the other vital faculties, is, likewise, enforced by sufficient reasons, and all spontaneity or autonomy of the living matter denied in this any more than other instances. In short, every function of living matter is reduced to dependence on adequate causes, although these are as yet mostly quite unknown, exactly in the same way as the phenomena of the inorganic world. Fletcher does not hesitate to uphold and push to its legitimate conclusion the Brunonian doctrine, that all positive agents act as stimuli on the living matter, and are essential to the actions we call life. There are no direct sedatives, and all deleterious agents, even the most rapidly destructive, such as excessive heat, electricity, hydrocyanic acid, and serpent poisons, kill by "deterioration" of the living matter to such a degree that renewal from the pabulum is impossible, or else depress temporarily by excessive constriction of the capillaries, shutting off the supply of pabulum for a time. For this latter reason the primary excitant action of all stimuli is generally overlooked, and in fact forms, according to Fletcher, the latent stage of acute disease and the action of poisons, because, owing to the capillary constriction, the increased formation of irritable matter that would take place cannot do so owing to the deficient supply of blood. Consequently till that is allowed by the relaxation of the capillaries (or what corresponds to that, the relief of the spasm, allowing the return of the peristaltic action), the effect of stimuli in morbid increase of growth of living matter, or of its deterioration in altered quality, cannot be manifested. The so-called physiological or pathogenetic effects of poisons and medicines, and of the exciting causes of disease, belong, therefore, to the *second stage* of this operation, and that

stage is always one of lowered vital action, or both lowered and deteriorated action. Hence the important therapeutic principle, that all remedies acting on the diseased part act as stimuli to it, either indirectly, as counter-irritants through the nerves of organic sympathy, or directly, as specific stimuli adapted by their specific properties to act on the specific irritability of the part or the specific character of the deterioration. The subordination of the germinal faculty likewise to the action of stimuli is of great moment for the hope of therapeutics, as it is only, according to Fletcher, by "a regeneration of the irritable matter" (p. 145) that recovery can take place. The principle of deterioration of the living matter by morbid causes acting on the germinal faculty in particular throws light also on the operation of the remote causes of disease.

In accordance with these views, I wrote a paper in April, 1868, explaining the nature of dormant disease, by referring it to a gradual change in the composition of the living matter induced by the slow operation of abnormal stimuli or pabulum of a specific character. This change, not being sufficient to cause quantitative disturbances in the living matter, may go on for long without any marked disturbance, till at length the supervention of any of the occasional non-specific exciting causes of disease, such as cold, injuries, digestive disorders, &c., produce their usual acute and quantitative disturbance, and this combined with the previous alteration of the quality of the living matter gives the disease its specific character—such as gout for example. I proposed, therefore, to define dormant disease as that in which the qualitative is dissociated from the quantitative element, and considered that it is always of a lowered or degenerated character in comparison with health.

These few remarks on the bearing of our subject on pathology and therapeutics may serve to show the harmony between Fletcher's views and those of late becoming generally prevalent. This could be done also to a much greater extent if necessary, but probably the above will suffice, and we shall now conclude this part of the subject by stating the singular dilemma into which Fletcher now falls.

§ 80. He speaks constantly of the vital functions of the several tissues, while, at the same time, life is with him merely a series of actions depending on the properties of the substance in which they take place, and, therefore, which cannot be communicated or conveyed in any degree. With him, as regards the higher animals, the parenchyma or capillary tissue was the "seat of all the molecular actions

of the body ;" thus including secretion and nutrition not only of all other tissues but of the ganglionic nervous matter itself.

" But the ganglionic nervous tissue, assuming this as the immediate seat of the property in question, is, of course, continually renewed and consumed, like all the other tissues of the body, by molecular processes, the seat of which is the parenchyma ; which thus effects incessant changes in this tissue, not only as interwoven with the substance of every other organ, but also as entering into the composition of the minute vessels of which itself consists " (p. 125).

How can the parenchyma or the capillary walls take any vital part in the formation of the substance, whose presence alone constitutes their vitality, and yet which properly cannot be transferred to the smallest distance ? Speaking in the present language of the force-doctrine, the metabolic power is not the offspring of a force, and therefore cannot be conveyed or transferred through other kinds of matter or the interstellar æther so as to operate at a distance, but it is the result of an affinity proper to one particular aggregation of matter alone, and can, therefore, only act upon particles within that, nor can any catalytic action take place between portions of matter in the metabolic and the chemical states of combination respectively. The difficulty was not overlooked by Fletcher, who says (p. 92, II) :

" With respect to the manner in which irritability or vitality—presuming that its immediate seat is, at least in all the higher tribes of organized beings, the ganglionic nervous tissue—may be supposed to be extended to the other organized tissues, so that each appears to be *per se* possessed of it, very little needs be said. In the case of sensibility, the immediate seat of which is the sensiferous nervous tissue, there is no difficulty in at once admitting that this is the exclusive seat of the property in question, and that the comparatively few other tissues which appear to be *per se* possessed of it, are, except in as far as they contain this, entirely insensible. But with respect to irritability, we may have more hesitation in confining this property to the ganglionic nervous tissue, since irritability is like sensibility, not merely an adventitious, but an essential property of organized beings ; and it seems, therefore, to be necessary not only to every organ and every organized tissue, but also to every point of such tissue, in order to effect its molecular actions. But we must remember that there is no point of any organized tissue which does not contain ganglionic nervous

matter, such matter being essential to its organism; and consequently there is no point which is not possessed of the property in question, which is accordingly, as has been from the first insisted on, the result of such organism. It is true, unless every organized tissue be one uniform mass of ganglionic nervous matter, there must be interstices of some other matter. Thus there must be spaces in the ganglionic nervous tissue occupied by one or other of those peculiar aggregations of matter which go to form respectively the cellular, dermoid, mucous, serous, fibrous, vascular, osseous, cartilaginous, or muscular tissues, wherever an organized part presents itself. But the spaces thus occupied are almost infinitely minute, and such as to be perceived only by the mind's eye; and we may, therefore, without doing much violence to preconceived opinions, deny that the tissues occupying them have any direct participation in irritability or vitality. As merely a property, it can never be directly transferred; and if it be immediately it must be exclusively that of the ganglionic nervous system. But such interposed substances, thus destitute *per se* of irritability or vitality, are not, nevertheless, in the condition of merely inorganized matter—of a mere 'solidum mortuum,' with respect to either their structure, their composition, or their actions. It is never otherwise than as combined into a whole with ganglionic nervous matter that they actually exist; and although the former alone perhaps has a structure and composition which can be strictly called vital, and is alone susceptible of vital changes, still the latter are not less *sui generis* in these respects, possessing as they must do such physical and chemical characters as qualify them to co-operate in these changes, and thus to temper and modify in every part the resulting phenomena. It is easy to conceive that, if a flame be applied to a mixture of combustible and incombustible matters, the former alone will burn, but the latter may still be competent to undergo, each according to its peculiar character, such modifications of expansion, liquefaction, vaporisation, incandescence, and so forth, as will materially qualify the nature of the combustion, and may even be essential to it. It is in a similar relation that the substances occupying the interstices of the ganglionic nervous matter, in the several organized tissues, appear to stand to the nervous matter itself—they have *per se* no irritability or vitality, nor are the phenomena which they display primarily vital; but they are, nevertheless, such as no merely inorganized matters could have presented, and such as not only temper and modify vital action, but are perhaps necessary to render such action available."

It cannot be said that this is satisfactory, nor is the doctrine put forward as otherwise than a hypothesis resting upon presumptive evidence alone. That evidence was, however, so strong that a man of powerful intellectual

grasp like Fletcher did not hesitate to accept the conclusion to which it pointed, in spite of the apparent inconsistency involved therein, in the persuasion that time and the progress of discovery would realise what was true in the previsions of deductive reasoning by giving us new facts to fill up the blank and explain the apparent inconsistency.

The pupils and disciples of Fletcher have waited well nigh thirty years in the hope that such a time would come, although, perhaps, not in their day. But the time has come, and with it the explanation, which in some respects is so simple, and so completely in accordance with Fletcher's whole theory of the living state, that it strikes us as strange that we did not see it sooner, and now we can hardly state Fletcher's position without falling into the language of the new theory of growth and nutrition of tissue.

(To be continued.)

A CASE ILLUSTRATING ONE OF THE ACTIONS OF SILICA.

By MR. ALFRED C. POPE.

(Read before the British Homœopathic Society, 1871.)

THE case, my brief notes of which I am about to read, has, I trust, sufficient of interest about it to justify my occupying your time for a few minutes. Its nature is somewhat obscure. The disorder had existed for a considerable length of time—had been more than once relieved by a remedy *partially* homœopathic to it, and, finally, was rapidly cured by one which commonly excites the ridicule of medical men who know but little, and refuse all clinical investigation of homœopathy; while the dose used was—if we can rely upon the late Dr. Lehrmann's integrity in the preparation of his dilutions—one I very rarely prescribe—one in the power of which to control morbid action a large proportion of physicians who have, within a certain narrow range, perfect

confidence in the life-modifying power of diluted medicine, have not the least faith.

H—, a domestic servant, æt. 16, came under my care at the Blackheath Dispensary, October 27th, 1869.

Eighteen months ago she fell while going upstairs, and sustained a severe blow over the sacrum and left hip-joint. After the first shock of the injury passed off she felt but little inconvenience for nine months, when pain in the site of the original injury became somewhat severe. She then rubbed in a rhus liniment, and was relieved for a time. The sense of pain returned again two months before I saw her, and was again for a while relieved by rhus liniment.

At present she is suffering much from pain. It occupies the sacrum, extends upwards to the lumbar vertebræ, and across the left ilium to the superior spinous process. The pain is throbbing in character, is much increased on pressure over the lumbar vertebræ, and this, when severe, gives rise to a pain in the nape of the neck, and a sense of loss of power in the lower extremities. It is aggravated by moving quickly. When stooping to lift any ordinary weight, a severe pain is felt darting up the spinal column. The back aches much when, and the usual pain is considerably increased by, walking any distance. Occasionally, when walking rapidly, a pain is felt darting down the left thigh and leg.

R. Glb. Silicæ 200, xij ;

Aq. Dest., ℥iv.

Direct, a tablespoonful to be taken every four hours.

She called again in eight days. The pain she said had been much less; she was stronger and better able to lift anything. The same prescription was repeated.

On the 10th of November—fifteen days after her first visit—no pain was felt on pressure; she could lift or walk without any pain; in short, she said she felt well. No further medicine was given. She called at the dispensary to report herself on the 17th of November, and said she had had no return of pain. She came again a month later, and stated that she had continued perfectly free from pain,

and that whereas, when considering herself well, she had always had more or less suffering when lifting anything, she was able to do so now without the least indication of pain.

Satisfactory as was the termination of this case, striking as was the relief which followed on the use of so utterly infinitesimal a dose of a substance which, in the crude state, is usually held to be inert, and complete as was the recovery—its exact nature, the precise pathological condition which gave rise to the pain, is sufficiently doubtful to justify a little speculative comment.

The first thing to attract one's attention is the length of time which elapsed between the receipt of the injury, to which the suffering endured was traced, and the occurrence of that suffering. The shock of injury, the bruising of skin and muscle, and—for a time—of ligament, passed away, and for nine months no further inconvenience was felt than some little aching when stooping, so little as not to excite any attention, or to induce resort to any remedial measure. After nine months, however, we have pain—throbbing and aching in character—in the site of the original blow, and radiating therefrom upwards, and to the left ilium. It is further to be noticed that this pain is increased when certain muscles are put into action—certain ligaments are stretched, viz. those brought into play in the acts of stooping and of motion; and, again, when this latter act is performed rapidly, a darting pain is felt extending down the left lower extremity; and, finally, when pressure is exerted somewhat rudely in the part where pain is more or less constantly present, a reflex pain is felt in the nape of the neck, and some degree of temporary loss of power is experienced in both extremities.

The tissues exposed to injury from the fall were those of skin, muscle, ligament, bone, and nervous matter. Our inquiry then is, which was the texture in which the consequences of the blow were most permanent? What and where situate was the morbid process for which she sought, and from which she obtained relief?

Doubtless at the outset the muscular structure was more or less bruised, the ligaments connecting the lumbar

vertebræ and the sacrum, with the ilium and ischium, were injured, and in all probability it was these latter structures that obtained relief from the rhus liniment. But more mischief must have arisen than this. And when we consider the slowness in the development of the more enduring pain, and its throbbing and aching character, I am disposed to think that a subacute, but unarrested, inflammatory action, with its plastic exudation, must have been set up in the periosteum of the sacrum and posterior portion of the lower lumbar vertebræ. It is true that in the long bones the progress of inflammation is from the commencement rapid and excruciatingly painful. In the vertebræ and sacrum it is otherwise. The structure is not so dense, and pressure is longer in making itself felt. Hence, I think it probable that there may have been some thickening of the periosteum of the posterior part of the sacrum and of the spines, and foramina of the lower lumbar vertebræ. Such a theory accounts for the character of the pain, for its increase on pressure, on motion, and for the temporary loss of power in active movement, by reason of the temporary exposure of the nerves passing through the foramina to increased pressure.

That the disorder was meningeal or medullary is, I think, rendered very improbable, from the length of time that elapsed between the receipt of the injury and the experience of its effects, as well as by the quality of the pain, the absence of any twitching or spasmodic movements. Therefore, while feeling that the diagnosis is one largely conjectural—dependent on “probabilities,” as Dr. Wilks puts it, I think it probable that the condition existing at the time I first saw this girl was one of traumatic periostitis of the lower lumbar vertebræ and of the sacrum.

As to the means of cure. The first remedy that would have suggested itself would have been rest. But the patient was a housemaid in a large school. For her to rest was impossible. There were no indications for altering her diet, which was simple enough. The ligamentous injury had been subdued by *Rhus*. No external application was called for—possibly a compress might have done good—but the

position scarcely admitted one being worn. The only means left for modifying the unhealthy nutrition of the part was by a medicine administered internally. The taking of a medicine, be it observed, was the only alteration made in her mode of life. The medicine taken was *Silica*.

The pathogenesis of *Silica* is an unsatisfactory one. It is profuse in unconnected symptoms. Here and there we come upon a few which, taken collectively, point with more or less definition to certain forms of disease, and clinical evidence has sustained the accuracy of the conclusions they have suggested. But it is often difficult to "cover" a case, as it is termed, with *Silica*. So here, it was rather from the idea that I had to deal with a chronic thickening of periosteum than from any precise analogy between the symptoms of my patient and those occurring in the provers of *Silica* that I was led to give it.

The only symptoms of *Silica* recorded in Hahnemann's proving which have any likeness to those of the case I have described are: "Pain of the os coccygis as after a long drive;" this I presume may be read as a bruised and aching feeling of the sacrum. "Violent pain in the back;" that pathologically may mean almost anything. "Paralysis in the small of the back;" that, again, may mean simply weakness. "Pressure and tension in the small of the back." "Beating in the back;" that I take to represent throbbing. *Silica* also gives rise to a good deal of pain in the nape of the neck; but this appears to depend rather upon the headache excited than, as in my patient, upon pressure from below. There is but one more symptom which by any degree of straining can be made to fit any that occurred in this case. It is "pain in the left hip when stooping for a quarter of an hour."

Doubtless, had we the diaries of the provers, much that is now dark and largely incomprehensible would become clear and be easily understood. But we have them not, and hence must fall back upon the general hints thrown out in the course of the proving. That the nutrition of bone is greatly modified by *Silica* many symptoms indicate. Clinical observation, guided by generalisation, has confirmed its

value in cases of this nature. In the report of the Leopoldstadt Hospital, at Vienna, for 1867 (*Brit. Journ. of Hom.*, vol. xxvii), there are two cases of coxitis mentioned as having been cured by *Silica* 30, and one of periostitis on the inside and anterior ridge of the tibia from its upper third to within two inches of the ankle, when the patient recovered in six weeks under *Silica* 30.

My friend, Mr. Nankivell, of York, reports in the last number of the *British Journal of Homœopathy* a case of disease of the cervical vertebræ in an interesting and instructive manner, in the course of which though *Sulph.*, *Arnica*, and *Calcarea* were also occasionally and for brief periods prescribed, there can be little doubt that the recovery was due to *Silica*.

With regard to the dose I have nothing further to say than that it was the 200th dilution. The preparation used was that known as Lehrmann's, and I have always understood that his dilutions may be relied on as being what they are represented to be. I rarely ever use a 200th, and this chiefly because of the difficulty, I may say the impossibility, of being really sure that the preparation described as the 200th is the 200th. It is only when we deal with low dilutions and crude substances that we can speak confidently of the dose we have prescribed. Therefore I desire to lay no stress, and to attach no importance to the fact of the 200th having been the supposed dose. It is, I think, sufficiently remarkable that a recovery so rapid and so complete from the effects of an injury which must have been in progress for some months should have taken place from 24 globules saturated with any dilution of *Silica*.

ON THE URINARY SYMPTOMS IN OUR *MATERIA MEDICA*, REFERRING TO COLOUR AND SEDIMENT.

By DR. HAMILTON.

(Read before the British Homœopathic Society.)

IN referring to the symptoms of the urinary organs described in the *Materia Medica*, their very unsatisfactory state as regards colour and sediment of the urine, cannot fail to be observed by the inquirer. It is well known that colour and sediment may occur from the most simple derangement of the digestive organs, and also from the most important abnormal changes in the urinary organs themselves, and yet there is no distinctive difference between these two opposite states indicated; no examination made as to the cause of change of colour or of sediment except by the eye; no record taken of when the urine was passed; the quantity and quality of food taken; whether the person was of sedentary or active habits; what had been taken in shape of liquids; the state of the atmosphere, &c.

Dr. Müller, in a paper "On the changes of the Urine," published in the *British Journal of Homœopathy*, 1859, says: "It is a question whether our *Mat. Med.*, founded on provings on healthy individuals, contains a sufficient supply of corresponding urinary symptoms, and whether in our investigations extensive, or even a bare sufficiency of attention has been paid to the changes and peculiarities on the urine caused by substances under investigation."

This is a very important question, and, were it necessary to give a decided negative, we should be compelled either to give up for the present all practical application of such symptoms, or at least to avail ourselves of them to a very small extent.

Unfortunately the answer to the question cannot be very favorable, especially at the first glance; for we cannot but acknowledge the imperfect attention which has been paid to urinary symptoms in our provings. With very few

exceptions, it has not occurred to any of our investigators to study the urine chemically or microscopically, and their descriptions of the secreted and evidently abnormal urine are confined to superficial and uncertain notices."

As an example of this superficial and uncertain mode of describing these symptoms, take the proving of *Ambra grisea*.

"Symptom 205. Urine of a yellow citron colour, nearly without odour, with a slight cloud. 206. Urine brown. 207. Urine like whey. 208. Urine with a red cloud. 209. Urine with a brown sediment, that floats in a liquid yellow and clear. 211. Bloody urine. Urine of a dark brown colour during the emission."

Here we find symptoms enumerated of the most diverse character, which may involve serious organic change, or be the result of some slight derangement of the digestive organs. For instance: "Symp. 205. Urine of a yellow citron colour, nearly without odour, with a slight cloud." Now this may be only the result of slightly impaired digestion resulting from eating a full meal; or it may be much more serious; that slight cloud under the lens of the microscope may be oxalate of lime, or under the action of the spirit lamp and nitric acid may indicate the presence of albumen.

"Symp. 206, 207. Urine brown; urine like whey." Symptoms again indicating very important changes. Is the brown urine bile or blood? Is the whey-like urine, albumen or earthy phosphates? Chemical analysis and the microscope alone can decide, in each case requiring totally different modes of treatment; and is it probable that *Ambra grisea* would produce similars to both?

I do not say that the action of these drugs will not produce the symptoms recorded; they probably might do so. But from the mode of investigation it is uncertain, and we require fresh provings on a more scientific plan.

First, then, as regards colour of the urine. What is meant by colour in the urine? In the enumeration of these symptoms we must, I think, conclude that colour means the appearance of the urine as a whole, independent

of sediment, which is treated separately; and yet the confusion is such that it is most difficult to ascertain if this is the case. However, we must take them as recorded.

Colour of the urine may be the result—

1st. Of natural conditions.

2nd. Of excess of those natural conditions.

3rdly. From other abnormal conditions, as brown urine, from concentration, the result of fever, &c.; produced by blood or bile, the result of obstruction; or of presence of these elements in the urine from organic change; or by purpurine, producing red urine; from portal congestion, with organic disease of liver or spleen; or red urine, from blood, from hæmorrhage of some part of the urinary apparatus. Greenish urine, or greenish-brown urine, from the same cause. Grass-green urine, from the presence of cystine or from excess of *Sulphur*.

We may also have a change of colour from the normal state; from accidental colouring matter, either from the food or drink; and the same may be said as regards odours. Certain vegetables will impart both colour and smell—as asparagus, for instance.

Therefore, to be certain that the symptoms recorded are the result of the action of a certain remedy taken, all these accidental circumstances should be carefully noted, but this evidently was not done, and the consequence is we have a jumble of symptoms which it is difficult to recognise as medicinal. We have thus blackish urine, which is produced by *Colchicum*. Is it bile or blood? *Colchicum* also produces whitish urine; is it mucus, or phosphates, or albumen?

Brown urine is recorded under *Acon.*, *Amb.*, *Ant.*, *Arnica*, *Arsen.*, *Asarum*, *Bell.*, *Bry.*, *Calc.*, *Colc.*, *Dig.*, *Dros.*, *Lach.*, *Merc.*, *Nitric acid*, *Petrol.*, *Prun.*, *Puls.*, *Sulph. acid*, and *Tart. emet.* Now, is it probable that all these medicines in their proving produce the same kind of brown urine? here, again, how fallacious and confounding such a symptom is. Brown urine may be caused by lithates, phosphates, bile or blood, &c.; may be the result of indigestion simply, or serious lesion of kidneys or liver. The record of such a

symptom in this vague manner is, to say the least, very unsatisfactory.

Clay-coloured urine is recorded under *Anacard.*, *Berb.*, *Ferr.*, *Magn.*, *Sabadilla*, *Sep.*, *Sulphur* and *Zinc*. Clay-colour may be produced by the earthy phosphates, or may be a mixture of pus and albumen: one a passing derangement produced by the depurating action of the kidney, the other the result of lesion of kidney or bladder, or both.

Milk-white urine, or like butter-milk, is recorded under *Aurum*, *Berb.*, *Iodium*, *Phosphoric acid*. Such a condition of the urine indicates albumen, or some of the earthy phosphates, a point to be decided only by chemical analysis and the microscope.

Again, we have white or whitish urine recorded under *Alum.*, *Amm. carb.*, *Berb.*, *Carb. veg.*, *Cycl.*, *Dulc.*, *Merc.*, *Phos.*, *Secale*, *Sulph*. What does white or whitish mean?

Again, white, turbid, or dirty under *Cannabis*, *China*, *Conium*, *Cycl.*, *Rhus*.

Under reddish, *Acon.*, *Ammon.*, *Ant.*, *Bell.*, *Berb.*, *Bryonia*, *Calc.*, *Camph.*, *Cannabis*, *Canth.*, *Cact.*, *Caustic*, *Colch.*, *Con.*, *Daphn.*, *Dig.*, *Dulc.*, *Fer.*, *Mag.*, *Grat.*, *Hæm.*, *Hep.*, *Ipec.*, *Lact.*, *Merc.*, *Nux vom.*, *Petr.*, *Sel.*, *Sep.*, *Squill*, *Staph.*, *Sulph.*, *Sulph. acid.*, *Tab.*, *Tart.* Blood-red: *Bell.*, *Calc.*, *Veget.*, *Merc.*, *Rhus*. Red and blood red. Lithates, Purpurates, or blood?

Greenish urine is recorded under *Arsen.*, *Camph.*, *Iod.*, *Magn.*, *Ol. an.*, *Rhab.*, *Ruta*, *Veratrum*.

Colour, then, as a symptom, in the *Materia Medica* is most vague and unsatisfactory. It is of no use in indicating the *modus operandi* of the medicine under which it is recorded; it is impossible to define it separately from sediment, and it is confusing, as leading the inquirer to false conclusions.

The symptomatologist may say, but it is only as one of the totality of the symptoms. But if the one symptom is given under false premises, it is utterly valueless in leading to the choice of the remedy, as the symptoms recorded may be the result of other causes independent of the drug.

Now, as to sediment. Here we have more confusion and uncertainty as to causes, &c.

Thick brownish sediment, recorded under *Digitalis*.

Loose yellowish resin-coloured sediment.—*Chin. sulph.*

Sediment resembling white flour.—*Sulphur, Calc. carbon.*

Sediment like flour in watery urine.—*Tart. emet.*

Sediment brown green, with strong pale urine.—*Chin. sulph.*

Sediment brick coloured.—*Puls.*

Brickdust sediment.—*Op., Phos., Chin. sulph., Acon.*

Sediment reddish.—*Kreas., Squill., Sulph.*

Sediment copious, thick, red.—*Sec. cornutum.*

Firmly adhering, bright red sediment after standing.—*Nitric acid.*

White sediment.—*Alum., Caps., Euphorb., Magnes., Carb., Petrol., Senega, Sep., Spigelia, Sulph. acid.*

Thick white sediment.—*Bell.*

Thick greyish-white sediment.—*Spongia.*

Snow-white sediment.—*Rhus.*

Whitish sediment, with ammoniacal odour.—*Nitric acid.*

Urine leaving white spots.—*Tart. emet.*

Pale urine deposited white crust on side of vessels.

Urine deposits a pink sediment, with small brown crystal like tuft.

Copious saturated urine, depositing crystals.—*Chin. sulph.*

Pale urine, with a light brown transparent flocculent sediment, depositing small red hard tenacious crystals on the sides of the vessels.—*Colocynth.*

Urine with a little cloud at the bottom of the vessel, which disappears gradually, in the place of which form little crystals, which are all deposited on the sides of the vessels.—*Crot. tgl.*

Urine depositing a number of bright red crystals.—*Ferr. muriat.*

Urine with a violet foam-ring on the top of sandy sediment.—*Pulsatilla.*

Urine clear, with red sand.—*Lycopodium.*

Brown-red gravel in the urine.—*Nitric acid*.

Gravel.—*Chin. sulph.*, *Ruta*.

Foaming urine, with a sediment of fine yellowish sand after cooling.—*Chin. sulph.*

Urine with a white sandy sediment for several days.—*Amm. Carb.*

Reddish-yellow sediment in the evening.—*Natrum sulph.*; in the morning yellowish white.

Yellow sediment.—*Spong.*, *Silic.*, *Phosp.*

Dingy yellow loose sediment.—*China*.

Fatty substance deposited by.—*Asparagus*.

Yellowish-white sediment.—*Phosph.*

Violet-red sediment.—*Pulsatilla*.

Violet-coloured sediment by *Magnesia*.

Jelly-like sediment.—*Pulsatilla*.

White flocculent sediment.—*Asparag.*

Urine deposits whitish flocks.—*Phosp.*, *Zinc*.

Reddish cloudy sediment.—*Ambra gris*.

Bloody-red filamentous sediment.—*Tart. emet.*

Small red bodies in the urine after standing.—*Ant. Crud.*

Now, here is a long list of various sediments of all colours and kinds, attributed to the action of certain remedies on the body in health.

Let us see how much can be attributed to that cause, to the condition produced by digestion, to the effect of colouring matter passing by the kidneys, and to a healthy state of the urine.

Sedimentary deposits from the urine may be divided thus:

1. Those formed directly or indirectly from the metamorphosis of tissues, or from organic elements of food capable of assuming a crystalline form, viz., Uric acid and Urates, Uric oxide, Oxalate of lime, Cystine.

2. Inorganic deposits, as Phosphate of Lime, Ammonio-phosphate of Magnesia, Carbonate of Lime, Silicic acid.

3. Deposits of highly coloured black or blue of doubtful origin, as Cyanourine, Melanourine, Indigo, Prussian blue.

4. Deposits consisting of non-crystalline organic products, including

1. Organized, as blood, pus, mucus, organic globules, epithelium, spermatozon, confervoid bodies, vibri-ones.

2. Non-organized: Milk, fatty matter, Stearolith.

Healthy urine is composed of the following, or rather made up of the following, ingredients.

1st. Organic products, as urea, uric acid, creatine, creatinine, colouring and odorous principles; these are derived or produced by the destructive assimilation of tissues, and separated from the blood by the kidneys; also, in addition, chiefly from the food during the process of assimilation, hippuric acid, lactic acid, and accidental constituents of various kinds.

2nd. Sulphates, phosphates, Chloride of Sodium, and all soluble salts taken with the food, and often undergoing decomposition in the system; these are saline combinations separated from the blood, and derived chiefly from the food; also sulphates and phosphates derived from the process of destructive assimilation.

3rd. Mucus from the bladder; débris of epithelium; Phosphate of Lime.

Of these, the first class can alone be considered as really essential to the urine and characteristic of it as a secretion, the kidneys being the only organ that eliminates these ingredients from the blood. The saline ingredients of the second class are met with in most secretions of the body, with the exception of sulphates, which are rarely found except in the urine. The third class are met with in all fluids passing over mucous surfaces, the Phosphate of Lime being derived from the mucus, of which it is a constituent part. These facts are derived from the late Dr. Golding Bird's analysis.*

There is also a great difference in the amount of organic matter and fixed salts, in the urine of men and that of women, *e. g.*—

* *Urinary Deposits: their Diagnosis, Pathology, &c.* By Golding Bird. 4th edit.

Grains in 1000 parts.

In men, as 150 grains of fixed salt to	126 in women.
„ 176 „ of organic matter to 145 „	
„ 270 „ of urea to 240 „	

Also very slight causes, either from food, exercise, state of general health, &c., tend to vary the proportions of the different ingredients of the urine, solids increasing in direct ratio with the amount of muscular exertion, and consequently alteration of tissues, and inversely with the length of time elapsing after taking food.

Dr. Bird gives an analysis and example of the great variation existing in the composition of urine passed at different times of the day. He collected carefully all the urine secreted by a person in good health during twenty-four hours; it amounted to twenty-two ounces; he had previously drunk very little. It was passed at the following hours:—At 8 a.m., eight ounces, depositing urate of ammonia; at 1 and 5 p.m., six ounces; at 11 p.m., eight ounces; all these specimens were acid. The first of these was passed after being ten hours without food—a good specimen of *urina sanguinis*. The second was influenced by breakfast and lunch at noon, and the third, containing the products of the metamorphosis of food taken at dinner early in the evening, *urina cibi*.

The composition of these three specimens was in 1000 grains—

	A	B	C	D	E
The urine at 8 a.m. Urina sanguinis.	8 a.m.	noon.	11 p.m.	D	C
Specific gravity	·1016	·1020	·1030		1·018
Water	962·72	953·40	930·10		958
Solids	37·28	46·60	69·90		41·94
Urea	14·3	15·3	24·4	180·4	11·0
Uric acid	0·28	·09	1·33	8·0	1·1
Fixed salts	5·1	16·5	9·9	98·0	11·2
Creatine, creatinine, colouring and volatile saline compounds	17·6	13·9	34·27	213·0	18·6

The bulk of the urine secreted was nearly one half the average, being but twenty-two ounces; and the composition of the solids existing in the whole quantity is shown in the column D of the above table. In the following twenty hours, the same person having partaken freely of fluids, secreted thirty-six ounces, and the composition of 1000 grains is shown in column E. (Bird, loc. cit.)

These facts prove that very great changes, as regard sediments in the urine, take place solely from the condition of the system either before or after a meal, or from taking too much or too little food or liquid. How, then, can we place the symptoms of sediment and colour recorded in our *Materia Medica* to the pathogenetic effect of the drugs under which they are recorded. Take, for instance, *Pulsatilla*; we have the following symptoms recorded:—Urine red from time to time; urine of a red brown; urine of a dark red without sediment; urine surrounded with a circle of violet froth with sandy sediment; gelatinous sediment in the urine; urine with violet sediment; urine with a red sediment; urine with brick-dust sediment; without any record as to when or how these symptoms occurred.

Most of these common sediments exist from conditions totally distinct from either medicinal action or any abnormal state of health. I do not say that *Pulsatilla* may not induce this state, but we cannot be certain from the loose manner in which these symptoms have been recorded.

Dr. Golding Bird's explanation of the common urate of ammonia sediment so constantly found is the result of chemical action. Uric acid at the moment of separation comes in contact with the double phosphate of soda and ammonia derived from the food, forms urate of ammonia evolving phosphoric acid, which thus produces the natural acid reaction of urine. If the whole bulk of the urine be to the urate of ammonia found not less than about 270 to 1, the secretion will, at the ordinary temperature of the air, remain clear; but if the bulk of fluid be less, an amorphous deposit of the urate will occur. On the other hand, if an excess of uric acid be separated by the kidneys it will act

on the phosphate of soda of the double salt, and hence, on cooling, the urine will deposit a crystalline sediment of acid sand, very probably mixed with amorphous urate of ammonia, the latter usually forming a layer above the crystals which always sink to the bottom of the vessel. (Bird, loc. cit.)

Here we have proof that the conditions found under many of the symptoms recorded under *sediment* is the result of a chemical action necessarily going on for the purpose of health, and can be attributed to nothing else but from the chemical assimilation taking place as a natural result.

To make, therefore, correct provings of medicinal substances on the urine, it is essential that a careful record be taken—

1st. As to the food and drink taken during the proving.

2nd. The time of day of passing the urine.

3rd. Chemical test and microscopic examination to ascertain the condition of the urine.

4th. The amount of exercise; the condition of the nervous system.

5th. Whether the prover be male or female, and

6th. Distinct and careful observation between colour and sediment.

That these necessary cautions were not taken by the provers in the *Materia Medica* is very evident, indeed chemical and microscopical science had not developed itself to that extent compatible with careful investigation, and therefore some excuse may be advanced. At the present time more care is being taken, as we find, in the *New Remedies in Homœopathic Practice*, by Dr. Hale, of America.

Under the proving of *Asclepias*, this subject has been carefully noted; a table given of the quantity of urine as the normal standard, the specific gravity, solid matter, &c., and then the same conditions under the action of the *Asclepias*, with the following remarks:—"By this it will be seen that *Asclepias Syriaca* notably increases the solid matters of the urine. This is an important fact, for, from careful experiments by Dr. Hammond, it appears that *Colchicum* actually increases

the organic and inorganic solids of the urine ; while *Digitalis*, *Squills*, and *Juniper* increase only the inorganic matter. Now, as it is the organic matter which is generally considered as contaminating the blood in disease, it is evident that *Digitalis*, *Squills*, and *Juniper* exert no effect in depurating the blood. *Asclepias Syriaca*, then, is a congener of *Colchicum*."

But we also found in some of the provers great want of care—for instance, under another medicine, the *Asclepia tuberosa*, we have the following undefined symptoms recorded:—Urine red to saturation ; looks as if mixed with blood. After standing a short time, small, dark-red, almost black points the size of a pin rise to the surface, and much mucus is deposited at the bottom of the vessel. No effort made to discover by scientific research the *actual* condition of the urine.

The proving of drugs now, as formerly, is not conducted with the absolute care necessary for the proper discrimination between drug-symptoms and those arising from other causes. It seems to me requisite that the individual proving the drug should submit himself to a careful record of symptoms which may arise previous to taking the drug, and that this record should continue for eight or ten days. The record should then be compared with that taken at the time when the drug to be proved has been administered ; by these means we should be pretty certain that the symptoms eventually retained were drug-symptoms, and we should get a much more accurate image of the effect of the medicine on the system.

It is surprising, until attention is directed to the subject, how many symptoms occur in a person in ordinary health which are quite unheeded and unattended to, but which, on a drug being taken for the purpose of proving, are put down as the effect of that drug. In illustration of this I will relate the symptoms occurring in two days to a person in good health, not taking any medicine, but whose attention was directed purposely to this end.

In the morning on awaking, slightly bitter taste in the mouth ; slight pain across the epigastrium, more on walking ;

pain across the small of the back. Slight frontal headache after luncheon. Lacerating pain in chest at times. Rheumatic pain in right upper arm. Itching in the hairy scalp. A kind of nervous pain in the nape of neck, running up to top of head at intervals.

Urine depositing a white sediment; phosphates; spec. grav. 130°0.

Next day, bitter taste in mouth in the morning; transitory pains about the body; smarting in the inner canthus of left eye; constant fits of sneezing; lancinating pain after evacuation of fæces; pain in epigastrium at times.

Urine depositing a red sediment lithates; spec. grav. 120°0.

I think it is evident that a great number of these false symptoms, which confuse the student and complicate the true symptoms, may with safety be discarded from the *materia medica*; and when we have such excellent observations recorded as in the paper on the "*Action of Belladonna on the Urinary Organs*," by Dr. R. Hughes, we may look forward to a time, I hope not far distant, when our *materia medica* will be a scientific record of pathogenetic symptoms of drugs, cleansed from the mass of non-medicinal symptoms which at present pervades its pages.

Discussion on Dr. Hamilton's paper.

Dr. DUDGEON said that Dr. Hamilton had doubtless pointed out a defect in the provings recorded in the *Materia Medica* of Hahnemann; but that when we took into consideration the physiological and pathological knowledge of his day, the urinary symptoms could scarcely have been more scientifically recorded. He would not say anything against the microscopical and chemical investigation of the urine in persons proving medicines, but he doubted if the characters obtained by such investigations were always so valuable as they were often considered to be. Chemical analysis of the urine did not in many instances teach us much, unless we had at the same time chemical analyses of the food and drink, of the fæces, perspiration, and other secretions and excretions. It was obvious that the class of persons whom we could

get to prove our medicines, more especially the non-professionals and women, could not be got to make such chemical analyses of all their secretions and excretions during their proving, and few professional men had leisure to make them; therefore, in most cases, we must rest content with the general physical characters of the urine, such as recorded by Hahnemann's provers. Very minute analyses of the urine had not hitherto been of a value in therapeutics proportioned to the trouble they gave and the hopes they had raised. He understood Dr. Hamilton to recommend not such minute analyses of the urine by provers as we know were instituted by some practitioners on their patients, but only that we should ascertain the more general characters of the urine during trials of medicines, such as its specific gravity, alkalinity, acidity, or neutral reaction; whether it contained sugar, albumen, excess of lithates or phosphates, &c.; and in this he entirely agreed, but he did not think it possible, nor even advisable, to go much further than this.

Mr. POPE said that there could be no doubt but that the symptoms were, as Dr. Hamilton had stated, too indefinite to be of much practical value. From a scientific point of view it was of course desirable that a much higher degree of exactitude should be attained than that which had been arrived at. But when we came to look at the urinary symptoms practically, this precision was scarcely so important as it appeared under other aspects. Much that Dr. Hamilton had alleged against the urinary symptoms, as they stood, might with equal force be brought against those of the stomach and bowels. Every foreign indigestible substance introduced into the body disturbed the conditions of these organs, and through them other secretions; of these, one was the urine. Hence it was that in nearly every proving, more or less alteration of the urine was observed. In these cases we relied upon much more specific symptoms to guide our selection of a medicine; which need not—save in kidney, bladder, and some diseases of the nervous system—be much influenced by the state of the urine. And in the case of these latter diseases, we had in many medicines well-marked and specific alteration of the urine recorded in the provings. Such medicines, for example, as *Arsenicum* and *Mercurius corrosivus*, which had produced albuminuria; *Turpentine*, which gave rise to acute congestion of the kidneys; *Cantharis* and *Cannabis*, which influenced the whole urinary tract. So that when urinary conditions gave real help to the physician in determining his medicine, we were pretty well off; and though, were scientific researches into the effect of medicines upon the urine to be more fully worked out, much valuable information might be derived, Mr. Pope doubted whether the imperfections which existed were of the magnitude which at first sight they appeared to be.

Dr. ROTH was pleased that Dr. Hamilton had called the attention of the Society to the deficiencies of the so-called materia

medica pura with regard to the symptoms of the urinary organs. Many years ago, Dr. Roth, of Paris, and later Dr. Laugheinz, of Darmstadt, had pointed out the desirability and necessity of purifying the *materia medica pura*, which contains many spurious symptoms, not only with regard to the urinary, but also to the other organs. Dr. Roth laid much stress on the importance of urinary symptoms, especially as it had been his task for many years to deal especially with chronic ailments; he is in the habit of applying to analytical chemists for minute analyses of the urine of chronic patients, and is frequently not unsuccessfully guided in his treatment by the knowledge of the quality and quantity of the various substances excreted by the kidneys.

Dr. BAYES said that he agreed with Dr. Hamilton that it is very important to note the variations of the urine during health. Before we can define what conditions of urine are caused by disease, we must first ascertain what variations are compatible with a fair average degree of health. Dr. Hamilton, quoting Dr. Golding Bird, had shown how greatly healthy urine may vary in its conditions at different parts of the day; but still more important are the varieties caused in the chemical constitution of urine by variations in the temperature, and by atmospheric changes, by changes in diet, &c. He (Dr. Bayes) had once a patient whose urine became albuminous if he eat heartily of piecrust; and recently he had seen a patient whose urine occasionally contained sugar, while, at other times, it was quite natural. Dr. Bayes agreed with Mr. Pope in considering that the urinary symptoms were often by no means essential indications for treatment, while in those cases in which they were of such importance as to become indications, our provings are clear and definitely useful.

Mr. J. H. SMITH said that Dr. Hamilton's interesting paper had shown that the urinary symptoms of the *materia medica*, taken alone, are of little therapeutic value as they now stand, and had proved the imperative necessity of re-proving the urine with the aids of modern science. He thought, however, that few practitioners would take as their guide to treatment the urinary symptoms alone (employing the phrase as in Dr. Hamilton's paper to mean the physical properties of the urine) as described in the *Materia Medica*, without also weighing the collateral symptoms referred to the related organs. The absolute necessity of re-provings was rendered evident by the fact, that those characteristics of the fluid to which medical men now attached the most importance, those revealed by the microscope and by chemical and mechanical tests, were entirely omitted in the provings of the *Materia Medica*. In fact there was no other organ, except, perhaps, the lungs, in regard to which the progress of science during the last fifty years had brought into the front rank so many symptoms formerly unobserved, as in affections of the kidney and abnormal conditions of its secretion.

Dr. VERNON BELL agreed in the opinion expressed by previous

speakers about the chaotic condition of our *materia medica*. The constant reiterations of the same symptoms which had substantially no difference except that of slight variations in phraseology, rendered the task of the student very difficult, and particularly repelled the allopath. Perhaps, he said, the allusion of one of our colleagues to the nature of some of the details contained in our *Materia Medica* was not so fair as might appear on the surface. The truth was our translators had employed our vernacular to the exclusion of professional terms which would have answered their purpose as well, and might have been less offensive to the general reader. He observed that writers of the old school when they described the result of any physiological researches which involved the operations of the genito-urinary system were just as minute, but that they employed language more strictly medical. He (Dr. Vernon Bell) considered it impossible to give an accurate description of the alvine functions and the metamorphosis of tissue effected by various organs without occasionally using expressions which none but the specially educated ought to see.

Dr. MADDEN, while thanking Dr. Hamilton for his paper, was not disposed to think so badly of the records of the urinary symptoms in the *Materia Medica* as some of his colleagues. Doubtless there was much repetition there as elsewhere; but from the method in which the provings were analysed in the *Materia Medica* such repetition was unavoidable. There was also much obscurity in consequence of the absence of chemical and microscopical analysis. Nevertheless, in spite of all these defects he found the urinary symptoms on the whole quite as useful as the records of the symptoms in any other region of the body. Under most remedies which influenced the renal secretion, there were sufficiently well marked urinary symptoms recorded to guide us to their selection in appropriate cases. Among other instances he referred especially to *Lycopodium* and *Pulsatilla* as bearing out this opinion.

ON POSOLOGY.

By FRANCIS BLACK, M.D.

(Read before the British Homœopathic Society.)

IN 1847 I read to you a paper "On Posology," and you may naturally ask me, as I have questioned myself, why is the subject now introduced? I beg to bring it again before

you because, in the present condition of homœopathy, it appears to me of the utmost importance for the consideration of this Society ; and I am the more induced to do so after reading the very able manner in which it has been presented to you by Dr. Yeldham.* I am not one of those who despair to elevate posology from its present state of routine and exaggeration ; on the contrary, it seems to be a subject worthy of co-operative work, and I feel sanguine that if this Society takes up the question with zeal, and prosecutes it scientifically, they will do much to rescue homœopathy from the obloquy which it invites from the contradictory and uncertain condition of its posology.

I have long felt the truth of the remark made by Dr. Drysdale in the course of the discussion on Dr. Yeldham's paper. "He had long held with many others that no truly homœopathic medicine can be said to have failed to cure until it had been tried in a dose up to the limit of physiological action. But it was now stated for the first time, that, as a general rule, the dose is to be found quite near the limit of physiological action ; if so, this would simplify homœopathic practice amazingly, and from his experience he would be inclined to assent to the correctness of the rule to a great extent. He would even go further, and say that if it had been generally followed homœopathy would be in a far better position than it is, for he had for long thought that extreme diminution of the dose had been the bane of our method."† Feeling acutely as I do, the unnecessary incubus which the dose question has laid on the progress of homœopathy, I am very desirous that you seriously examine this subject, and ascertain, as I think you must, that *the suitable dose lies near the limit of physiological action*.

For the correct examination of the dose it is necessary to define what is meant by homœopathy. It is a system of therapeutics where the pathogenetic effects of the remedy stand in the relation of similarity to the disease. Homœopathic remedies are specifics, and specifics have been well defined by Dr. Drysdale, as "medicines which cure with

* *Brit. Journ. of Hom.*, vol. xxviii, p. 745.

† *Ibid.*, p. 762.

absorption of their whole physiological into their therapeutic action.”* As a general statement, specific remedies require their therapeutic administration in doses smaller than are needful to excite physiological action, but there is no necessary connection between the dose and the therapeutic law. The one exists quite independently of the other ; it is the attempt to combine them which has dragged down homœopathy to the low level of a sect, and sadly retarded the progress of therapeutics. As a school, we appropriate all remedies which can be claimed as homœopathic, no matter in what dose they are given ; but while thus zealous to enrich ourselves, we are too often inconsistent and exclusive to our own brethren. How often is heard the observation, he is no homœopathist, or, it is not homœopathy, because material doses are given ?

I am very desirous that the distinction be clearly made between the dose, and the therapeutic rule, for lack of attention to this has caused many a bitter feud in our ranks ; it was a connection which Hahnemann made an essential part of his creed, and which led him to denounce many an able follower who might venture to give a dose of a remedy traceable by chemical or physical analysis.

The teaching of Hahnemann as regards the dose has been most injurious to the general adoption of his therapeutic law. There is a prevalent idea that Hahnemann arrived at his systematic division of the dose after years of much careful observation. History, as read in Hahnemann’s own writings, gives a very different result, and I cannot but consider the posological dogmas of Hahnemann as mythical more than real. By a myth, I mean wishes crystallised into assertions. Some may reprobate such language ; but while I yield to no man in esteem and reverence for Hahnemann, I cannot but regard some of his theoretical views on dose as signs of that peculiar weakness which men of great genius have sometimes shown in their hypothetical expositions.

I wish now to trace the history of the dose as given in Hahnemann’s writings. Commencing with the essay “On

* *Brit. Journ. of Hom.*, vol. xxvi, p. 78.

a New Principle for ascertaining the Curative Power of Drugs," published by him in *Hufeland's Journal* in 1796,* an essay testifying so remarkably to his learning, and his genius, the following passage occurs:—"We only require to know, on the one hand, the diseases of the human frame accurately in their essential characteristics and their accidental complications, and, on the other hand, the pure effects of drugs, that is, the essential characteristics of the specific artificial disease they usually excite, together with the accidental symptoms caused by difference of dose, form, &c.; and by choosing a remedy for a given natural disease that is capable of producing a very similar artificial disease we shall be able to cure the most obstinate diseases. The cautious physician, who will go gradually to work, gives this ordinary remedy only in such a dose as will scarcely perceptibly develop the expected artificial disease, and gradually increases the dose, so that he may be sure that the intended internal changes in the organism are produced with sufficient force, although with phenomena vastly inferior in intensity to the symptoms of the natural disease; thus a mild and certain cure will be effected" (p. 312).

In this same essay, where he so often gives so briefly, yet so admirably, the distinctive pathogenesis of various remedies, he also mentions the dose in which he gave them, *e. g.* an infusion of ten grains of *Ledum* once a day was a sufficient dose for a child six years old. Of *Arnica*, he says (p. 316), "It has been found excellent in the worst diarrhœas. It subdues them, because, without weakening the body, it is capable of causing frequent evacuations. In order to prove serviceable in diarrhœa without fœcal matter, it must be given in such small doses as not to produce perceptible purgation; or, in diarrhœa with acrid matters, in larger purgative doses, and thus the object will be attained."

Of *Veratrum album*, he writes (p. 350), "I gave her in the forenoon half a grain of *Veratrum* powder, and a similar dose in the afternoon at two o'clock. Deliria of another kind made their appearance, along with viscid mucus in the

* *Hahnemann's Lesser Writings*, translated by Dr. Dudgeon, p. 295.

mouth, but no fever returned, the patient slept, and in the morning passed white cloudy urine. She was well, quiet, and rational, except that the great weakness continued. The suffocating sensation in the throat was gone, the swelling of the face fell, as also that of the feet, but the following evening, without her having taken any medicine, there occurred a constrictive sensation in the chest. She therefore got another half grain of *Veratrum* the following afternoon; this was followed by scarcely perceptible delirium, tranquil sleep, in the morning copious discharge of urine, and a few small evacuations. For two more days she got half a grain of *Veratrum* in the afternoon. All her symptoms disappeared, the fever vanished, and the weakness yielded to a good regimen."

At this period, 1796, when he is discovering his therapeutic law, he gives ordinary material doses, and with full success. Two years later, in another article in *Hufeland's Journal* (1798), when writing on "The Specific Treatment of Fever,"* he says, "St. Ignatius bean produced effects that were truly surprising; I gave it in large doses every twelve hours; to children from nine months to three years, $\frac{1}{8}$ or $\frac{1}{4}$ of a grain; to those from four to six years, 1 to $1\frac{1}{2}$ grains; and to those from nine to twelve years, 2 to 8 grains. The intermittent fever under this treatment was removed in two or three days, without leaving any traces or any weakness."

Again, in the same fever, when *Opium* was indicated, he gave it to children from the dose of $\frac{1}{4}$ to $\frac{7}{16}$ of a grain in the morning, and repeating a weaker dose in the evening; he took himself $\frac{1}{4}$ a grain, and he reports the fever quickly disappearing, and the constipation ceasing.

Camphor, he writes (p. 390), "surpassed all his expectations; it was efficacious, and I may say specific in all the stages of the disease (remittent fever). At the commencement I was very cautious in its use, and did not give to adults above from fifteen to sixteen grains per diem, in almond milk, but I soon perceived that in order to produce a speedy recovery it was necessary to give, even to weak

* Translated by Dr. Dudgeon. p. 383.

subjects, thirty grains, and to more robust individuals forty grains, in the twenty-four hours. The favorable result was never long delayed."

Again, in the same year, and in *Hufeland's Journal*, he publishes another paper, "On Periodic Disease," in which ailment he gives as suitable and successful doses, "8 grains of *Ignatia*, or half a drachm of *Cinchona bark*."

In a paper published in 1801, "On the Cure and Prevention of Scarlet Fever,"* there is the first intimation of small doses, *e.g.*, he advises along with the external application of a strong tincture of *Opium* to the epigastrium, a minute dose of the tincture to be given internally. He diluted one drop of this tincture with 500 drops of alcohol, and again a drop of this with another 500 drops of alcohol, and of this he gave one to two drops. He gave this dose in water, or, strange to say, in beer. Larger doses, he says, cause raving, hiccough, &c., which, when they are not severe, disappear in a few hours, or may be more speedily relieved by smelling camphor. In the same disease he recommends *Ipecacuanha*, which, according to the age of the child, he gave either in powder, in doses of a tenth to half a grain, or from one to ten drops of a tincture. About the 1st dilution of *Belladonna*, he writes, "I gave to this girl of ten years a dose of this medicine, $\frac{1}{10000}$ part of a grain of the extract, which, according to my subsequent experience, is rather too large a dose. At least, if given for a preventive object, too large a dose for a child of this age, but probably exactly appropriate for the so far advanced symptoms of scarlet fever, but this I do not know for certain." Now, mark how wisely he goes on to add, "I cannot, therefore, advise an exact imitation of this case, for the scarlet fever is a much more serious evil than a few troublesome symptoms produced by a somewhat too large dose of *Belladonna*." This is a dose somewhere between our 2nd and 3rd dilution. Such doses created so much astonishment that Hufeland in the same year publicly called on Hahnemann to defend his remarkable

* Dr. Dudgeon's translation, *Lesser Writings*, p. 425.

posological statements.* Hahneman replies, "You ask me urgently what effect can $\frac{1}{1000000}$ part of a grain of *Belladonna* have? I hate the word *can*—let us ask nature what effect it has. A very hard dry pill of ex. of *Belladonna* produces on a robust healthy labourer usually no effect. But from this it by no means follows that a grain of this extract would be a proper or too weak a dose for this or a similar stout man if he was ill, or the grain was given in solution; certainly not." Here we have Hahnemann showing that the presence of a certain class of symptoms increases the susceptibility to the action of *Belladonna*, and this is further augmented by the carefully dissolved form of the medicine. "It is only by stirring, by brisk long-continued stirring, that a liquid medicine obtains the largest number of points of contact for the living fibres, thereby alone it becomes right powerful" (p. 440). He then goes on to describe how he divides this grain until the dose contains the millionth part of a grain. And now he makes the first of those extraordinary statements into which he was afterwards too often led. "A few teaspoonfuls of this mixture (containing a millionth part of a grain) bring him (*i. e.* the strong man) to the brink of the grave if he was previously ill, and if his disease was of a description as *Belladonna* is suitable for."

Is it remarkable that a statement so exaggerated turned away many a physician who might otherwise have willingly entertained the question, Have such minute doses of *Belladonna* an appreciative effect?

Hahnemann in this reply unfortunately gives no account of the steps or reasons which led him to arrive at his minute and systematic division of the dose, so that examination into this very important point is merely a matter of conjecture.

In his *Medicine of Experience*, published 1805, and in which he boldly announced the homœopathic law, he dwells on the wonderful sensitiveness of the diseased body to specific medicinal irritation, and he makes statements as to

* Dr. Dudgeon's translation, p. 443.

the power of drugs as compared with natural disease in a manner which no doubt much influenced his views of posology, *e. g.*—"Equally astonishing is the truth that there is no medicinal substance which, when employed in a curative manner, is weaker than the disease for which it is adapted."

Again, five years later, in the *Organon*, he expresses the same opinion. "Scarcely any dose of the homœopathically selected medicine can be so small as not to be stronger than the natural disease, and not capable of overcoming it."

In the *Medicine of Experience* he regards the action of medicines as dynamic, spiritual in its nature, like vitality itself, "and that it is of little, almost of no importance, how small the dose is."

In the *Organon* (cclxxx), he says, "The dose must therefore be reduced to the smallest point capable of causing an aggravation of the symptoms, however slight; such is the standard of measurement, and incontrovertible axiom of experience."

These views of the relative power of drugs and natural diseases, the former being absolute and stronger, the latter conditional, and weaker, were conceived, I imagine, to accord with Hahnemann's rationale of the homœopathic law. Given the coexistence of two similar diseases, the stronger overcomes the weaker.

I believe that these hypotheses, together with the valuable observation that some medicines administered in small quantities develop certain special effects which they do not manifest when given in larger, had far more effect in influencing Hahnemann's views of posology than any inconvenience arising from the administration of large doses.

Hahnemann, finding medicines systematically and infinitesimally divided by trituration and succussion produce a manifest curative effect, forgets the circumstance that he adopted division in order to avoid too much aggravation, and he now asserts that this process so potentized the remedy that he was "compelled by convincing experience to reduce the ten succussions formerly directed to be given to two." He published his *Theory of Dynamisation* in 1825. No theory is so likely to be incorrect as that which is formed at the

first flush of a discovery, and, if not at once rejected, time exalts the authority of him from whom it emanated. Such, alas ! has been the consequence of this theory ; unfortunately embraced by too many of Hahnemann's followers, it placed him on a pedestal which he valued higher than the homœopathic law, but, at the same time, it dealt a severe blow to the progress of the healing art, and launched our posology into a sea of myths and contradictions.

This theory is that the pharmaceutical process of division developes actual increments of power in all remedies, depriving the medicine of its material particles, and giving birth to the pure dynamic medicinal powers which, from their freedom from matter, are enabled at once to attack disease. I shall very briefly answer the principal arguments which Hahnemann gives in proof of his views.

1st. *Gold, Lead, &c.*—The results following the administration of the triturated preparations of these substances are very readily explained by the circumstance *corpora non agunt nisi soluta*. Oxidize gold and the same results follow, and so with lead.

2nd. *Natrum muriaticum*.—"The effects of *Natrum muriaticum*," writes Hahnemann, "must convince the dullest person that trituration gives birth to new forces, which nature hitherto concealed in her bosom ; and if any further evidence were wanted, it would be the conversion of common salt into a heroic and most potent remedy, one to be administered with the greatest caution. What an incredible fact, a new creation !"

Now, unfortunately for this statement, it is based on the assumption that salt in its ordinary state is inert, a statement totally at variance with fact, for *Muriate of Soda* given in solution, and on an empty stomach, may be regarded as an irritant salt ; various cases are on record of the violent effects following its ingestion. Other writers, quite independent of Hahnemann, have described its therapeutic virtues when administered in its ordinary state.

The argument that because in the proving of Hahnemann's *Natr. mur.* a pathogenesis is manifested which is not observed in those who daily take salt, and that this

action is due to the dynamised condition of the *Natr. mur.*, is explained by the well-known fact that when a substance is assimilated as food it ceases to act as a specific stimulus.*

8rd. Hahnemann quotes the effects of *Drosera* as proof that liquid medicines do not become weaker by dilution, but always more potent, and penetrating, and he warns against too much shaking in the following words :—“The infinite power of this process is so great that by this means a drop of *Drosera* 30, which at each dilution has received twenty shakes, endangers the life of a child suffering from whooping-cough, while if each dilution is only shaken twice a globule the size of a poppy-seed moistened with it, cures the disease easily.”† Such a statement requires no contradiction.

4th. If trituration, dilution, and succussion, develop new power, how is it that Hahnemann, in his introduction to certain medicines, such as *Ars.*, *Op.*, *Bell.*, *Ver. Olean.*, speaks of these processes as being necessary to diminish the energy of the drug.

5th. In your Society Dr. Hale‡ lately claimed the difference of specific effects of *Carbo veg.* and *Graphites* as instances of the power of dynamisation. It is not, he says, minute division, but some inherent quality which does not exist in the ordinary carbon, and which is only brought into action by the processes which homœopathy adopts.

Now, Dr. Hale is wrong in considering *Graphites* to be identical in chemical composition with *Carbo veg.* or *Carbo an.*, the only difference being that it is a purer form of carbon. Hahnemann states in his preface to *Graph.* that it contains a minute quantity of *Iron*; I would add, contains so much impurity in its native state that Professor Brodie discovered a process, which has been made the subject of a patent, to free *Graphites* from impurities in order to fit it for certain commercial purposes. It is so different from

* Vide paper by Dr. Madden, *B. J. H.*, vol. vi, 1848, p. 221; also Dr. Drysdale, vol. xxvii, p. 275.

† *Mat. Med. Pur.*, Part VI, Introd.

‡ *Annals of Brit. Hom. Soc.*, Dec., 1870, p. 46.

carbon, that when oxidised several times it is converted into a yellow crystalline substance, to which Professor Brodie has given the name of *Graphic acid*.

Dr. Hale must have forgotten Hahnemann's introduction to *Graphites*. Hahnemann tells us that this remedy was first employed by Dr. Weinhold, who was led to use it from seeing it employed by the workmen at a glass manufactory in Venice for eruptions on the skin. Dr. Weinhold, he says, gave it with great success, administering it internally in the dose of a drachm a day, or using it externally mixed with *saliva*, or as an ointment.

Up to 1827 the dilutions used by Hahnemann were various, but he now, in his *Chronic Diseases*, prescribes the 30th as the appropriate dilution for every remedy, and one globule not larger than a mustard-seed soaked in this dilution as the fixed dose. But later on he departs from this standard; he drops below it, and ranges higher in the scale to 60, 150, and even 300th dilution.

In 1832 Hahnemann wrote a short critique on some very wild ideas of a Count von Korsakoff, who imagined that after the 3rd or 5th dilution the subsequent dilutions obtained their medicinal properties by a kind of infection, so that one medicated globule, when shaken with a large quantity of unmedicated globules, communicated to them medicinal properties.*

I shall quote a portion of Hahnemann's critique as showing how extravagant he is in his views of dose, how slight the observation by which he illustrates them, and consequently how unsatisfactory are the conclusions he draws.

"The essay of this intelligent promoter of our art incontestably corroborates the following truths that some observations of my own had already hinted at, viz. 1.—That the development of the power of medicinal substances by the process peculiar to homœopathy may be assumed to be almost illimitable. 2. That the higher their dematerialisation is carried the more penetrating and rapid does their operation become. All this is in strict accordance with

* Dudgeon's translation, loc. cit., p. 856.

my own experience, though I have not carried them so far." Then he gives a case :—" I administered the 90 of *Sul.* to an aged lady who was subject to rare epileptic attacks, one every nine, twelve, or fourteen months, within an hour after she had an epileptic fit, and since then she has remained free from them."

Here Hahnemann has no doubts, no fears that the *propter hoc* may be the *post hoc*, but the case is given as a cure of epilepsy by one dose of *Sulphur* 90.

I shall now draw your attention to the only two cases published by Hahnemann after the appearance of his *Medicine of Experience*. They are cases given to illustrate the selection of a remedy, but interesting for the present discussion as bearing on the dose.

In the one case, a washerwoman suffering for three weeks from gastrodynia, he administers one drop of the mother-tincture of *Bryonia*, and with complete success.

In the other case, one of vertigo with nausea, *Puls.* was selected, and because the patient was weakly it was given in the 12th dilution. Next day the patient is well.

These cases were treated in 1815, but published in 1833, so that Hahnemann adds the following note to both the above doses :—

"According to our present knowledge and experience the same object would have been attained by taking one of the smallest globules of the 30th potency, and with equal certainty a single olfaction of a single globule of the same potency."

Let us examine carefully this note. Why, we may ask, recommend so high a dilution as the 30th, when those now mentioned answered to perfection? It is evidently not the result of any serious aggravation produced, nor the lack of cures following such doses, that induces him to write this note, but the theoretical feeling as to dynamisation. He is expressing in this note what he states more fully in the *Chronic Diseases*, that thousands of warning experiments had at length convinced him that these very minute doses were the most appropriate, and at the same time he denies the utility of larger doses, and asserts he never had obtained

the true curative effect of the medicine until he arrived at this dilution of the dose (30th.)

Mark, "never had obtained," though his own recorded experience in his first essays and the two cases now given prove the contrary, and in the very volume where he hazards this opinion he states that he found *Carbo veg.* 8 and *Sep.* 3 quite efficacious in recent itch, given in half-grain doses.*

I have now attempted to give a brief statement of Hahnemann's views of dose, because they are the keynote to the practice of many of our school, and they have, and still do exercise, an injurious effect on the adoption of our therapeutic rule. I advise all who desire seriously to study the question of dose to read, in Hahnemann's own words, the numerous dogmatic and contradictory statements he makes. If they have not leisure to consult the original writings they will find a full résumé of them in Dr. Dudgeon's admirable *Lectures on Homœopathy*.

I do not know of any carefully recorded series of cures, for one or a few cases prove nothing, which show the superior efficacy of high dilutions. My own experience of thirty years has pointed in the opposite direction. I began the practice of homœopathy by using the higher dilutions, encouraged by the personal exhortation of Hahnemann, but the exigencies of practice soon led me to reduce the scale. I now think the suitable therapeutic dose is so near that dose which can excite physiological action in the healthy body that a range from crude substances to the 3rd centesimal dilution is amply sufficient to meet all the requirements of practice. I do not make this statement without having repeatedly, and for years looked at practice with a view to aid in determining this question. Minute doses have so proved the bane to the progress of homœopathy that I have for a long time striven to make the dose as low as possible, not only with a view to cure, but also to disarm prejudice, and gain medical converts to our ranks.

A dose is sufficient when it stimulates the diseased parts

* *Chronic Diseases*, vol. i, p. 130.

to healthy action without exciting aggravation or the occurrence of chemical or physiological action.

These conditions of suitability are dependent on the specific susceptibility of the diseased parts, on the susceptibility of the individual sick person, and on the specific peculiarities of the medicine.

The first two conditions are more within the range of experiment than the last. True aggravation must be distinguished from the disturbance occasionally set up in some temperaments, but due, not to specific action, but to the peculiar sensitiveness or idiosyncrasy of the patient, who suffers the same effects from a variety of distinct causes.

The true aggravation is often very fleeting, but if persistent is met by diminution of the dose; the medicinal perturbation is oftentimes obviated by an increase of the dose, sometimes by less frequent repetition of it. This liability to medicinal perturbation is in some nervous habits a troublesome barrier to cure, especially when the patient has been placed in an attitude of expectation, anticipating all sorts of effects from these wonderful doses. It is curious to find how much oftener such kinds of aggravation occur in the practice of those who believe in dynamisation than in those who take a purely objective view of the dose. The occurrence of physiological action, independent of the disease, does not necessarily interfere with the cure, and is removed by diminishing the dose.

The susceptibility of certain persons, and the susceptibility existing in some diseases more than others, are matters which come readily into the domain of medical experience. The great difficulty is the administration of such a dose as will manifest the peculiar specific action of the drug. By peculiar I mean that class of action which has been called by Dr. Madden *ideo-dynamic*, and by Dr. Drysdale *contingent*, because they are not readily produced at will, but are dependent on the presence of a special susceptibility. In this region of contingent effects lies the real difficulty of the dose, and one which depends for its thorough elucidation on a very correct knowledge of the pathogenesis of the drug.

I believe all the requirements for suitability are met

within a range extending from material doses of the crude substances up to the 3rd centesimal dilution, and never need to exceed that point. For the last two years I have carefully pressed the experiment further, on the one hand, with such remedies as *Arsen.*, *Merc.*, *Kali bich.*, *Iod.*, and *Bell.*, a class acting strongly and very readily on the human system; and, on the other hand, with such medicines as *Sul.*, *Sep.*, *Lyc.*, *Hep.*, *Sil.*, a class manifesting much less readily on healthy subjects their physiological action. The results of my experiments have been such that I now very rarely prescribe these remedies higher than the 3rd decimal dilution. But at present my thesis is that the dilutions up to the third centesimal meet all the requirements of practice; it is on this point I am desirous to know your opinion.

I do not deny the action of the higher dilutions, but I claim a more general efficacy for the lower. For the sake of argument I admit that in very exceptional cases the higher may answer better, yet this rare advantage is much more than counterbalanced by the prejudice which such practice creates. A few patients cured is a poor counterweight to many medical converts lost. I believe that our failures to cure are more frequently due to smallness than to excess of dose.

We know the value of minute doses, but let us also take a lesson from our brethren of the ordinary school, and mark how in their hands such specifics as *Arsen.*, *Bell.*, *Bis.*, *Quin.*, *Kal.*, *Hydr.*, *Merc.*, &c., act. Let us remember that Hahnemann was guided to such remedies as *Cham.*, *Verbascum*, *Samb.*, from the beneficial effects of large doses of the infusion used in domestic practice. Remember that the value of Hahnemann's observations on the dose prior to his *Belladonna* paper cannot be diminished by pleading his improved experience in after years. The facts of the action of the drug and its dose are perfectly valid, and even more valuable than many later statements, for the cases, not mere opinions, are given. In drawing attention to the lesson to be gained in the direction of more material doses, I wish most carefully to guard against the error of supposing that the therapeutic

action of a medicine increases in the ratio of its quantity, or that any increase in quantity will supply a lack of similarity; the large dose is not a substitute for the study of the *materia medica*.

In keeping within the limits of the 3rd cent. I benefit my patient without distressing him, I find the specific action of the medicine manifested, and I do not evoke that bugbear aggravation more than when I gave higher dilutions.

I relieve the chemist from an enormous amount of labour and responsibility, a point of no mean consequence, for chemists are but men, and, as such, fallible. In the ordinary ratio of probabilities the chances of error in diluting up to the 30th are greater in favour of mistake than of accuracy.

The limitation of choice within the 3rd lessens the labour of the physician. Our school have a very simple rule as a guide to practice, but in return it has laid most grievous burdens on its followers; it points to a *materia medica* of immense extent as the mine in which the ore is to be found, brought to the surface, and cleared from a considerable percentage of dross; but the wearied worker's labour is not yet done, for he is told this ore which he has at last found may be used in doses varying from the crude substance up through all the stages of 10th, 100th, 1000th, 1,000,000th, on to points which are far more inconceivable than the distance from the earth to the sun given in inches. All the guide you assign him in this illimitable ocean is either his own experience or the traditionary practices springing from the contradictory and dogmatic tenets of Hahnemann.

This labour can be very much diminished by limiting the range of dose; then time and energy are left to determine, not merely the dilution, but the amount of it—above all, its repetition, for this is nearly as essential as the actual quantity given. I know that medical experience is too often a shifting sandbank, the apparent fact of to-day proving the delusion of to-morrow, but careful repetition of the experiment lessens the chances of mistake. I am, therefore,

very anxious for your co-operation in investigating the question of dose within the limits I have given.

It is just the subject which requires a number of honest workers in order to correct the fallacy of medical experience.

This experience is often fallacious because it is too often limited and imperfect. Limited to a few observations gleaned in a short time and in a narrow area. Imperfect, because we are regarded too much as contractors to cure, not as observers; hence human weakness bends to what is most expected of us. Imperfect through our failures in diagnosis; imperfect, above all, through our ignorance of the natural course of disease, so that we attribute results to our interference rather than to the essential course of things.

Combination alone will not counterbalance these fallacies, and solve the problem of the dose unless we cast aside all personal feeling, all prejudice, and strive to work in a strictly impartial and scientific spirit.

Discussion on Dr. Black's paper.

Dr. KIDD said, the admirable manner with which Dr. Black has refuted Hahnemann's ideas of the dose from his own writings leaves nothing to be desired. The rule as to the dose which Dr. Black has adopted for a year past is nearly the echo of his (Dr. Kidd's) practice for the last ten years. From his own personal experience he learnt that all the effects of a medicine can be had without going beyond the 3rd decimal dilution or trituration. For the last ten years such has been the rule of his practice. It is truly wonderful to observe how the great leading principle of *similia similibus curantur* evolves the truth of the dose. Every remedial agent, acting in harmony with that great law of cure, shows its efficacy with moderate doses, but that efficacy is not perfectly produced by infinitesimal doses. The physician who has the deepest faith in the law of cure will not rest satisfied with less than the perfect benefit of each medicine. He should be as ready to trust to small doses as to prescribe large, if the latter did not cure. The small dose is a great boon to humanity, but the idea of infinitesimal doses has proved a deadly hindrance to the science of therapeutics. Alas! that the cow which gave such good milk should kick the pail over at the last. Each organ seems to require

its own special dose; thus the brain and spine will respond to much smaller doses of the homœopathic specific than the stomach, liver, or kidney. In the special neuralgia of colocynth its action is perfect in the 3rd decimal dilution, but in colic or spasms of the colon the mother-tincture is infinitely more useful and certain—more to be depended on. Each sick person requires a different dose according to the temperament or susceptibility; thus two cases of dropsy under his treatment at the same time were both cured by *Turpentine*. One, a lady of phlegmatic temperament, showed no signs of amendment till the dose was gradually increased from three drops to sixty, three times a day. Ten or twelve days' use of the latter dose perfectly and permanently cured her and carried off all dropsy without purgings. The other case, a weakly old man of an irritable nervous temperament, was freely purged by four drops three times a day of the *Terebinth* θ ; the dose was reduced to one drop, which still slightly purged; but six weeks' use of one drop three times a day perfectly cured him. It was a truer satisfaction to cure the latter with one drop rather than the first with sixty drops; yet why abandon the right remedy till the full and true curative effect was produced?

Mr. POPE said that he had thought that the question of dosage had been so much written on and discussed that it had become impossible to render a paper on the subject of any interest. That this was an error had been abundantly proved that evening. The paper Dr. Black had read was one full of interest, of great practical importance, calculated to render eminent service to homœopathy, and to be extremely useful to homœopathic practitioners. He had been much gratified by the very clear manner in which, in his opening remarks, Dr. Black had distinguished between homœopathy and small doses. That homœopathy is entirely independent of small doses it was very essential should be distinctly asserted. It could not be too frequently or too forcibly dwelt upon. Homœopathy was a method of selecting medicines: the quantity of medicine to be given was another matter altogether. The doctrine of dynamisation, too, was one that Dr. Black had, he thought, very correctly criticised. If it were true that, by succussion and trituration, drugs received an increment of power they became altered, they were no longer the same drugs as those that had been proved. Again, dynamisation could not be demonstrated. There had been no proof given of any alteration being effected by succussion and trituration, and so far as he could see, none was forthcoming. While fully agreeing that the practical rule which Dr. Black had laid down, that the crude substance or dilutions below the 3rd centesimal should be the preparations employed in practice, Mr. Pope thought that occasionally cases were met with where advantages were obtained from higher dilutions that could not be secured by those below the 3rd centesimal. These cases were doubtless few and exceptional, but they existed; and we ought not to rest satisfied until we had some principle by which

we could recognise them. Again, we ought not to forget that, though in the earliest period of homœopathy crude substances were exclusively prescribed, there was a middle period in which all the success that was obtained was derived from the use of infinitesimals. Without any desire to return to the infinitesimals of this period, it would, he thought, be unjust to the memory of those men, through whose success at the bedside many of us had been induced to study homœopathy, to lose sight of the fact that by them the 6th, 12th, and 30th dilutions were almost uniformly adhered to. While we have abundant reason for preferring the dose advocated by Dr. Black, it was impossible to avoid the conclusion that the evidence which existed of the curative power of infinitesimals had opened up a field of enquiry, which might, in the future, be productive of much valuable fruit. It must also be remembered in discussing this question that some medicines, and notably *Lachesis*, could only be obtained in the 5th or 6th dilution. The efficacy of *Lachesis* had been much debated; but his own experience assured him that, when given homœopathically, *Lachesis* was a valuable remedy, one he should be sorry to be prevented from using. The practical rule of Dr. Black had many advantages; it was simple, it enabled us to be sure that we were giving medicine, and rendered us less dependent on the accuracy and integrity of our chemists than we were when using high dilutions. This was an advantage of no small importance. But after all, this rule of limiting the preparations to the 3rd centesimal and those below it was but an empirical rule. Further investigation was required, a more thorough sifting of a yet larger and more accurately studied experience was needed, to give scientific exactitude to the choice of a correct dose. It was probable that, as Dr. Kidd had remarked, each disease had its proper dose; and further, it was also probable that each medicine had a dose better adapted than another to meet each form of disease to which it might be homœopathic. There was in these matters still much room for enquiry. Mr. Pope concluded by expressing his sense of the obligation he felt all were under to Dr. Black for having brought before them so thoroughly practical and useful a contribution to the important subject of the dose.

Dr. CARFRAE considered Dr. Black's paper a most valuable and instructive one, inasmuch as the question with which it deals is perhaps the most important point for solution in the present time. It is an undoubted fact that the majority of homœopaths now use very different doses from those habitually used by Hahnemann and his immediate disciples. It is also an undoubted fact that the dose used by practitioners of the old school differs very materially from that used in the last generation. As an illustration of this he may mention that only a few months since the *Practitioner*, in an interesting article "On Ipecacuanha," suggested its use in drop-doses of the *Vinum Ipecacuanhæ* of the British Pharmacopœia in some forms of sickness. The only

conclusion we can draw from these facts is that, although we have since the days of Hahnemann discovered many valuable remedies, and have also a guide as to how to use them, we do not yet know in what quantity they ought to be administered. Dr. Carfrae has devoted a good deal of thought to the subject, and has not hitherto been able to arrive at any satisfactory conclusion. He is now convinced, however, that the question of the dose is to a great extent settled. He entirely agrees with Dr. Black in giving the preference to the low dilutions, and thinks that this Society is deeply indebted to him for his able and lucid paper.

Dr. RANSFORD thought that, so far as the question of dose can be settled in an essay, Dr. Black had decided it in the present admirable one; but he confessed that he was not quite decided upon the question of dynamisation, so called, because he was convinced by experience that certain medicines act more beneficially in higher dilutions than in the lower ones. For example, take *Baryta carbonica* in inflammatory affections of the tonsils and pharynx, with the 12th dilution of this medicine he had never yet been disappointed; but both he and some of his colleagues who had tried it at his recommendation had failed with dilutions lower than the 6th. In the ordinary use of medicines, especially of the vegetable preparations, he ranged from the mother-tincture to the 3rd dilution, but reserved to himself liberty to do that which he believed to be the best for his patient. Did time permit he could enlarge upon this subject. He would take the liberty of suggesting that in all clinical cases recorded, the dilution and dose of the prescribed medicine might be given likewise, so that by extended clinical experience we should be likely to arrive at the truth. He had no reason to doubt the credibility or accuracy of many of his colleagues who use high in preference to low dilutions. He was disposed to agree with Dr. Carfrae in believing that many cases of so-called cures under homœopathic treatment were due to the abandonment of pernicious allopathic doctoring. He would give one instance. When residing in Edinburgh he attended a lady who from the age of fourteen had been the subject of frequent attacks of epilepsy. She had had all sorts of advice and taken every kind of drug without benefit. At length she left off drugs, and had no fit for fifteen years. She is now upwards of eighty years of age. Had this lady taken our medicines, which she never did, the credit of cure would no doubt have been given to homœopathy.

Dr. TUTHILL MASSY would not have risen to make any observation, for the speakers who had preceded him had so fully expressed the opinions which he had entertained, but for a few lines handed to him by Dr. Every Carmichael who was present among them as a visitor; they referred to *Natrum muriaticum* as introduced by Dr. Black:—"In conducting delicate chemical analyses *Chloride of Sodium* is one of the many obstacles to accurate results. It is found so largely distributed in nature that

it is one of the commonest medicinal substances we take into our systems; already dynamised by nature, existing in a state of minute division, thoroughly separated by natural agitation, if it be the potent medicine represented it is a wonder that any of the human family now exist on the surface of the earth. *Chloride of Sodium* will be found even in the most carefully distilled water; in fact it is one of those substances which may fairly be called 'omnipresent,' and so persistent that I doubt much if it is ever entirely got rid of. Like the 'sporules' of Professor Tyndal, found to contaminate his ray of light, it embarrasses the chemist, and should convince homœopathists of the fallacy of the theory of high dilution, because the dilution is already made—in the air we breathe, in the water we drink, and in the food we eat." Although, as a practical rule, Dr. Massy has kept to tinctures and triturations of the third power, yet he has frequently prescribed the high potencies and the mother-tinctures in particular cases with marked beneficial results. The rules for practice so ably laid down by Dr. Black were excellent, but he did not see why high potencies should be cast aside altogether. One inconvenience from them was the reported cases of aggravations, and another the greater care and accuracy required in the prescribing of them. He had himself seen the symptoms of salivation in a susceptible young girl from a few globules of *Mercurius solubilis* (the 3rd or 6th), which in the old school would be considered incredible. Dr. Kidd's rule, in proportioning the remedy to the diseased organ or amount of poison imbibed in any particular case, is wise. A case in point occurred about this time last year, where in "Bright's disease" with a syphilitic taint *Merc. cor.* 2nd was given. The results were marked, a hard node began gradually to diminish, the albuminous urine improved, and the muscular strength increased, so much so that, notwithstanding the hopeless opinion of Dr. Gull, Sir Wm. Jenner, and others, the patient is now one of the healthiest-looking men in Brighton.

Mr. J. H. SMITH observed that ever since he became an homœopathist he had acted on the principle so clearly expounded and so ably enforced by Dr. Black, of not diminishing the dose more than he believed to be necessary to avoid the physiological action of the medicine, and yet to secure the curative effect. This object in practice he had generally been able to secure by doses varying from the 1st to the 3rd decimal dilution. There were certainly, however, exceptions to any general rule of this kind, and he should like to ask Dr. Black whether he had *advisedly* included *Ohamomilla* in the list of remedies which Hahnemann had originally used successfully in mother-tincture? Dr. Hughes, who is certainly not a high-dilutionist, indorses the statement of Dr. Holcombe, that this medicine "is of no more value in disease in the low dilutions than catnip or mint tea," and that it begins to manifest its power at the 6th dilution, and may often be given

advantageously as high as the 18th. This he gives as the general experience of homœopathic practitioners.

Dr. WYLD much preferred the low dilutions, viz., the *first* and *third* decimal; chiefly because the quantity was appreciable, and could not excite the ridicule, contempt, or disbelief of the opponents of infinitesimal medicine. He held that we were bound to present our system in the light least liable to excite opposition. The great error of old medicine was over-drugging; and the mistake of Hahnemann, and his early followers especially, was an excess in the opposite direction. As a body of dissenters we laboured under many disadvantages, both in a social and scientific point of view, in being excluded from intercourse and interchange of ideas and knowledge with the highest minds in the profession, and Dr. Wyld, for one, would be only too happy to see all unnecessary barriers cleared away. At the same time he could not resist the evidence of his senses in favour of the fact that infinitesimals did produce extraordinary cures. His own son had suffered from extremely large tonsils for two years; so much so, that at the age of four it was in contemplation to place him at the seaside for a year. This condition entirely disappeared in two weeks under *Calcareæ* 30, given twice a day, and during the last eight years the tonsils have continued in a perfectly healthy state. So also Dr. Wyld had frequently cured housemaid's knee, or effusions into the bursa patellæ by *Silicea* 12. Facts of this kind could not be ignored, and very possibly they might one day afford a key to the philosopher in his attempt to solve the mystery of matter.

Dr. EDWARD BLAKE considered that Dr. Black's paper was one that was much more easily commended than criticised. He thought that the matter might be reduced to two main points—*dynamization and dose*. Thanks to the strides with which physical science had advanced since the days of the great master, the former question had received its solution. From the working out of the thermo-dynamic theory, we know that we may convert a given amount of energy stored up in the muscles of the arm into an equal amount of divulsive power, which drives apart the molecules of the substance being triturated; but every school-boy is now taught that *no fresh force of any kind is formed*, but that by again condensing the substance we can (with a slight allowance for correlative electricity and frictional heat) collect exactly that quantity of *energy* which was expended in producing the trituration. Attenuations may be more active because more soluble, but they receive by succussion no augmentation of power in the sense that Hahnemann meant, for it is now everywhere recognised that it is as impossible to generate as it is to annihilate force. Organic substances, and the bulk of our drugs are organic, tend by excessive sub-division to be reduced to their component elements, thus losing more and more their specific distinctive characters. Although there has been a vast amount of assertion

as to the superiority of the higher dilutions, he could only recall two sets of definite experiments, those of Dr. Neville Wood *On Aconite*, and of Jousset *On Drosera*, 'Bul. de la S. M. H. de F.,' Jan. 1868. If these be carefully weighed they go no farther than to show that the higher dilutions are *as good as the lower*; but, if they are no better, why adopt forms of medication so peculiarly open to disturbing influences from their cradle in the Pharmacy to their grave in the primæ viæ of the unhappy patient?

Dr. C. WATSON desired to call attention to Dr. Malan's case, in the *Homœopathic Review* for this month (May), when Dr. Wilson was called into consultation. A case of encephalitis and spinitis, in which *Hyoscyamus* 200th dil., and *Cocculus* 200th dil. were given. Globules were prescribed, and though certainly the convalescence seemed slow, and perhaps more due to expectancy than medicine, yet it was a very satisfactory recovery, and Dr. Watson thought it behoved us to investigate such cases thoroughly, and not to dismiss as *idle* the testimony of such well-known homœopathists. Ever since he commenced practice he had observed that in skin diseases, such as psoriasis and eczema, large doses were required to effect a permanent cure, whereas, in nervous diseases, which mostly occurred in delicate females, higher attenuations were forced upon him. Dr. Watson's conviction was that to the nervous system and its varying susceptibility in different individuals, and in different diseases, we must look for the rule on the dose. A French gentleman, a visitor to the Society, had just mentioned to him that a relative of his, a lady coming from the Mauritius, who had contracted intermittent fever there, sought relief at Paris from allopathic treatment, with *Quinine*, *Arsenic*, &c., in vain, and it was not until Dr. Jousset, a homœopath, after trying various dilutions of *Nux vomica* without effect, finally resorted to the 200th, that he accomplished a cure.

Dr. BAYES, while fully sensible to the great value of the observations which had been read that evening by Dr. Black, and of the great advantage to be derived from the experience of such men as Dr. Black, Dr. Yeldham, and Dr. Kidd, whose remarks were the fruition of some twenty to thirty years of very active and extensive practice, yet was bound to say that he considered it very difficult to lay down a fixed rule as to the best dose or dilution in all cases. The late Dr. Marston, in a series of cases reported in the *Homœopathic Review*, had laid it down as a rule, deduced from his experience, that in derangements and diseases of the stomach he found it needful to give low dilutions and triturations, as also in skin diseases. In both these conditions of disease, his (Dr. Bayes') experience coincided with this view. But in diseases chiefly affecting nervous centres, and in very sensitive individuals, the opposite rule dominates, and the higher dilutions appear to act better. Excepting up to a certain point, with insoluble substances, he (Dr. Bayes) does not believe in dynamization. The more soluble a substance becomes, the greater

its physiological power. It is from its greater solubility that *Mercurius corrosivus* is, in the lower dilutions, more potent than the same dilutions of *Mercurius vivus*. But though trituration and dilution do not increase the physiological power of medicine, yet he was not prepared to say that the curative power did not become greater, in some cases, by dilution. As regards the proposition laid down by Dr. Black in his paper, that the proper dose is that which is just within the physiological dose, he (Dr. Bayes) conceives it to be very difficult to assign a limit to the physiological dose; some patients are sensitive to an extraordinary degree, while others are little affected by large doses. He had seen *Arnica*, 18th dilution, induce erysipelas in a patient, and he related other cases showing extreme sensitiveness to the action of medicinal drugs. Some medicines undoubtedly act far better in the higher dilutions than in the lower. Of this he instanced *Chamomilla*, which appears to exert a curative power in the 12th, while the 3rd is comparatively inert. He also instanced the action of *Lachesis* 6, in the treatment of flushing, and occurring in women at the change of life, as an absolute proof of the great curative power residing in the higher dilutions.

Dr. LEADAM.—Dr. Black in his observations has suggested discrepancies in Hahnemann's data, which are only to be attributed to the progress of his experiments, and are to be explained by his own astonishment at the results. First, he was surprised at the so much better effects of *Bryonia* 6 and *Pulsatilla* 6 over the old-fashioned crude doses in quantities, mixed up too with a polypharmacy. Then, as he began with dilutions, he was again surprised at the increased power developed at another stage, and thought he had attained a finality, but as he went on there seemed no end to his development of power, and his enthusiasm became extreme. Were we to be pinned down to the dilutions No. 1 and 3 in all cases, when, according to one speaker, *Colocynth* was barely useful in the 1st, showing evident signs of exacerbation, while in cases of neuralgia nothing short of No. 3 had any effect. Where was the benefit, in this case of Dr. Kidd's, in travelling tentatively from 6 till arriving at Nos. 2 and 3, when a more expanded dilutionist would at once have given No. 3 for internal affections with success, and 12 for the neuralgia, or 30? And according to another, *Chamomilla* 12 was declared much more effectual than 3; and to another, *Baryta carb.* 12 was far better than 3rd trituration; and another, that *Chamomilla* in the lowest dilutions has no virtue; and another, that *Silicea* 12 was most beneficial in synovitis, and *Calcar. carb.* 30 in some other affections. Dr. Bayes says that erysipelas has been produced palpably by *Arnica* 18, and *Lachesis* 6, which is the lowest dilution to be got at the chemists, is most effectual in climacteric disease, as most of us can testify. Why, then, should we be tied down to the 1st and 3rd dilutions? Dr. Leadam begged to thank Dr. Black for the well laid-out paper, in which, however, he could not agree.

Dr. YELDHAM said he had not long since fully expressed his opinions on this subject, in a paper which he read before the Society "On the Rule of Dose," and to which Dr. Black had courteously alluded as having suggested to him the present paper. Several of the previous speakers alluded to the *difficulties* of determining the proper dose in any given case. But these difficulties were surely not insurmountable. The physician was not an automaton. He was endowed with intelligence, trained to the duty of working out the problem of the dose; and any man with ordinary powers of observation, keeping steadily in view the rule of *non-aggravation*, and bearing in mind the different susceptibilities of different constitutions and ages, the different powers of different medicines, and the different requirements of different diseases, would seldom fail to arrive at safe conclusions as to the proper dose. As the question at present stood they were in the slough of despond, one man giving bulky doses of crude drugs, another the 14,000th, or higher dilutions still. This was a state of things which could not be satisfactory to any thoughtful mind, and an escape from it was worth any sacrifice. Let each one of them, then, forego his predilections and prejudices, and lend a helping hand in putting to the proof this rule of giving medicines near to the physiological dose—a rule deducible from the fundamental law of homœopathy, and which answered all their requirements. To such an undertaking the individual sacrifice would be small, the aggregate gain incalculable.

Dr. MADDEN expressed great doubts whether the vexed question of the dose was yet ripe for settlement; he hoped, however, that a good many of the members would agree to some plan of working, by which they might accumulate materials by means of which the question might be decided. For his own part he would greatly prefer using only the low dilutions; he disliked prescribing medicines in such a form that one was absolutely dependent on the chemist or his assistant for the assurance that one was giving any medicine at all. At the same time he had seen occasionally such gratifying results from high dilutions that he was unwilling to relinquish them altogether. Experience had certainly taught him that certain remedies produce better results in the 12th and 30th than in the 3rd and 3rd decimal, but it is quite possible that descending the scale would have been as useful as ascending it, and that ϕ would have acted as well as the 12th and 30th. Still this has yet to be proved. He saw many reasons why the larger and more material doses should be preferred to the infinitesimal, provided it were once demonstrated that they act as well; but until that result is attained he felt that, in justice to his patients, all the length of the scale must be utilised.

Dr. HALE was quite convinced that it is impossible to lay down any rule for the dose; all dilutions and doses consistent with an adherence to the guiding law of homœopathy were admissible. Dr. Hale protested against any attempt to be tied down to any dilu-

tion, either high or low ; he himself prescribed medicines from 1st to the 30th, and sometimes in higher dilutions still, and the speakers who had preceded him had given abundant evidence of the curative power of the higher dilutions. Into the question of dynamisation at so late an hour in the evening it was impossible to enter, but this much Dr. Hale might venture to say, that without accepting Hahnemann's theory of dynamisation, the fact of dynamic force being developed by the processes of trituration and succussion would still remain a fact, Dr. Black's attempted refutation notwithstanding. In confirmation of such developed forces there are several analogous facts in physics, and did time permit these might be adduced. Dr. Black, in condemning the theory of dynamisation and the employment of high dilutions, had employed the word "pernicious" to such theory and practice ; Dr. Hale objected to the use of such a strong expression in relation to either the theory of dynamisation or to the practice of the high-dilutionists, and contended that, if the word were applicable at all, it would be more so to the practice of giving massive doses as a rule instead of on exceptionally rare necessity.

Dr. DRURY, V. P. (in the chair), had heard Dr. Black read a paper on homœopathy nearly thirty years ago, and though that paper had not convinced him of the truth of Hahnemann's doctrine, still it had so impressed him with Dr. Black's honesty of purpose, and had shown him that there was no cause for treating homœopaths with ridicule or disrespect. For this he was deeply indebted to his friend Dr. Black, as well as to the late Dr. Fearon, who had also impressed him favorably as to some of the facts connected with homœopathy, so that, when the time came that he was induced to test the truth of homœopathy by investigation carried on at the bedside, he had already surmounted some of those difficulties at which some of his brethren stumbled. While thanking Dr. Black for his present paper, which was full of interest, he felt that the question was one that could alone be settled by actual experiment ; he had often expressed his wish that men holding opposite views should treat patients in the same hospital, for neither expression of opinion nor success in private practice proved anything. A man by force of assertion might defend the greatest truth or the grossest error. He had been early led to the use of the 30th dilution from seeing it used with the greatest success by a gentleman from whom he chiefly learned his homœopathy. For several years he used it largely in private practice, and still continued to do so almost exclusively in the hospital in treating diseases of children. Dr. Carfrae expressed an opinion as to what he had seen in the hospital, founded, he presumed, on what he had seen of the physicians' and surgeons' practice, but from inquiries Dr. Drury had made, others had not come to the same conclusion. However, as the opportunity was not afforded to test the question in the only satisfactory way in which it could be done, he had determined in his private practice

to try and arrive at some conclusion; he had accordingly tried the 12th dilution on a large scale, and had satisfied himself that it was a safe and useful strength for practice. He had, then, on a more limited scale tested No. 6 or 12^x, and with this strength he had obtained some most gratifying results. He was now trying in some cases that seemed to him specially adapted for the purpose, 1^x; but his experience was, as yet, too limited to do more than say that in some cases he had seen much benefit follow. It was a matter of great importance that as little temptation as possible should be put in the way of the chemist, nor was it desirable to unnecessarily tax the faith of the patient, but having had a very large experience with the high dilutions, neither of these considerations should weigh with him, unless convinced that in a low dilution he had a medicine equally serviceable to a high. Dr. Kidd had expressed an opinion very unfavorable to the practice of the late Dr. Curie, but he proved too much; for if patients were left to nature alone, the returns ought to have been much more favorable; Dr. Kidd's prejudices may have led him to conclusions differing from those of others, but this only showed that such a question should not be left in its present state. A number of gentlemen associating together, and as far as possible acting without prejudice, might come to a conclusion that would clear away present doubts.

ANNUAL MEETING OF LONDON HOMŒOPATHIC HOSPITAL.

THE twenty-first annual meeting of the governors of this hospital was held in the board-room of the institution, Great Ormond Street, Bloomsbury, on Thursday, April 27th. The Right Hon. Lord EBURY, Chairman of the Board of Management, occupied the chair; and among those present were John Boodle, Esq. (Deputy Chairman), Charles Trueman, Esq., J. B. Crampert, Esq., A. J. Ellis, Esq., F.R.S. The Rev. Mr. Flather having opened the meeting with prayer,

The CLERK (Mr. John R. Warren) read the minutes of the last meeting, which were confirmed and signed by the Chairman.

The OFFICIAL MANAGER (Mr. Charles Trueman) then read the following report of the Committee of Management:

"The Board and Management can but congratulate the governors and subscribers on the general well-being of the hospital; and it will be noted with satisfaction that the record under each separate head of the report is favorable.

"The total number of patients treated since the opening of the hospital to 31st December, 1870, was 89,568, of which 7,836 were

patients in the past year, being 7,340 out- and 496 in-patients, an increase of 468 out-patients and 1 in-patient over the preceding year. The increase in the number of out-patients, as well as that of the in-patients, must be deemed satisfactory.

"The balance at the banker's at the beginning of 1870 was £625 3s. 11d., and with petty cash balance, &c., formed a sum of £639 9s. 2d., whilst the total income from all sources amounted to £2,771 8s., against £3,725 5s. 6d. in 1869; but in the last-named year the sum of £1,285 13s. 1d. was received from the nett proceeds of the dinner at the Freemasons' Tavern.

"If, therefore, from this total income of 1869 the dinner proceeds are deducted, it would show the income from ordinary sources to have been £2,439 12s. 5d.; and as that of 1870 was £2,771 8s., there would appear an actual increase in 1870 of £331 15s. 7d. The increase was in truth made up of increased dividends on stock, £31 15s. 2d.; increased subscriptions, £31 16s.; increased registration fees, £28 16s.; and increased donations, £243 2s. 4d. The donations of the past year reach the sum of £1,145 16s. 8d., and of this £933 10s. has been invested in Consols, &c., and carried to the reserve fund, which now amounts, exclusive of house and furniture, to £7,613 3s. 11d., at a cost of £7,005 11s. 11d.

"Of the above-named donations, £82 was paid us on account of the legacy of the late Lord Henry Seymour to the hospitals of London and Paris; £20 a legacy from the late Mrs. Phoebe Lee, per Dr. Yeldham, and £50 a legacy from the late Mrs. Elizabeth Cope; whilst the gifts were £100 from R. Greaves, Esq., per Dr. Collins; £31 10s. from J. R. Wegg Prosser, Esq.; £50 from our always generous Treasurer, Mr. Rosher; and two sums, one of £100, one of £500, from friends of Dr. Quin, again mindful of the interests of the hospital. To these donors letters of grateful acknowledgment have been sent.

"The expenditure of the year has been £2,287 0s. 1d., against £2,191 1s. 10d. in 1869, an excess of £95 18s. 8d. This excess has been caused principally by the increased price of meat and milk; and the board feel that the governors and subscribers will fully acknowledge that such increase is not great when the high prices of the year are considered. There is no doubt that the hospital will have to pay a still increased price, at least for meat, during the current year. The balance at the bankers, including that of petty cash, on 31st December was £120 4s. 4d.

"The governors and subscribers will clearly see, notwithstanding the general encouraging nature of the financial report, what efforts are required to keep up the efficiency of the hospital, and how a larger reserved fund would aid them. The board are therefore induced to call the attention of the governors and subscribers to the following remarks which were reprinted, by the kind permission of the editors, from the June number of 1870 of the *Homœopathic Review*, and inserted at the end of last year's

report. As it is probable very many have not read these remarks, the board now propose to insert them in the body of the present report, feeling as they do, how completely the claims of this hospital have been urged and advocated, not by the board or those connected with the hospital, but by parties from without, bystanders who read rightly its position and need. The remarks were as follows :

“ ‘ No public charity of this or any other kind can exist without funds. The sources whence the means of support to the hospital are derived are two—the endowment fund and the subscription list. The former is rapidly increasing, and now amounts to £6,600 (independently of the building and furniture, worth at least £10,000), and yields an income of £167. This is the fund to which public attention should be especially directed ; and were a strenuous effort made to obtain an addition to it of £20,000, the results to homœopathy would be of incalculable value. It is a sum which, we believe, could be obtained. When one considers the large number of people—and of wealthy people, too—who are indebted to homœopathy for the measure of health they enjoy, who have fruitlessly sought relief from many other sources, but have obtained it from homœopathic treatment, twenty thousand pounds among so many is really no large sum. Two hundred subscribers of one hundred pounds each, or four hundred of fifty apiece, are all that are needed. Surely such a number of persons, willing and able to subscribe in this manner, would not be difficult to find, even in London alone ; while this is an institution which has claims upon all who believe in homœopathy throughout the country ! Twenty thousand pounds would at once place the hospital beyond the risk of having its usefulness checked by a deficiency of income ; and this is a risk which all must desire to assist in avoiding.

“ ‘ Scarcely less important is the list of annual subscribers. The rate of progress is satisfactory, but quite capable of being improved upon, without drawing very considerably upon the purses of the large number who regularly resort to homœopathy in the hour of sickness. There are at least 800 medical men who openly practise homœopathy in this country, and in all probability as many more who do so secretly. With these latter we cannot deal ; but from those who acknowledge homœopathy to be true, we might reasonably expect more help than we receive at present. Were every homœopathic practitioner to present the claims of the hospital to the consideration of only a few of his patients, each might without difficulty obtain an annual contribution of £5, and thus produce an income of £1,500 a year.

“ ‘ In thus appealing to medical men and their patients, in all parts of the country, to give increased aid in supporting our hospital, we feel ourselves perfectly justified by the fact of its being a representative institution. It is not a merely local charity ; it is, as has often been urged on its behalf, an outward and visible sign that homœopathy has a hold upon the public mind. It is

also said that the degree in which homœopathy is esteemed by the public can be measured by the prosperity and usefulness of the hospital in Great Ormond Street. If this be so—and to some extent it undoubtedly is so—it is the bounden duty of all who desire the development of homœopathy to aid in rendering it as efficient an institution as possible. To this end money is a matter of prime necessity. Though the funds are well maintained and have in fact been considerably increased, more money is still required. More money means more beds, more patients, greater opportunities for doing good to the sick, greater opportunities for publicly representing the remedial power of medicines prescribed homœopathically. Hence we [trust that those who desire to witness progress of this kind will exert themselves to obtain more money.]

“The board would urge upon the governors and subscribers who take an interest in the hospital, as evinced by their subscriptions and donations, to do still more for it by advocating its claims on homœopaths generally, and who now pass by, to a great extent, what may be termed a Mother Hospital, in favour of local claims, which, however pressing, should not hinder support of an institution of the peculiar character which ours possesses.

“The necessity for an increase of the reserved fund becomes even more apparent when the position of the board, as regards the dinner given from time to time on behalf of the current expenses of the hospital, is looked at. With much regret the board, after every effort to organize such biennial dinner, have been obliged, as is well known to all present, to postpone it until next year, when the board have every reason to hope they will be well supported; but it is clear that a larger reserved fund would render these calls less frequent, and at the same time increase the efficiency of the hospital.

“The board of management, on the part of the governors and subscribers, have tendered their cordial thanks to Miss Alexander for her goodness in both commencing and, conjointly with the board, in carrying on the chaplain's fund, which in the autumn of last year was transferred to the management of the board alone. The board trust that those who have hitherto subscribed to it, as well as others, will evince their appreciation of Miss Alexander's endeavour to carry on such, by aiding that fund by their subscriptions.

“The board trust that according to the usually adopted mode, the governors and subscribers will vote their cordial thanks to Dr. Metcalf and Mr. Pope, who were appointed as the medical inspectors of the hospital, and who devoted considerable time to make a thorough inspection of all parts. Their report, which is satisfactory to the board, may be seen at length in the clerk's office.

“Messrs Crassweller, Hughes, Rosher, and Trueman retire in rotation from the board of management, but all being eligible for re-election, again offer themselves to serve.

"There have been two resignations during the past year by officers of the medical staff, both in charge of out-patients: viz., Dr. Moore, who was only elected the previous year, and who was compelled by temporary illness to resign, and Dr. Hale, who after his election to the internal duties of the hospital had kindly continued to see out-patients up to the end of January this year. The board of management, on behalf of the governors and subscribers, have conveyed to both these gentlemen thanks for their services. The board, by virtue of the powers vested in them, there being only one applicant for the post, have appointed Mr. S. S. Stephens as one of the medical officers in charge of out-patients. The confirmation of this appointment will be submitted to the meeting this day.

"In concluding their report, the board wish once more to state how invaluable have been the services of the honorary medical staff—services which, under Divine Providence, have the means of curing and alleviating much human suffering. The usual vote of thanks to them will no doubt receive the cordial approbation of the meeting, who will also be called on to thank warmly those ladies who have visited the patients from time to time, and have administered that sympathy which is doubly appreciated when coming from woman."

The Noble CHAIRMAN, in addressing the meeting, said: Ladies and gentlemen, I now come to a duty which I have performed annually for some years—to propose the adoption of the report which you have just heard read, and I think that next year I had better give the chair to somebody else, for I have addressed you so often on this subject, that I am afraid it is not possible for me to invent anything new or of a novel nature that you have not been accustomed to hear before. The committee have commenced their report by congratulating you upon the soundness and general well-being of the hospital. Such is the case, but I cannot say that it is altogether satisfactory to myself. We have certainly made progress under all our branches, and in the number of patients to whom the benefits of our system are extended, but considering what one and all of our medical men are, and the large number who are now engaged in the practice of medicine on the homœopathic system, I must say that £2,000 a-year is a very indifferent figure for the sum total that is contributed by the public for the sustentation of this hospital. (Hear, hear.) You have heard, and I entirely concur in those remarks which were read from the *Homœopathic Review*. I don't want to throw blame on any one here present; but I must say there are those absent medical men practising homœopathy in London and elsewhere, who do not take that notice of us that they ought to do, looking to the fact that they owe all their good fortune to the practice of homœopathy. I quite agree with the writer in the *Homœopathic Review* that if they would only exert themselves as they ought our balance sheet would show very

different figures to what it does now. (Hear, hear.) I get letters for this meeting from country homœopathic practitioners, and they make some objections to the manner in which the hospital is carried on, but they are of so trifling a nature that I shall not mention them on this occasion. They are of a carping nature, and I am almost ashamed that such objections should exist in the minds of anybody. What we are to consider is not whether the hospital is carried on in the way we should particularly wish or desire. We are told that our mode of electing our medical officers is not a good one—we are told that one gentleman is preferred to another. They are elected at a general meeting. All the subscribers have an equal interest and an equal power in the election of the medical staff of the institution. Everything that is done is done openly and above board, and no medical practitioner need complain of our management or of our system. Happily we do not quarrel among ourselves; the committee of management are all perfectly agreed, we give and take, and I really wish the faculty would lay aside any little objection they may have to this small thing or that small thing, and only keep steadily in view the general promotion of the system, to which nobody is more favorable or has a higher opinion than I have. (Cheers.) After forty years' experience of it, I do not hesitate to express my conviction that it bestows upon humanity greater blessings than any other system I am acquainted with, or of which I have ever read. (Loud applause.) And experience is like religion itself, true belief, and our first object should be to try and impart it to others. I do really think we have a fair right to expect that those medical practitioners who are now benefiting by the discovery of homœopathy should do what they can to promote the object we have in view. (Cheers.) There is nothing particularly new that calls for any observation in the pages of this report. In point of fact, the citation from the *Homœopathic Review* really exhausts most of the arguments that could be used to induce this meeting to adopt this report, and cause it to be circulated for the benefit of the hospital. I wish that those who only come occasionally to London would not confine their visit to its meetings, but would come and examine the hospital, and go through its wards, and really see for themselves what was being done. They would then be able to go back and tell their friends, from their own experience,—having looked at the hospital and gone through it, without any preparation being made for their visit,—exactly the style in which they found it. I have no apprehension, no fear whatever, that any one would go away dissatisfied. From our official manager (Mr. Trueman), and from the ladies who visit the hospital, I learn and hear a great deal about it, and I have no hesitation in saying that *it will stand comparison with any other institution in London*. I am glad to see more people than usual at our meeting to-day. The absence of many others shows the

immense confidence they have in the institution and its management, but I should like to see a little larger attendance at the meetings. The French say it is no use preaching to people who are already converted; in the same way it is no use a president scolding a public meeting—they having done their best by appearing here themselves—because it was not larger. (Laughter.) We are rather in hard times just now. There has been an enormous drain upon the pockets of the benevolent to soothe the distress and to relieve the sufferings of a neighbouring nation, which has suffered from a severe war, the most terrible, perhaps, of which history gives any example. We are told that when people once begin to give they are fond of giving, and as they tell us of habits, though easily acquired, are not so easily got rid of, we may hope that this is one that will remain with them for the rest of their lives. (Cheers and laughter.) I beg to move the adoption of the Report.

Mr. GOULD: I beg to second it.

The resolution was put to the meeting, and carried unanimously.

Mr. POPE said he rose with much pleasure to propose the next resolution—viz. "That the best thanks of the meeting be given to the Board of Management, to the House Committee, the Treasurer, Sub-Treasurer, and the lady visitors, for their zealous exertions during the past year." (Hear, hear.) He was quite sure that any one who would take the trouble to look at the institution, and to pass through the wards, would feel that all who were interested in its welfare owed a deep debt of gratitude to the Board of Management for the excellent arrangements they had made for the comfort and well-being of the patients, for the convenience of the medical officers, and for the comfort of the whole household. (Cheers.) He was sure, therefore, they would cordially support him in moving the resolution. He was happy also to have the opportunity of offering their thanks to their Treasurer and Sub-Treasurer—two officers who had rendered most efficient service to this hospital now for many years. They had an evidence that afternoon of the great interest which the Treasurer had taken in it. He remembered his name being mentioned as one of the donors to the hospital this year. Finally, they had to acknowledge with gratefulness the services of the lady visitors to this institution. These ladies went round the wards of the hospital, week after week, sympathising with the patients in their sufferings, and seeing that all their wants were properly and duly attended to; and, he was sure, the fact that ladies did this, must be a great comfort to all who had friends admitted as patients to these wards, to know that there was this mode of supervision at the hospital; and, he believed, it was the case that any observations made by ladies was recorded in a book kept for that purpose. (Hear, hear.) He would, therefore, hope that they would thank the Lady Visitors, the Treasurer, the Sub-

Treasurer, and the Board of Management; and all he would say in addition was, that he most heartily desired that, with such willing servants connected with the Institution, there was much more work for them to do. He could wish that there was a larger number of patients for the lady visitors to take care of, and that the Board of Management had larger scope for their administrative skill. (Cheers.)

Mr. SLATER had very great pleasure in seconding the resolution. He did so as a layman, and he was quite sure that any one acquainted with an institution of this kind will join heartily in the vote of thanks to the Board of Management, because to have a number of gentlemen giving their services, and managing to keep within their income, as the Committee of this Hospital did, was something to be thankful for. When they heard of gentlemen making complaints against the management, it was only what was the case in all institutions, and therefore very little notice need be taken of it. If they were to say less they would probably do more. Those who talked so much were rarely givers. On the other hand, the workers were the greatest givers, and the workers here were generally the workers also in a great many other charities, and the labours which these gentlemen gave sometimes for the good of the public was something beyond calculation. He was sure the noble Chairman would bear him out in that statement, therefore in passing this vote of thanks they would only be doing what those ladies and gentleman really merited. (Hear, hear.)

The resolution was put to the meeting, and carried unanimously.

The DEPUTY CHAIRMAN (Mr. John Boodle).—Gentlemen, on behalf of the board of management, I beg to thank you for the vote you have been so good as to pass. I would thank you also on behalf of the treasurer, who is absent, and also on behalf of the lady visitors.

Mr. CRAMPERN then moved the confirmation of the election of Mr. S. S. Stephens and Dr. Wheeler to the medical staff of the hospital.

Mr. BOODLE seconded, and the resolution was carried unanimously.

The Rev. JAMES HILDYARD next moved that the cordial thanks of the meeting be given to the medical inspectors, Dr. Metcalfe and Mr. Pope, for their able report after their inspection of the hospital. He was sure the inspection of these gentlemen was a thorough one, and was calculated to promote the advantage and welfare of the hospital.

Mr. ELLIS, as a member of the board of management, rose to second the resolution. He could bear testimony to the care and exactness with which these gentlemen had performed their duties. If they would take the trouble to read the report of these gentlemen, they would be convinced that what they undertook to do

had been properly done. (Hear, hear.) Therefore, he had the greatest pleasure in seconding this resolution.

The CHAIRMAN said he ought, perhaps, to remind the meeting that these gentlemen were not members of their own medical staff. They were anxious to get a thoroughly impartial and independent report, and it was on the table if any gentlemen would like to read it. These gentlemen came and inspected the hospital; they took a great deal of pains and a great deal of trouble, and if they pointed out anything that they thought should be remedied, the committee of management endeavoured to do their best to carry out their suggestion. (Hear, hear.)

The resolution was put to the meeting, and carried unanimously.

Mr. POPE, on the part of himself and Dr. Metcalfe, thanked the meeting for the vote of thanks. It had given them very great pleasure to inspect this hospital, inasmuch as the inspection had been most thoroughly satisfactory. (Hear, hear.) This had been so on one point especially, and that was a most important one—the matter of ventilation. Every ward they went into—and at the time they were tolerably well filled—the air was perfectly fresh and sweet. That was a matter of very great importance; and it was a great deal to say for a building like this, which had not been originally constructed for a hospital, but adapted for hospital purposes; it was a great thing in this case to say that the ventilation was exceedingly perfect.

Mr. CHARLES TRUEMAN (the Official Manager) said that it had fallen to his lot to ask them to consider the next resolution, which was one which he was sure would commend itself to their cordial approval. It was that the cordial thanks of the meeting be given to the medical staff for their valuable services, and for the care and attention paid to the sick patients under their care during the past year. He was not, however, sorry that it had fallen to him to propose this resolution, and they would see after he made the few observations he was about to do, that he was justified in that remark. It would be very easy for the board of managers to tell them, irrespective of the general duties of the medical staff of the hospital, how very anxious they always were to aid the board by their advice and by their counsel, on all occasions when it was sought for. It would be still easier on the part of the house committee to tell them certain facts connected with the medical staff which came before them; but there was one he would mention. Every week each patient on leaving the hospital went before the house committee, certain questions were put to them, and the answers to these were recorded in their book. These questions were very simple ones; they were questions as to the sufficiency of food, the goodness of the food, whether such was well cooked, whether the nurses were kind and attentive, whether they had given the nurses any sum of money, or had been solicited by them to do so, and

finally they were asked, "Have you any complaint of any kind to make since you was admitted into the hospital?" The reply to this was generally, "No, no,—many thanks for what has been done for us;" but to that was generally appended an expression of thanks to the particular medical officer and the house surgeon under whose charge they had been. (Cheers.) This was what the Board of Management and the House Committee could tell them; but he would go a step further, and tell them what fell to his lot only to know. Now, there were a great many points which arose in the Hospital, which were really points of value to the House Committee; points upon which he was very glad to have the counsel and advice of the Medical Staff. In matters of various arrangements, the Medical Staff were the best counsellors they could possibly have, and during the past year, as previously, they had always been most anxious to do all they could to assist them. But he would also say this, there were certain days—two days every week—on which the attendance of the internal Medical Staff at the Hospital was imperative; but he had known these gentlemen, when they had dangerous and difficult cases under their charge, to come to the Hospital on many other days of the week, when their attendance was quite a matter of their own choice. This was a matter that did not come prominently before the notice of the Board of Management or the House Committee. Thus they would see that they did positively owe a deep debt of gratitude to their Medical Staff for all they were doing and had done; and he had great pleasure in moving this resolution. (Cheers.)

The resolution was seconded, and carried *nem con.*

Dr. HALE said he had been called upon very unexpectedly to return thanks for the vote; he thought that duty devolved upon the junior member of the medical staff. He had, however, very great pleasure in expressing their acknowledgment of the very kind and flattering way in which Mr. Trueman had mentioned their services, and, on the part of his medical colleagues, he could only say that it had been to them a matter of very sincere pleasure, a great and real pleasure, to have satisfactorily fulfilled their duties. When they came there to take cognisance of special cases, on days when their attendance was not compulsory, they were merely fulfilling a duty which every medical man would do. None of them felt it an onerous duty to do that which was an essential part of their daily life in fighting attacks of serious disease. On the part of his colleagues he thanked them most sincerely for their vote.

Dr. HALE again rose and said he had been requested to propose the next resolution—the re-election of the four gentlemen retiring from the board of management, Messrs. Crasswaller, Hughes, Rosher, and Trueman. Among those they would notice the name of their indefatigable and zealous official manager, Mr. Trueman. It would be invidious, perhaps, to single out any one gentleman

for special mention, because he was quite sure all were most earnest in the duties they had to discharge ; but he thought he might be permitted to refer again to one name, which certainly, as connected with this institution, was a household name—that of their official manager, Mr. Trueman. (Cheers.) As the medical men's duties had been referred to, he might say that he only expressed the feelings of the medical officers when he said that but for Mr. Trueman their efforts would not be as successful or as agreeably performed as they are now. (Cheers.) In all cases of difficulty when it had been desirable to ask questions of, or for advice of, Mr. Trueman, that gentleman was always found ready and ever willing to give them his assistance. (Cheers.) He, therefore, had no hesitation in proposing this resolution.

This was seconded.

The CHAIRMAN said he had the honour to be the chairman of the committee of management, and he entirely concurred in all that Dr. Hale had said with regard to Mr. Trueman. They had plenty of examples of the truth of what Dr. Hale had stated, that without the intervention of Mr. Trueman, things would not go on so satisfactorily as they did. (Cheers.) He could not except any one of the other three gentlemen proposed for re-election : so long, to use a slang phrase, as he continued to drive the team, he should be very sorry if these four horses were taken away from him. (Cheers and laughter.)

The resolution was put to the meeting and carried unanimously.

On the motion of Dr. DRURY, a cordial vote of thanks was then accorded to the noble chairman for his conduct in the chair that day, and for the general interest he took in the hospital—the doctor remarking that as the noble lord had done so much for homœopathy, he hoped homœopathy would do a great deal for him, in giving him a long life and good health.

The Noble CHAIRMAN, in response, said : Ladies and gentlemen, Dr. Drury expressed a hope that “ homœopathy will return the compliment.” (Laughter.) Now, in point of fact, I am returning a compliment to homœopathy, which has done so very much for me. I have myself derived so much benefit and advantage from it that I think it my duty to do everything in my power to endeavour to convey the blessing to others. (Cheers.) I do hope and trust we shall exert ourselves to the utmost in the future, and that those homœopaths who now practise in secret, because they may thus evade abuse, will come forward openly, and show the world what the benefits to be derived from homœopathy really are. (Loud cheers.)

The meeting then dispersed.

REVIEWS.

The Hahnemann Materia Medica, Part II, containing *Uranium nitricum*. By E. D. BLAKE, M.B. London : H. Turner & Co.

THIS long-suspended undertaking has, we are glad to see, once more been set going. Several medicines are announced as in preparation : and this second part, now published, adds to the *Kali bichromicum* of Dr. Drysdale, the *Aconite* of Dr. Dudgeon, and the *Arsenic* of Dr. Black, an arrangement of *Uranium nitricum* by Dr. Edward Blake.

Dr. Blake's choice of his medicine was obviously dictated by the special labour he had bestowed on it in time past, of which the record stands in Vol. XXVI of this Journal. We might otherwise have cavilled at his selecting so unimportant a member of the *Materia Medica* to follow the polychrests hitherto treated. The assertion of Lecomte, that he "always found sugar in the urine of dogs slowly poisoned by small doses of *Nitrate of Uranium*," naturally led homœopathic physicians to try the drug in the treatment of diabetes mellitus. Dr. Blake instituted his experiments and provings "with the view of supplementing and elaborating the researches of Lecomte." But they led to this curious result, that neither in dogs or any other animal, nor in the human subject, did the Uranium cause the urine to become saccharine. While, on the other hand, it was found to exert a hitherto unknown action on the stomach and duodenum, especially in causing ulceration near the pylorus. Dr. Blake compares this action to that of *Arsenic* and *Kali bichromicum*.

The second part of the *Hahnemann Materia Medica*, accordingly, contains the experiments and provings made in 1867 both in full, and arranged in the usual schema form. To them he has appended physiological comments and therapeutic inferences ; and, in an appendix, the "thera-

peutic history, action, and cases of cure;" embracing all that has been written upon the drug in our literature.

When we add that the work has been thoroughly done we think we have said enough to induce every non-subscriber to the Hahnemann Publishing Society at least to possess himself of Dr. Blake's *Uranium Nitricum*. But he would do better still to be a non-subscriber no longer, but rather to help with purse and pen the excellent objects of the Society.

On Chronic Diseases of the Organs of Respiration ; being a series of Clinical Observations on Diseases of the Air-passages and the Lungs. By JOHN MEYHOFFER, M.D., &c. &c. Vol. I. London : H. Turner & Co.

THIS work is too important to be dismissed in the page which would be all we could now accord to it. In our next number we hope to give it a notice in length at least proportionate to its value. In the meantime, while sure that it is needless to commend it to those who have read Dr. Meyhoffer's contributions to our journals, we bespeak the attention to it of all who have at heart the advance of homœopathic therapeutics.

The Diseases of Children. By Prof. VOGEL. Translated by Dr. RAPHAEL.

CONSIDERING the time that this book has been in the hands of the English-reading part of the profession, it does not appear to have engaged that amount of attention given to the other works of the celebrated physiologist. Indeed we must say that we should have expected from the pen of Vogel a far more carefully-written book.

Taken as a whole it is a disappointing book, and for breadth of thought, combined with original and accurate observation, cannot compare with *The Wasting Diseases of Children*, which, by the way, though it is fathered by Dr. Eustace Smith, is really a transcript of Sir William Jenner's *Clinical Lectures on Tuberculosis, &c.*

Certainly the articles *Sclerema* and *Sclerosis* are excellently written, and add much to our scanty store of knowledge of these medical curiosities, but these are and ever will be only curiosities. Not so *Enteritis folliculosa*, the pathology and diagnosis of which are portrayed admirably.

The much-vexed question of the *præ-* or *post-mortem* occurrence of stomach-softening is well discussed and put beyond further dispute.

When Jenner's celebrated little *brochure* on typhus and typhoid has been so long in existence it seems strange to read a work wherein two such different diseases are confounded and described as one.

In enumerating the conditions predisponent to prolapsus ani in children, Vogel, to our surprise, omits altogether the cause *par excellence*, viz., calculus in the bladder.

The author gives us some valuable and interesting information as to the clinical worth of the dicrotic pulse, when it occurs in children under ten years of age.

There is also an amusing anecdote based on the abominable practice of preventive lactation—a physiological offence that seems to be even more terribly rife in the States than with us. A young American gentleman, aged 2 years, is brought by his nurse at the usual hour to receive his customary sustenance from the maternal fount; he draws back, exclaiming, "Oh, no mamma! I cannot nurse any longer, it is so extremely tedious."

There is one practical point of great importance which, if true, ought to be more generally known. At p. 44 we read "the important difference between woman's and cow's milk is to be found in this, that the casein of woman's milk curdles in the stomach into small, light flakes, forming a very loose jelly, while that of the cow coagulates into large

compact lumps. Of this one may convince himself by causing a child brought up at the breast and one artificially reared, to vomit a quarter or half an hour after the meal. I have been in the habit of using, for some time back, a solution of *Carbonate of Soda* (3j ad aq. 3vj), a teaspoon of which is added to the milk at every meal."

Amongst the pieces of treatment (as a rule, the old story), we note that Prof. Vogel recommends *Arg. nit.* for the *anorexia* of jaundice, and for *gastric catarrh* (vomiting of mucus) in children. .

As regards the translation itself, on the whole it gives a fair rendering of the author's meaning. But one cannot but regret that what should be a standard work is disfigured by such spelling as "scorbutis" and "coagulæ," nor can these be mere clerical errors, for whilst the latter is repeated twice, we counted no less than four recurrences of the former.

What Dr. Raphael means by "uro-poetic" viscera we fail to see; the intestines might under certain conditions be termed musical, but we must say that a poetic kidney is beyond our comprehension.

Those amongst us "versed in curious lore" may know what is the nature and constitution of the following substances: "*Ung. lin.*, p. 188; *Mont. Pomat.*, p. 160; *Acid Halleri*, p. 192; and *Salep. water*, id.;" but as the work is intended for that hard-worked animal, *The General Practitioner*, a small vocabulary, or at least explanatory notes would not have been amiss.

The book is well got up and, though the paper is thin, the type is clear and facile.

It is certainly too diffuse, and the 600 pages suggest a vague suspicion of "book-making;" it would boil down with advantage to two thirds the size.

CLINICAL RECORD.

Stannum Headache. By DR. DUDGON.

A nurse in my own service, aged about 40, became subject to a very peculiar headache. She was in perfect health otherwise, and rose up in the morning apparently quite well, but soon after rising, and that whether she ate breakfast or not, there came on a pain over the eyes, dull and stupefying, sometimes more over one eye (the left) than the other. This pain increased gradually in intensity and involved the whole forehead, attaining its climax in about two hours, after which it as gradually subsided, and in about two hours more was quite gone. The character of the pain was pressing in, cramping, crushing. I tried several remedies in vain until I found the almost exact parallel of the case in the symptoms of *Stannum*. I gave this medicine in the first trituration, and the headache which had tortured the poor woman for several weeks was cured completely in a couple of days. She had no return of this headache until upwards of a year later, when a few doses of the same preparation of *Stannum* again conjured it away.

Calcareo Muriatrica in Porrigo Capitis.

By G. H. BUTLER, Esq., L.S.A.

E. P. (daughter of a dairyman), æt. 12. This case, the most severe I ever witnessed, came under my care on the 14th of April last. It had been much neglected and no medical treatment had been adopted for six or seven weeks, so that I am unable to describe the primary symptoms. When I first saw the little sufferer, the entire scalp was covered with dark-coloured, hard, thick, incrustations, emitting a most foetid odour; nor was the disease confined to the head, but had extended to the face, neck, arms, and legs; the itching had been most distressing, I was informed,

in the first stages of the malady, but had somewhat abated when my attention was directed to the case. Carefully removing every remaining portion of hair, I directed the head to be well washed every morning with soft soap, and a poultice of bread and water to be applied at night, and prescribed (as recommended by Dr. R. Hughes in his valuable work, *A Manual of Therapeutics*) *Calc. mur.* 1, gtt. iij every four hours. This treatment was continued till the 20th, when *Graphites* 3^x trit. was given for a few days, after which I reverted to the *Calcareæ*, and daily improvement was the gratifying result. Deeming now that a short course of *Sulphur* might expedite the cure, I gave her three drops of 3^x dilution three times a day till the 9th May, and then resumed the first-named remedy, which was persevered with till the 20th, when every vestige of the disease had disappeared. I believe that, for a case of so malignant a character, the cure may be considered to have been unusually rapid.

Plumbum cures.

1. By Dr. LOEBACHER.

Mrs. Kùchler, 58 years old, a labourer's wife, sought my advice in the latter part of November, 1870. She looked pale and rather emaciated, but, with the exception of a tendency to constipation, she has never been seriously ill. She even got over her climacteric period without any particular ailments. Her present malady began about three weeks ago, without any ascertainable cause. Her first uncomfortable sensations commenced after carrying a heavy basket to her abode up five pair of stairs. Her motions, which were always rather costive, became more scanty and more difficult to pass, and she could not get a satisfactory evacuation with either *Castor oil*, infusion of *Rhamnus*, or injections; they only brought away small lumps of healthy-looking fæces. The abdomen began to swell and increased daily in size, till at length it attained the dimensions to be presently described, and then the ever increasing pains, loss of appetite and vomiting compelled her to seek medical aid. On turning down the bed-clothes I was struck by the enormous size of the abdomen, which

was as large as that of a woman in the seventh month of pregnancy. The swelling was greater on the left than on the right side, and completely filled up the space between the spine and the navel. The convolutions of the bowels filled with hard fæces could be distinctly seen and felt. On the right side it felt exactly as if a bag not quite filled with fluid lay there; but no morbid condition of any part could be detected there. The swelling was not very tender to pressure, but prevented the patient lying on her side. There occurred occasionally very violent, spasmodically contractive, cutting pains, with great restlessness, anxiety, and cold sweat; whereupon lumps the size of the fist appeared on the left side. After eating there always ensued violent retching and vomiting, sometimes of the food, sometimes of mucus. Loss of appetite with tolerably clean tongue, great thirst, flow of urine only slightly diminished and of normal appearance, the skin somewhat shrivelled. Not a trace of fever, headache, or other morbid phenomena. I need scarcely add that she was extremely weak, pale and hollow eyed. Although the characteristic board-like drawing-in of the abdomen towards the spine was absent, the other symptoms pointed so decidedly to *Plumbum*, that without hesitating an instant I prescribed it to be given in the dose of three drops of the 5th dilution in a tablespoonful of water every three or four hours. It required four doses of the medicine to produce its full effect. After the above-described pains becoming more and more violent, though occurring at longer intervals, there occurred in the night between the fourth and fifth day a movement in the accumulated fæces which resulted next day in the evacuation of a large painful of excrement, and thereupon the swelling, pains, vomiting, and other morbid symptoms were removed. In addition to the general debility it is not surprising that there remained a local weakness of the bowels which had been so excessively distended, that rendered it difficult for her to retain the fæces. But this is improving and will, I believe, be soon removed. (*Pop. Hom. Ztg.*)

2. By Dr. DUDGEON.

F. A—, aged 33, a cheesemonger's man, consulted me on the 21st May, 1871. His occupation was to serve in the shop and take the goods out to customers, which involved driving about in a tradesman's cart, with much getting off and on again, and run-

ning up and down area steps with heavy parcels in his hands. He had been gradually getting weaker for the last nine or ten weeks, and he now felt almost unable to get through his work. During these nine or ten weeks he has suffered from constant sickness, acid eructations, vomiting every article of solid food he takes, so that he has latterly been obliged to give up meat and bread and limit himself to beef tea, and occasionally a raw oyster or two, keeping himself up with frequent sips of brandy and other spirits. He has constant dull pain in the hepatic region, violent attacks of enteralgia, constipation, so that his bowels will not act without a purgative, and the motions are hard and dark, numbness of extremities, both upper and lower, and a sort of semi-paralytic condition of the arms so that the knife will frequently drop from his hand. The expulsive power of the bladder is much weakened, and his urine is only expelled with difficulty and in a dribbling manner. The urine high-coloured and foetid. Sexual functions much weakened. Pulse weak and quick. The symptoms reminded me so strongly of the effects of lead-poisoning that I at once looked to his gums, half expecting to find the blue line there. There was, however, nothing of the sort, and I could not find that in his occupation he ever came in contact with lead. He was so extremely weak, and so unable to take nourishment, and had lost flesh to such an extent, that I told him if he was not decidedly better in a week he had better go into hospital. I gave him *Plumbum carbonicum*, 1 trit., one ninth of a grain, three times a day. On the 28th he returned. He was much better in every way. The sickness gone; he can eat meat three times a day. No more pain in the bowels. The water clearer, and he can pass it without difficulty. Bowels open regularly every day; evacuations healthy-looking. Is much stronger and able to get through his work with comfort, and he has hardly a trace of the numbness of extremities remaining. I saw him again a fortnight later, and though, in consequence of over-work, owing to the illness of a fellow shopman, he was suffering from exhaustion and dyspepsia, there was no return of the former serious symptoms.

Though there is no actual proving of *Plumbum*, there is no substance whose general physiological action is better known to us through the recorded cases of poisoning. Lead-palsy, lead-colic, and lead-gout are diseases of as decidedly specific character, and as well known as any in the nosology, and hence we are

enabled to employ lead for homœopathic therapeutic purposes with almost as much certainty as our best proved medicines. Indeed, we actually know more of the pathology of lead diseases than we do of many of our best proved medicines, for in the case of lead the provings have often been pursued to the bitter end, *i.e.*, the fatal termination, or at least to the production of serious diseases, whereas the most heroic homœopathic provings stop short of the production of fatal or even serious disease. It were desirable, however, to subject lead to a thorough homœopathic proving in order to elicit those finer shades of symptoms that are so useful to us in the selection of the remedy, and were this done our knowledge of the action of this valuable metal would be as nearly perfect as it could be; and it would undoubtedly play a more conspicuous part in our therapeutics than it has hitherto done. In the whole of Rückert there are only twelve cases recorded as having been cured or benefited by *Plumbum*, and one of these is repeated to make up the dozen. The diseases are, namely, spasmodic pains in stomach, constipation, paralytic state of legs after confinement, semi-paralysed state of the outside of one leg, a good case of hypopion after iritis with tearing pain at night in forehead, a case of paralysis of œsophagus, amelioration of some of the symptoms of phthisis, and a case of sciatica cured in a week. Raue (1870) gives two cases cured by *Plumbum*, one of constipation with hard abdomen and paralytic weakness of extremities, another of obstinate diarrhœa and constipation, with retracted abdomen.

Chapman (*Brit. Jour. of Hom.*, iii, 170) gives three striking cases treated successfully by *Plumb. carb.* Two of these were chiefly mental maladies and the third was one of painful spasm of the rectum like that connected with fissure. There must be many more diseases for which *Plumbum* is the remedy for such a powerful metallic poison must have a wide range of therapeutic power.

MISCELLANEOUS.

Ohloral Hydrate.

By EDWARD T. BLAKE, M.B., of Reigate.

In the *Monthly Homœopathic Review* I have recently perused with much pleasure the remarks of my friend Dr. Dyce Brown on the results, with regard to the visual sphere, of the administration of *Ohloral Hydrate*.

Some months since I had the opportunity of seeing a remarkable effect of this drug on the upper eyelids, in an overworked professional man of bilious temperament.

Temporary *ptosis* twice followed the exhibition of the ordinary dose of *Chloral*, for chronic and obstinate insomnia.

In another instance, the wife of one of our colleagues, after a dose of *Ohloral*, becoming delirious, hurled a hot-water bottle at an imaginary figure which stood menacing her at the foot of the bed.

Again, this agent has been accredited with the induction of a fatuous state in a medical man occupying a high position in the profession.

The latter cases would go to prove the power of this drug to produce primary disturbance of the sensorium; and they serve as well to explain some of the success attending its use in certain mental disorders.

These facts certainly show that on the one hand Dr. Brown has good grounds for saying that "the medicine in question is a very powerful one, producing marked physiological symptoms, and therefore valuable conversely as a therapeutic agent to homœopaths;" on the other hand, that the assurance given by "the profession" to the public of the entire innocuousness should be received *cum grano salis*.

A good proving of its minute effects, combined with a careful compilation of the general literature of the subject, were desirable.

The late Sir John Herschel.

The following letter from the illustrious philosopher whom science deplores is not without its sad interest. It shows that up to the very last he was highly interested in all matters bearing upon any of the physical sciences in which he was so eminently versed, and that even a trivial invention, such as that alluded to, was at once apprehended by him in all its bearings, and engaged his active mind to such a degree as to cause him to pen a letter to the unknown author.

I sent a copy of my first article on the "Dioptrics of Vision" to several scientific gentlemen, and among others to Sir John Herschel, through his publishers, Messrs. Longmans. A very few days afterwards I received the following letter:

" COLLINGWOOD; Jan. 22nd, 1871.

" SIR,—I beg to acknowledge, with thanks, your paper 'Notes on the Dioptrics of Vision.'

" The idea of employing a double concave *air lens* in water instead of the more obvious double convex *glass* one is ingenious, as it ceases to *be* a lens as soon as it is out of the water, and (barring distorsion from wet external surfaces) would not impede vision if used as spectacles out of water. Excuse me, however, if I remark that the way in which the principle of construction is stated in page 8, line 10, &c., had to myself, on a first rapid perusal, the effect of creating some degree of obscurity as to the actual disposition intended, which was only dissipated, as I read on, by the use of the term 'air lens' in page 15, line 8, when all became clear.

" I have the honour to be,

" Sir,

" Your obedient servant,

" J. F. W. HERSCHEL.

" P.S.—On reperusal I see the principle is all very clearly stated in page 5, line 10 et seq. But this, by turning over two leaves at once, I unluckily missed.

" P.S.—M. Chossat long ago pointed out the non-sphericity of the crystalline lens, and showed that the curvature was that of an

oblate spheroid, the central portion being less curved than the exterior.

"Dr. DUDGEON."

Homœopathy at the Antipodes.

Homœopathy shows signs of vitality in the remote antipodes, to judge by the publications that have lately reached us. From Melbourne we have some numbers of a periodical entitled *Australian Homœopathic Progress*, edited by J. W. GÜNST, M.D. The first thing that strikes us in this periodical is an astonishing allegorical picture on its outside. On the left hand a bust of Hahnemann, squinting horribly, surmounts a pillar, on which is inscribed the motto, "Magna est veritas et prævalebit." Opposite to him a hand issuing from the sky holds what looks like a lens, such as is seen on most drawing-room tables for examining photographs, only from the dispersive power it seems to exert upon the rays of light, we must judge it to be a concave lens. Round the brass rim of this lens is the legend, "Post nubila lux." At the foot of Hahnemann's pillar sits a youth, with his hair well brushed and greased, clad in nothing but a very scanty shirt, and apparently engaged in writing in a book with a knitting-needle. On one page there is inscribed "Similia similibus," and on the opposite he has just finished writing "curantur," which he has evidently written backwards, as his needle has just completed the last curve of the "c." We presume that his writing in this inverted manner is intended as a delicate compliment to the Chinese inhabitants of Melbourne, who are understood to prefer writing from right to left. Having completed his task, the youth is looking straight at a broken fluted column, round which winds a ribbon, on which is inscribed "Contraria contrariis curantur." Near this broken fluted column stands another column, equally fluted and equally broken, and at the foot of both are three books, bearing the names respectively of "Galen," "Ambrose Parry" (*sic*), and "Cullen," while two jam pots, with paper covers, are labelled "Blister" and "Leeches." Books, jam pots, and columns are partially obscured by a dense smoke, inscribed "Cautery," proceeding from an open grate, with a poker sticking into it. Close beside the grate are some horrible pointed black objects, probably

tomahawks or boomerangs, or other native weapons. At the foot of the youth in the "cutty sark," are a portfolio, two sheets of paper, a pair of compasses, and a black object, respecting which we are undecided as to whether it is a constable's staff, a sandwich-case, or a piece of india rubber. The background is occupied by a series of precipitous hills, on one of which stand two rather shaky pine trees, and over the summit of another of which a gigantic waterfall bursts, descending by two leaps into a placid lake, with a black rock in its middle. What the meaning of it all is we have not a notion; but perhaps it is rather an advantage for an allegorical picture to be incomprehensible to every one but the artist, and no doubt, were he to explain it to us, we should see its complete appropriateness.*

But whatever the merits of the frontispiece, there is no doubt that the letterpress contains interesting and valuable information, and we are pleased to see that our cause has its organ in the great and flourishing colony of Victoria.

The neighbouring island of Tasmania has a representative of homœopathy in its capital, Hobart Town, and he has started an organ called *Notes on Homœopathy*, several numbers of which lie before us. This periodical lets us see that though homœopathy has but one professional representative in Van Diemen's Land, it causes as much stir as it does in other places where it has many representatives. There has been a sensational coroner's inquest on the body of a man who died under homœopathic treatment of inflammation of the brain. As patients affected with that disease never die under allopathic treatment, numerous representatives of that school were eager to offer their opinion to the jury that the homœopathic treatment was to blame for the patient's death. The jury, however, proved to be of a different opinion, for they returned a verdict of "Died from natural causes," much to the disgust of the allopathic veterans, who seemed to be of opinion that the very least the homœopathic doctor deserved was to be hanged, drawn, and quartered. Probably they will tire of getting up coroner's inquests when they find that juries are not disposed to return verdicts of "wilful murder" against the medical attendant, simply because he happens to be a homœopath.

Dr. Atherton seems to have far from an easy time of it in Tasmania, for in addition to the coroner's inquest, he has been

* In the 9th No., received since the above was in type, this allegorical frontispiece is omitted.

engaged in a paper war with a Dr. Agnew. The hostile correspondence is published in a double number of the *Notes* for January last, and is very creditable for our Tasmanian champion. His antagonist seems but a weak exponent of the arguments on the other side, and Dr. Atherton has little difficulty in repelling his very feeble attack.

We wish all success to these two magazines of our school at the antipodes, and we are sure that both will powerfully contribute to spread a knowledge of homœopathy in those distant lands, and increase the number of its practitioners.

If we might offer a suggestion to our Australasian colleagues, it is that, as there must certainly be many and powerful drugs native to their soil, it would be highly desirable that means were taken to collect information concerning them. Their medicinal employment by the aborigines (Australia still has some, though Tasmania has improved her's off the face of creation) cases of accidental poisoning by them, and even original provings might be collected. Such work would give a character of originality and value to the homœopathic journals of these parts, which they can never obtain if they are merely a pale reflex of the homœopathy of this part of the world.

Desertion.

WE have received from the author the following copy of a letter addressed to the Honorary Secretary of the British Homœopathic Society.

“ 10th June, 1871.

“ DEAR DR. MADDEN,

“ It is due to you that I should let you know as soon as possible that I have sent to the *Monthly Homœopathic Review* an article which I do not think can be reconciled with the exclusive profession of homœopathy. I cannot convey to you the reluctance which I feel towards giving up the cherished idea of healing all diseases by specific medicines ; but I am convinced that in the present state of our knowledge the attempt is perfectly Utopian. It appears to me, therefore, to be a manifest duty to relinquish the sectarian title of ‘ homœopath,’ and with it inceptive membership in the British Homœopathic Society. In doing so I beg to convey to you, as Honorary Secretary, my high

appreciation of all that I have learnt from attending the meetings of the Society, and of the personal integrity, intelligence, and professional skill of most of its members with whom I have come in contact. That homœopathy will continue to exercise an increasing influence on the practice of medicine I fully believe (more particularly if some concession be made in respect of infinitesimals); but I think, at the same time, that it must ultimately take the place of a branch or department of medicine, and that the attempt to include under Hahnemann's rule all therapeutic principles can only result in failure and disappointment.

"I am,

"Dear Dr. Madden,

"Yours faithfully,

"H. R. MADDEN, Esq., M.D.,

"EDWARD HAUGHTON.

"*Hon. Sec. Brit. Hom. Soc.*"

Under ordinary circumstances we should have taken no notice of this communication. Dr. Haughton has not so made his mark among us that we can affect any regret at his loss to our ranks. Nor would it be any impugnement to our doctrines that they failed any longer to commend themselves to one who has shown his capacity of appreciating evidence by coming forward as an opponent of vaccination. But the reason assigned by Dr. Haughton for renouncing our fellowship demands some comment at our hands, more especially so as it is somewhat of an echo of that lately put forward by a better-known renegade. We speak of his assumption that in calling himself an "homœopath," and enjoying an inceptive membership of the British Homœopathic Society, he is bound to the "exclusive profession of homœopathy," and the practice of attempting to heal all diseases by specific medicines.

We can only say that if this assumption be warranted, we had better all of us drop the term "homœopathic," and any membership of the Society which pertains to us, at once. We have never made profession of such exclusiveness, nor do we in practice feel bound to use no means of healing but specific medicines. Dr. Haughton must know that it is so. He signed no formularies when he joined the Society. He has never found restrictions put upon expressions of opinion in its debates, or in the pages of this Journal, or of our *monthly* contemporary. No one has had any right to arraign his fullest liberty in prescribing. It is his private interpretation of his name and position which makes them untenable for him any longer. Let this be understood and admitted,

and we have nothing more to say. "*Conscientia erronea obligat*" is sound casuistry ; and we have only to remind Dr. Haughton of the danger, common to ourselves as to him, of mistaking the promptings of self-interest for the commands of conscience, and inventing pretexts where we imagine ourselves to be assigning reasons.

What then (it may be asked) do you mean by allowing yourselves to be known as homœopaths ; by sustaining a Homœopathic Society, and carrying on Homœopathic Journals ? We mean just this. We believe an important truth to be contained in the system of therapeutics inaugurated by Hahnemann. We regard *specific* medication, wherever it can be had, as by a long way the best mode of treating disease ; and we believe that the principle, "*similia similibus*," is the best guide to the discovery of the specific remedy in each case. We believe farther, that in carrying out this system Hahnemann and his followers have made many valuable practical discoveries, viz. in the matter of new medicines, of modes of preparation, of dose, of way of administration, and so on. We would gladly advocate and practise this creed of ours, undistinguished otherwise among our fellows ; but by some strange disorder of vision its maintenance has come to be regarded as a professional crime. To avow adherence to it means to be—so far as established institutions are concerned—ostracized, excluded, silenced. We are forbidden to advocate it in society or journal, to put it in practice at hospital or dispensary. What can we do, if we hold allegiance to truth, the supreme duty, but go with it without the camp, bearing its reproach ? The discussion, the development, the experimental application refused to it elsewhere, we are bound to supply ; and this we do by our homœopathic societies, our homœopathic journals, and our homœopathic hospitals. Their existence implies no sectarian feeling, nor any exclusiveness of practice. The truth they embody is positive, not negative ; it says, this thing is the best where it can be had, but it pronounces nothing as to the relative goodness of other things, or as to the range of its own applicability. We find these out by experience, and our distinctive position means nothing but a claim of liberty to exercise ourselves by word and act in obtaining such experience.

We owe it, then, to the truth whose vision has been granted us that we in nowise deny it, but at all risks serve it and bring it into action. We should be guilty of moral degradation did we

shrink from the consequences to us personally, and refuse to follow its leadings; or if we adopted its embodiments in practice while concealing the source of our inspiration. We speak for ourselves and judge no man. But we count him at least gratuitously blind who thinks he must leave our ranks because his short experience does not confirm the estimate of some as to the range of the homœopathic principle. Dr. Haughton "fully believes that homœopathy will continue to exercise an increasing influence on the practice of medicine." If this influence is for good, it might have been well for him had he paused ere he gave up his share—however small—in it; ere he did what in him lies to hinder it by passing over to the ranks of the enemy while the battle is waging. We, as long as we can only serve the truth by enduring its stigma, will so endure and so serve. If we greatly cared for such rewards, it would not be difficult to hear sounds of future acclaim to those who through evil report and good report keep the sacred deposit committed to them. But it is sufficient that we have an approving conscience, and clear, before the Higher Judge.

Conspicuous by Absence.

We had the pleasure of listening to the eloquent oration of Dr. T. K. Chambers, delivered in the hall of the College of Physicians, on the 21st of June. The subject was "Therapeutics," and the orator dwelt on the changes that had taken place in the mode of viewing disease of late years, which in place of, as previously, being viewed as something added to the vital processes, was now looked upon as something detracted from them—in fact, a deficiency to be supplied, not an excess to be abstracted. This had produced a marked change in the treatment now most generally adopted, which was mostly of a building up and supporting, in place of a depleting and enfeebling character. He then proceeded to consider separately the various modes of treating disease, as by contraries, heat to cure cold, purgatives to cure constipation, &c.; by counter-irritants, where a diseased action was artificially produced on a less important organ in order to draw it away from a more important organ. He mentioned also the treatment by alteratives, the action of which was of an inexplicable character. But the learned orator made no mention at all, not even the faintest allusion, to the treatment

- by specifics or medicines that act by reason of similarity ; a very significant omission, which certainly did not proceed from ignorance on the part of the orator, who is well known for the extensive range of his medical acquirements ; and we are left to conjecture why in a lecture on therapeutics, one of the most important systems of therapeutics, indeed, the most important that has appeared since the days of Hippocrates, was altogether ignored. We shall not attempt to guess the reasons, but leave it to our readers to do this at their leisure. One negative compliment to our system we may extract from this slight to it, and that is, if the orator had been able to say anything to the discredit of homœopathy we may be sure he would not have failed to do so.
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Homœopathic Pharmaceutical Society of Great Britain.

ON the 20th ult. the annual meeting of the above Society was held at its temporary rooms, 445, Strand, when the annual report was presented and adopted.

The officers for the past year then retired, and a vote of thanks acknowledging the efficient manner in which the business of the Society has been conducted during the past year, and conveying the best thanks of members, recorded.

Hitherto the executive has been entirely composed of London members. Opinions having been expressed that country members would be more likely to interest themselves more zealously in the welfare of the Society if they were eligible for office, a canvass was made, and four expressed their willingness to serve.

The voting arrangements having recently undergone thorough revision to enable members at a distance to have a voice in the management of the Society, the election of the executive board was this year for the first time accomplished through post with the following results:—*President*—Mr. J. C. Pottage, Edinburgh ; *Secretary*—Mr. G. Cheverton, Tunbridge ; *Treasurer*—Mr. J. Thompson, Liverpool.

In future the meetings will be held quarterly, on the third Tuesdays in October, January, March, and June.

The Society begs to acknowledge with best thanks a contribution of microscopic slides received from Dr. Mackechnie, amongst which are some subjects possessing great interest to the homœopathic pharmacist.

BOOKS RECEIVED.

The Nelubium Luteum, or Great American Water Lily ; its Value as an Ornament to the Public Parks of Chicago, by E. M. HALE, M.D. Chicago, 1871.

Vaccination viewed as a Sanitary Measure, &c., by J. HANDS, M.R.C.S., &c. 1871.

The Past, Present, and Future of Homœopathy, by W. D. HUMPHREY, Analytical Chemist.

On Chronic Diseases of the Organs of Respiration ; being a series of Clinical Observations on Diseases of the Air-passages and the Lungs, by JOHN MEYHOFFER, M.D., &c. &c. Vol. I. London : H. Turner and Co.

The Dublin Quarterly Journal of Medical Science.

Buffalo Express. Jan. 2, 1871.

Australian Homœopathic Progress.

The Monthly Homœopathic Review.

The Hahnemannian Monthly.

The American Homœopathic Observer.

The Western Homœopathic Observer.

The Chicago Medical Investigator.

The North American Journal of Homœopathy.

The Western Homœopathic Observer.

The New England Medical Gazette.

El Criterio Medico.

La Reforma Medica.

La Homœopatía.

The Calcutta Journal of Medicine.

La Revista Omeopática.

The Food Journal.

The Chemist and Druggist.

THE
BRITISH JOURNAL
OF
HOMŒOPATHY.

FORCE, PROTOPLASM, AND STIMULUS.

By Dr. DRYSDALE.

(*Concluded from p. 549.*)

CHAP. IV.—THE ANATOMICAL SEAT OF THE LIVING
MATTER (*concluded*).

§ 81. The death of Fletcher seemed to occur at the close of a period in the history of physiology, for he was one of the last writers of a consistent system of physiology before the recent great advance in microscopic histology took place, and before the discovery of the conservation of force doctrine introduced a most important element into all physiological investigations. For about ten years after his death there was a dreary period, in which the unfortunate revival of humoralist theories by the Vienna school of pathology occupied men's minds, and was most unfavorable for the fair hearing of a purely solidist system. It is well said by Virchow (*Cellular Pathology*, p. 2) that every important epoch in medicine has been ushered in by important discoveries concerning the structure of the body. With Fletcher, we have seen, the capillary or parenchymatous tissue was the last word of anatomy ; beyond that we could

not go for a material seat of the true vital actions of growth and nutrition. But even while he wrote Schleiden had begun to make those observations which were afterwards followed up and developed by Schwann into what is known as the cellular theory. It is not my intention to go into the history of this large subject farther than to allude to the cardinal point in which it touches our subject, which is that a complete revulsion from humoralism again took place. Besides that, the doctrine of a central overruling vital principle received a shock, and that of the inherent and special vitality of certain anatomical elements in all tissues and parts became recognised. This is, in fact, nothing but the specific irritability of Fletcher, and now his other doctrines, one would have hoped, might have had a prospect of a fuller hearing; but the time was not fully come yet. For by Schwann and his followers the cell as a whole, and more particularly the cell-wall, was looked upon as taking an active part, or being, in fact, the seat of the vital process and former of tissues.* If this be allowed, we are not advanced beyond the stage where Fletcher left us, for the cell wall is, in many cases, of firm texture and chemically different from the soft cell contents. It likewise passes insensibly often into the intercellular substance of which the tissue is said to consist. It is, therefore, open to all the difficulties enumerated at § 80 against the vital or metabolic action at a distance supposed to be exerted by anything, and more especially by a rigid structure. It likewise is incompatible with the hypothesis of vitality being seated in "one homogeneous, pulpy matter, yielding fibrin," for it differs in every

* "So far from being the centre of activity of the vital actions, it [the endoplast—central portion] would appear much rather to be the *less important histological element*. The *periplast*, on the other hand, which has hitherto passed under the name of cell wall, contents, and intercellular substance, is the subject of all the most important metamorphic processes in the animal and plant. By its differentiation every variety of tissue is produced; and this differentiation is the result, not of any metabolic action of the endoplast, which has frequently disappeared before the metamorphosis begins, but the intimate molecular changes in its substance which take place under the guidance of the 'vis essentialis.'" (Huxley, *Brit. and Foreign Rev.*, Oct., 1853.) This may be taken as a correct statement of the opinions then prevailing, but they are quite erroneous, and are abandoned by Huxley himself now.

conceivable way in kind, texture, and composition in all animals and plants, and in different parts of the same organised being. However, even before the cellular theory was complete its foundations were sapped, and now we may already look on it as a theory of the past, at least in a physiological sense, for the cell still takes its rank as the anatomical elementary part, and represents to a great extent the substance and function given by the older anatomy to the capillaries.

In 1835 a living moving substance was discovered by Dujardin in the lower animals, which he called *sarcode*, but for long this was looked on as something different from the cell. Afterwards a similar substance was found by Von Mohl in vegetable cells, and named *protoplasma*. The attention of a long list of eminent physiologists has been directed towards these substances, and finally, chiefly through Cohn, Leydig, Max Schultze, Kühne, Brücke, Häckel, and others, they were shown to be of the same nature, and gradually they have assumed a position of supreme importance, and, in fact, seem to take the place of the cell altogether. With the German school that is not allowed as yet, and they still cling to the idea of the cell, although they admit the existence of naked masses of protoplasm, apparently having separate existence and vital powers formerly attributed to cells.

§ 82. From what is said in the last paragraph it is evident that the dawn of the day was approaching which might realise the hypothesis set forth in the preceding pages. And so it has proved, for in 1861 Dr. Lionel S. Beale delivered before the Royal College of Physicians, and afterwards published, in the same year, in his *Archives*, a course of lectures "On the Structure of the Simple Tissues of the Human Body," wherein was proclaimed a discovery which is destined to have an influence on physiology which cannot yet be appreciated, viz. that of the true anatomical nature of the living matter, and the sharp line which divides the living from the dead in all organised beings. Such a discovery is surely, in intellectual merit, equal to, if not greater than, that of the circulation of the blood, although

its effect on the progress of science cannot be expected to be comparable to that, for the times are very different and no one can now found anew the science of experimental physiology, and hardly even a new method in its special departments.

In a field where there must be so many labourers, as in physiology at this time, the steps which lead up to the discovery of any great principle must be the work of many hands, but the claim of Dr. Beale, as the first who perceived the unity and full significance of the protoplasm, can hardly, I think, be disputed, and his name will stand forward in future times as the pre-eminent one, while most of those who are now mentioned as on a level with, or even above him, have faded into oblivion. In the discovery of the chief facts on which the doctrine rests, priority must also be given to Beale, for it was only after years of patient research and experiment—during which he discovered new methods of histological analysis, enabling him to surpass the microscopic preparations of others as far as Ruysch did the injections of his contemporaries—that he accumulated sufficient evidence for the establishment of the doctrine. He says in the preface to the first publication of the lectures in 1861, “he thinks it right to state that the conclusions which have now assumed a definite form have gradually grown upon him during the course of observations extending over a period of several years. In fact, some of the drawings in this volume, and others which have been published elsewhere, equally favorable to this view, were made long before any specific theory had been arrived at.”

All that has been done since can merely confirm or refute the doctrine, but can have no share in its discovery. It is therefore a matter of satisfaction that the man of genius to whom it is due should be one of our countrymen, and that he has again gained for England the palm of physiological discovery. The German school (in the race with whom we are as yet so heavily weighted from the want of state aid to physiology) are, in truth, hardly prepared to dispute it, as they do not accept the doctrine in its full significance and are still occupied with contests over the details of the cell

theory,* and are also for the most part too much trammelled with chemicalist hypotheses to take much interest in the fixing of a sharp boundary line between the living and the dead. But the time will ere long come when pure vitalism will regain its ascendancy, and the full significance of the principle will be generally recognised.

* In 1861 Max Schultze wrote a paper in *Reichert und Dubois' Archiv*, in which he showed that the cellular theory, as then held, required great modifications, and that the cell contents, especially the parts corresponding to Von Mohl's protoplasm, was of higher importance, not only for the life of the cell itself, but for the building up of tissues. In his work *Das Protoplasma der Rhizopoden und der Pflanzenzellen*, 1863, he recapitulates some of the positions of the above paper, and enforces the necessity of those modifications by saying that "the necessity for such improvements is also felt in those quarters where the cellular theory has not yet been understood; this applies to the treatise of Beale (*Lectures* of 1861), which will not receive the attention which it, in many respects, deserves, because it stands outside of the cellular theory. Beale's germinal matter, certainly, is essentially that which we call protoplasm comprehending the nucleus, and the formed material is brought into what is essentially a true dependence on the protoplasm. But of cells as elementary parts or elementary organisms, and of nuclei as different from the protoplasm, and still so necessary to it, there is no word. For him (Beale) the great and inalienable [unveräußerliche] discovery of the cell is of no more than historical interest" (p. 8). Max Schultze then goes into the details of the subject, and in the great majority of points comes to conclusions almost identical with Beale's. In these, also, Brücke and Hæckel, for the most part, agree, while Reichert takes the opposite side, and defends the older views of the cell. Schultze, however, never gives up the importance and the vitality of other parts of the cell when present, and concludes with the asseveration, "I must again expressly declare that Reichert's fears are completely groundless, that by my conception of the cell the bases (Grundvesten) of the cell theory are shaken. No one can be more deeply penetrated than I am with the conviction that the doctrine of the cell, as the fundamental element of all animal tissues, is, for all time, inalienably assured" (p. 68). This, be it observed, is written in 1868, and when commenting on Beale's lectures. Now, Max Schultze and Brücke are, of all observers, the nearest to Beale in their views of protoplasm; and if they repudiate both Beale's facts and his theory, it is obvious that henceforward no one else can dispute with Beale the claim to the discovery, if it be one, that the sole living part of all organized beings at all stages of their life, from the polype to man and from the fungus to the oak, is that "clear, structureless, formless stuff" now called protoplasm. I claim no share in the actual discovery for Fletcher, but I think it is a high merit to have demonstrated, a quarter of a century before, that some such substance was to be discovered; and I hope it will serve to attract towards his complete system of physiology, pathology and therapeutics, the attention it deserves from professed physiologists.

At present the vitalist school even are hardly prepared to accept it, and many of those who receive as true Beale's doctrine of nutrition and secretion in a general way cannot bring themselves to believe that the greater part in bulk of all tissues, even the muscular and nervous, hitherto deemed *par excellence* the very seat of life, are actually dead, as dead as hairs, nails, and cuticle, all our lives long.

§ 82*a*. The word protoplasm has of late got rather into disrepute through Mr. Huxley's popular lecture on the "Physical Basis of Life" at Edinburgh, reported in the *Fortnightly Review*, vol. v (1869). The doctrines there put forth need not have excited displeasure, for, with the exception of a certain levity in the mode of statement, they are substantially the same as those taught by Fletcher up to 1836. The question put by Huxley, "Is the matter of life composed of ordinary matter, differing from it only in the manner in which its atoms are aggregated?" (p. 136), is somewhat more precisely stated and answered in the affirmative by Fletcher. But Huxley does not see so strongly the immense distinction between the vital and chemical state of aggregation, and thus falls into the inconsistency of speaking of "ready made" protoplasm, and even protoplasm of such a proverbially dead substance as mutton, being "dissolved" and "transubstantiated" into his own. He speaks likewise of protoplasm as an albumenoid. With such chemical views he is obliged, like the rest of the mere chemist school, to fall back upon "subtle influences" to aid mere solution in converting mutton into living matter. In fact, his theory is to a certain extent a reversion to the hypothesis of organic molecules which were supposed to build up the different organised beings by their perpetual transmigrations from one to another through the aliment. This hypothesis was judged and pronounced untenable by Fletcher (p. 154). If the author of the tract *As Regards Protoplasm* wishes properly to criticise the doctrine of a Physical Basis of Life, he should go to the fountain-head and read Fletcher's *Rudiments of Physiology*. In the meantime, however, Dr. Beale, who was then giving his Oxford lectures, pauses in the middle, and takes Huxley to task for his strange views in applying the term protoplasm to dead lobster, mutton, bathybius, albumen, and other things. The word protoplasm was invented by Von Mohl to designate a certain part of the constituents of vegetable cells, and when adopted by others it was applied by them to a great variety of substances, in accordance very much with their theories of correspondence in vital function with the original matter. The consequence has been that it has come to mean with different authors, in addition to its original signification, the cell-wall, other cell contents, the whole cell, mucus, various granular and fibrillar structures, the contractile matter *par excellence*, intercellular substance, nerve-fibre, nerve-

cell-walls, bathybius, roast mutton, bread, and many more things. For these reasons Beale objects to its retention as the designation of the living matter, and since for several reasons his own original term germinal matter is inconvenient and imperfect, as applying to one faculty of the living matter alone, he proposes a new name instead of restricting the word protoplasm to one accurately defined meaning:

"The name I propose to give to the living or germinal self-increasing matter of living beings, and to restrict to this, is Bioplasm. Now that the word Biology has come into common use, it seems desirable to employ the same root in designating the matter which it is the main purpose of biology to investigate. Bioplasm involves no theory as regards the nature or the origin of the matter; it simply distinguishes it as living. A living white blood-corpuscle is a mass of bioplasm, or it might be termed bioplast. A very minute particle is a bioplast, and we may speak of living matter as bioplasmic substances." (*Microscopic Quarterly*, July, 1870.)

If the name bioplasm had been given when the thing was discovered probably no one would have objected to it, but it is different now; and when Beale's germinal matter is, in the great majority of cases, exactly the same object which has long been described as protoplasm by many naturalists, the scientific world may be more willing to accept from him a more precise restriction of the term than a new one altogether. We cannot tell yet how this will be, and although Dr. Beale's wish will have, of course, more weight than that of any single individual, it may not prevail. There are many inconveniences attending the continual introduction of new names. Of this Fletcher was particularly careful and refused to invent any new name, although he was far from being satisfied with irritability. That word had been used in a great variety of senses since the time of its inventor, Glisson, quite as many and as little consistent as protoplasm, and Fletcher's meaning corresponded exactly with none of them, yet he refused to invent another word, but proposed simply to define it more strictly, for these reasons:

"There seems to be no advantage, however, but, on the contrary, very great inconvenience in the endless invention of new terms to which modern philosophers have been in general so addicted, and by which many who are incapable of distinguishing themselves in any other way have sought distinction. Many better and less equivocal terms than irritability might unquestionably have been originally invented to express the faculty under consideration; but no man has a right in the present day to expect that *his* new nomenclature, however abstractedly excellent, will entirely supersede the employment of a term already in general use; and if this be not the case, the onomatomaniac, without obviating the necessity of our understanding old terms, only imposes upon us that of learning new ones likewise. Names in general are—

“ ‘Not bad *simplior*, nor good,
But only as they’re understood,’

and it seems quite sufficient to establish any given name as the best, that it is generally employed, and by all who employ it once clearly defined.” (p. 51.)

Fletcher was probably alluding to Mason Good, and such like literary interlopers in physiology, but the common-sense rules to which he submitted himself must be generally applicable. Besides, it does not appear that bioplasm is abstractedly more suitable, for it may mean “that which is formed by or through life,” an appellation that would apply equally to the dead “formed material” as to living matter. According to Richerand, “Anatomy is the science of organism,” while “Physiology is the science of life;” so for a name describing the anatomical seat of living matter protoplasm would be better, as it means “that which is first formed;” and as it is impossible to give a complete epitome of attributes of a thing in its name, it would be better to embody the cardinal point in Beale’s own theory, viz. that no pabulum can take part in living action till first converted into this substance. If the word life is to be brought into the name it might be bio-protoplasm, which would be a clumsy word.

An excellent classical scholar, Mr. Scott, of Birmingham, to whom I submitted the question, gives this opinion:—“Bioplasm is very vague. If I heard it for the first time without knowing anything of the connection in which it occurs I should take it to mean an image of life. It might be used to mean ‘anything formed or moulded by life;’ but there is nothing *in the word itself* to indicate that the thing formed is also itself living.”

On the whole, it is most probable that the term protoplasm will still be retained in this country to express Beale’s germinal matter, as it is so accepted by Dr. Sharpey, whose calm and solid judgment reveals to us, as it were, the verdict of posterity. I will therefore continue to consider as synonymous terms “living matter,” “irritable matter,” “diffused ganglionic nervous matter,” “germinal matter,” “bioplasm,” and “protoplasm,” but will use chiefly the last and “living matter.”

§ 83. As the results of those years of independent observation, and resting on them alone, Beale came to the conclusion that life or vital power is not generally diffused over different tissues or organs, but is restricted to one kind of matter alone in all living things, both animal and vegetable, from the highest to the lowest. It is in this substance, which he terms *germinal matter*, alone that “take place all the changes which more especially distinguish living structures from lifeless matter. The particles of which this is composed, after passing through certain definite

stages of existence, undergo conversion into the peculiar substance or substances they were destined to produce. It is the germinal matter alone which is capable of *forming*, producing, and converting. The matter external to it (cell-wall, intercellular substance, or fluid) has been *formed* or produced, and it may be changed, but it has no power to *produce* structure or to alter itself." (*Arch.*, III, p. 116.)

By the use of these terms, "germinal matter and formed material," he designates the two radically and irreconcilably distinct states of matter of which in various proportions all living things are composed. By means of this clear distinction he brings order into the chaos still existing in the cell theory, and shows that the sole active matter at one time corresponds with the nucleus, at another to both nucleus and cell contents, again to the matter lying between the cell-wall and certain of the cell contents; while, again, the formed material in some cases corresponds exactly to the cell-wall; in others, to cell-wall and part of the cell contents; in others, to the intercellular substance, and all the tissues and fluid secretions, and, in fact, to every part of living beings formed by vital action. To us the essential point is, not to reconcile the various theories of the cell structures and contents; but what is the anatomical nature of this one true and only living matter?

"Germinal or living matter is always transparent, colourless, and, as far as can be ascertained by examination with the highest powers, perfectly structureless, and it exhibits these same characters at every period of its existence * (36). However much organisms and tissues in their fully formed state may vary as regards the character, properties, and composition of the formed material, all were first in the condition of clear, transparent, structureless, formless, living matter" (p. 46).

"There is a period in the development of every tissue and every living thing when there are actually no structural peculiarities whatever; when the whole organism consists of transparent, structureless, semi-fluid, living germinal matter; when it would not be possible to distinguish the growing, moving matter which was to evolve the oak from that which was the germ of a vertebrate animal. Nor can any difference be discerned between the germinal matter of the lowest, simplest, epithelial scale of man's organism, and that from which the nerve-cells of his brain are to be evolved." (*Oxford Lectures*, p. 608.)

* Beale, *Protoplasm*, 1870.

Beale's germinal matter is identical with the protoplasm of the German school, as found in the amœbæ, white corpuscles, pus-corpuscles, &c.; so the description may be supplemented by the statement that it coagulates between 35° and 50° Cent., contracts under electricity, stiffens after death, and is then found to contain myosin and other nitrogenous proximate principles.

Here, then, we have the exact counterpart of Fletcher's irritable or diffused ganglionic nervous matter brought forward as the result of independent research by pure observation and experiment. This is surely a very remarkable fulfilment of the previsions of deductive reasoning!

It is quite unnecessary to go into the full consideration of Beale's wonderful discovery, as his theory of nutrition must be known to every one, so I will merely touch upon a few of the points which show correspondence with Fletcher's theory or clear up its difficulties.

With respect to the comparative bulk of the protoplasm and the lifeless tissues, there is no fear that we cannot account for the apparent inherent vitality of the smallest parts accessible to our senses, for Beale says, "There is not one portion of a living thing much more than $\frac{1}{500}$ th of an inch in diameter in which living matter cannot be demonstrated." (*Medical Times*, April, 1866.)

With respect to the specific irritability inherent in each portion of living matter—

"For the characters and composition of the living matter do not enable us to premise anything whatever concerning its formative properties. In the formation of man and the higher vertebrata the primary mass of bioplasm or living matter absorbs nutriment and grows, and then divides and subdivides into numerous masses, which are arranged in a definite manner, but what determines this is not known. From each of these in preordained order, and with perfect regularity more are produced, no doubt, according to 'laws,' but laws which we know nothing about, except that they are not physical. As this process of division goes on the resulting masses produce various substances, some having wonderful structure and properties. But the power of each series to produce these peculiar materials which did not exist before, and which cannot be extracted from the food supplied, differs from that of the series which preceded it, and so on, until the complex structural basis of the organism is, as it were, laid

down. There are masses of bioplasm to form nerve, others to produce muscle, others glands, and so on, all of which have been derived from one common mass; but the bioplasm destined to take part in the development of a gland will, under no circumstances, produce muscle or nerve. And yet with all this marvellous difference in power, which seems to be somehow acquired as development advances, there is, as far as is known, no difference in matter. The nerve- or muscle-producing bioplasm is, as far as can be ascertained, the exact counterpart of the gland- or bone-forming bioplasm, and why one produces one tissue and the other a very different tissue cannot be explained."

Germinal matter of bone transplanted.—"When periosteum is transplanted, as was done by M. Ollier, of Lyons, to various parts of the organism, the production of the bony tissue formed in the new situation is due to the growth and development of the masses of germinal matter which exist in such great number at the deep surface of this fibrous membrane. These grow and multiply, and produce formed material, just as if they had remained in the original seat of their development—a striking proof that the *kind* of tissue formed by germinal matter depends upon its *powers* rather than position or the conditions to which it is exposed. In certain forms of bone cancer very minute portions of actively growing germinal matter are sometimes carried to the lungs, and grow and multiply, and give rise to bone cancer in the pulmonary tissue, showing that the germinal matter possesses the peculiar property or power of giving rise to this particular tissue if supplied with pabulum." (*Oxford Lectures*, p. 112.)

He illustrates the consumption of protoplasm and constant renewal of it from the pabulum as follows. In the formation of the muscular fibre the mass of protoplasm moves gradually along, undergoing at the same time conversion into muscular fibre. "At the same time that it advances it absorbs nutrient matter, so that much formed material or contractile tissue may be produced without the germinal matter altering in size." (*Arch.*, IV, p. 153.)

In the secreting cells "the germinal matter remains near the lower attached extremity of the open-mouthed cylindrical cell, and takes up nutrient matter at its lower surface, while at its upper part, which forms the floor of the cell cavity, the germinal matter is changed into the *secretion* of the cell which occupies its cavity and escapes from the open orifice. A large quantity of pabulum may pass into the state of germinal matter, and a corresponding quantity of the latter undergo conversion into the formed material or

secretion of the cell, while the entire apparatus neither changes in volume nor alters in form or weight." (*Oxford Lectures*, p. 665.)

It is superfluous to illustrate this point further from Beale, as, according to him, it is invariable in all formations of tissue and secretion. But I may cite an instance from another source where it was also observed in the functional action of sensiferous nervous tissue. Liebreich made the horrible experiment on dogs of torturing them till they died from prolonged and excessive pain. On analysis it was found that the side of the spinal marrow corresponding to the roots of the nerves which had been excited contained always much less protagon than the opposite side. (Gavarret, p. 241.)

As to the relation of quantity of pabulum to growth of protoplasm, it is unnecessary to quote, as it is almost everywhere insisted on by Beale to be almost in the direct ratio. On this point he goes much further than Fletcher, and accounts for the operation of mechanical irritants in this way; so this subject had better be left for question of the stimuli.

We may now notice the most characteristic part of Beale's system, viz. the link between the physio-chemical and the vital processes, which go on perpetually side by side in all living beings, and yet are always essentially distinct. All pabulum is described as taken into the living state, wherein its elements are held together in a state of combination entirely different from ordinary chemical combination. When growth of living matter goes on this takes place by increase and subdivision from within, and the formation of new centres of germinal matter. But when the tissues or secretions are to be formed, the elements are rearranged into the new chemical compounds and structures constituting the formed material. They have then passed again out of the living state, and he constantly uses the expression that the protoplasm dies into the particular tissue or secretion it has the specific power to produce.

For example, after describing the growing, dividing, and

multiplying of masses of protoplasm, each of which is always derived from pre-existing masses of protoplasm, he says :

“After a time some of these cease to multiply, though they still live and take up food. The living matter of which they are composed undergoes change. It dies under certain conditions, and tissue results. In this way muscle, and nerve, and fibrous tissue, and bone, and hair, and horn, and nail, and all the other tissues, are formed.” (*Micros. Journ.*, July, 1870.) Or, again—“The formation of the fatty matter occurs in this way,—in the very substance of the bioplasm, but always outside and away from the new centre or nucleus, a little oil-globule makes its appearance. It results from changes in the living matter itself. A portion of this bioplasm dies, and among the substances resulting from its death are fatty matter, which, being insoluble, remains, and soluble substances, which are carried away in the blood. Starch-globules, and other secondary deposits, formed in the interior of elementary parts, are produced in the same manner by the death of the bioplasm. The fatty matter does not come from the blood as fat and deposit itself in the cell, nor is it formed by the collection and aggregation of excessively minute granules which traverse the vascular walls suspended in serum, nor is it precipitated from the nutrient fluid after the manner of crystals. But it invariably results from the *transformation of living matter*, and different kinds of living matter, as is well known, will produce different kinds of fat. The properties and composition of fat in different animals differ, because the powers of the bioplasm or living matter of each animal are so different.” (Beale's *Todd and Bowman*, p. 301.)

Thus we have a very simple conception of a process which relieves us from the dilemma of Fletcher. And when we come to think of it in this light we see that it could not be otherwise. Since vitality is the result of a combination of matter held together by a peculiar affinity, which is a property, and therefore incapable of being transferred, the only way to act upon fresh matter is to take it into that state of combination—to make it a part of the compound. Any change, therein produced, by which a portion is restored to ordinary chemical combination is, in fact, death just as much as if that happened to the whole mass as in ordinary complete death.

Read by the light of Beale's theory, Fletcher's chapter on death assumes a peculiar significance. Fletcher represents death as a vital process, the last of the living actions, whereby the elements are rearranged into the chemical

state; for those elements being hitherto in a totally different state, the usual chemical affinities cannot take effect on them till they are released from vital affinity by vital agency. He says of the living tissues, "their vitality cannot desert them otherwise than by a vital process" (p. 144).

But although Beale's theory removes the absolute incompatibility of two such processes as the living and the dead working together, yet it is, of course, far from being a complete explanation of the phenomena of nutrition, and many difficulties are still felt and objections raised against it. Professor Wyville Thomson thus speaks:

"It is impossible in the present state of knowledge to subject any view as to the ultimate mechanism of the formation of tissue through the means of protoplasm to direct proof. It seems now to be a very generally received opinion, supported by Huxley, Max Schultze, Hofmeister, Beale, and many others, and notably by Oscar Schmidt, who would seem to bring it almost to demonstration in his beautiful researches on the sponges of the Adriatic, that protoplasm is simply converted, with a certain change of composition, into tissue or 'formed material.' There are, however, almost insuperable objections to this view. The secondary products of organisation (formed material) are most various in their chemical constitutions, and it involves the admission that protoplasm may change in its chemical composition till it is *almost* carbonate of lime, or silica, or starch, or horn, or cellulose; the last stage of the metamorphosis being its absolute separation as one or other of these bodies. Another view which I have always regarded as more probable is that protoplasm, the substance which is endowed with the peculiar vital property, has always the same composition, and that it acts simply by catalysis, inducing, under certain known laws, decomposition and recombination in compounds which are subjected to its influence, without itself undergoing any change, absorbing the nascent products of combination and decomposition, and recombining them and reserving them with reference to the development or maintenance of the organ to which it gives its life." (*Nature*, May, 1871).

In the presumption that the placing of Beale's name on a level with the others is not an intentional denial of priority of the theory, no remark need be made on the position it here occupies. Nor was it necessary to adduce such a recondite example of the difficulty of the theory, as it is illustrated every instant by what goes on in ourselves and all the higher animals as well as the lower.

Beale is, in fact, here in the same difficulty as Fletcher, for he does not allow that the vital power can be taken on by any albumenoid, nor by any so-called proximate principle at all, but that before such a power can be manifested the material must be changed into a state of combination entirely *sui generis*, the passage out of which back into any ordinary chemically cognisable compound means death. But all protoplasm or living matter is nitrogenous, thus containing the four organic elements at least, besides some one or more of the fifteen other so-called inorganic elements found in living beings. How, then, can it die into a binary or ternary compound like carbonic acid or the fats or sugars, *i. e.* into substances whose elements, arrange them as you will, never could possess vitality? How are these extruded from the protoplasm while the nitrogen remains? Again, there are certain products which are synthetic. Thus, the hæmoglobin, protagon, and the probable generator of myosin, are of higher potential energy than the pabulum, and possibly also than the protoplasm; also the formation of fats from saccharine pabulum must be a reducing process; in both these processes, therefore, a certain quantum of force must be consumed and rendered latent. Whence is this derived? We have no proof that animals can consume the active forces of heat or light for this purpose, so probably there must be some simultaneous compensatory process of decomposition of a portion of pabulum in the living acts of nutrition and secretion which has not yet been followed out experimentally. Whether any such compensatory processes will explain both difficulties must for the present remain unknown.

Beale at first (*Archives*, 1861) thought that the transition from the living matter to the formed material was a gradual one; that "if the term life and vital activity be used, several degrees must be admitted. One might say, therefore, that the vital activity of the particles gradually becomes reduced as they recede from the centre at which they became animated" (III, p. 119). But since then he has abandoned this opinion for one more consistent with his own and

Fletcher's view of the constitution of living matter, viz. the following :

"The pabulum does not shade by imperceptible gradations into the living matter, and this latter into the formed material; but the transition from one state into the other is sudden and abrupt, although there may be much living matter mixed with a little lifeless matter, or *vice versâ*. The ultimate particles of matter pass from the lifeless into the living state and from the latter into the dead state suddenly. Matter cannot be said to *half live* or *half die*. It is either *dead* or *living*, *animate* or *inanimate*, and formed matter has ceased to live." (*Protoplasm*, p. 34.)

Beale also supposes that secondary changes may take place in the formed material which may first be deposited in a more complex state of composition and then split up or be oxidated into different organic compounds (*Oxford Lectures*, 1869, p. 57). Likewise in the formation of bone he believes that the protoplasm produces the matrix, either fibrous or cartilaginous, in the same way as all other formed material, and that afterwards the earthy parts are precipitated from the nutrient fluids by a purely chemical process (*Todd and Bowman*, p. 251).

It must, however, be acknowledged that certain difficulties remain which cannot be yet cleared up. But that is no reason for rejecting an hypothesis otherwise so well grounded, and falling back into the hopeless muddle of the chemicalist school, who think to explain everything by the all-embracing word catalysis (§ 77). I think Beale may find comfort in the answer of Copernicus, who, when the objection to his theory was made that the planet Venus had no phases like the moon, replied, "Truly I do not know how this may be, but I am sure that God in his own good time will give us the means of understanding it!" Ere long the telescope of Galileo showed that the anomaly was only apparent, and dissipated the difficulty.

In the retrograde metamorphosis the protoplasm plays a similar, but now a reversed part, consuming the formed material and reconvertng it into blood or into excrementitious matter. Here, however, Beale believes that oxidation and other chemical processes have a share, although that cannot be great or almost ever exclusive, for he says :

“In every change characteristic of living beings this living matter takes a part. In the formation of every tissue, in its disintegration, in its repair, living matter is concerned. Nor does the slightest marked change take place without the phenomena occurring in the living matter of the part being modified. No formation of structure, no action occurring in the structure after it has been produced, no secretion, can be accounted for without considering what goes on in the living matter.” (*Med. Times*, vol. i, 1866, p. 411.)

The last point of correspondence we may notice is that of deterioration of the living matter. After describing the specific differences of the various kinds of protoplasm which produce the different organs and tissues of the higher animals, he goes on to explain that developmental and formative power may be impaired or lost from various causes, mostly unknown. Then he says that it is a mistake to suppose that the germinative or developmental faculty is confined to embryonic life, but that there is no essential difference between the formative and nutritive operations in the embryonic and fully formed state (*Med. Times*, March, 1866). In other words, that the plastic germinal and metabolic faculties must always be present as long as life exists, but they may be deteriorated or degraded in different degrees, and more especially the germinal faculty may be degraded while the mere power of growth and increase may be even augmented, and this is a feature of many states of disease. He says:

“But, although developmental power may be lost for ever, power of a different kind may be acquired *pari passu* during the rapid multiplication of bioplasm. Progressive advance in the capacity to form lasting structures and elaborate organs is characterised by the comparatively slow but regular and orderly growth and multiplication of bioplasm. Rapid multiplication of the bioplasm, on the other hand, involves degradation in formative power, which is at length entirely lost, never to be reacquired. Degradation in power is commonly associated with increased rate of growth, increased faculty of resisting adverse conditions, and, in some cases, such is the vitality of the living matter, it takes up the nourishment which should be appropriated by healthy parts, and these are at length starved and deteriorate, or are completely destroyed. The actively living degraded bioplasm may be capable of retaining its vitality although removed altogether and for some time from the living body, and, remarkable as it seems,

it may grow and at length destroy other living organisms to which it gains access." (*Microscopic Quarterly*, July, 1870.)

Again, "Increase of formative and constructive power seems to be associated with the most limited change in germinal matter, while rapid change—increased vital action—seems to be invariably connected with decadence of power." (*Protoplasm*, p. 85.)

This corresponds with Fletcher's placing disease in the secondary or lowered stage of vital action as a whole.

These principles are of extreme importance, but their bearing is so very wide that it would be necessary to discuss a large part of pathology to follow them out. On the whole, a great degree of harmony could be shown between his views and discoveries, as far as they have gone, and the principles laid down by Fletcher, but the subject can be better discussed when the stimuli have been considered. We miss, however, the reference to stimuli, the necessity of adequate exciting causes for all aberrations from health, and the therapeutic deductions, which are such prominent features in Fletcher's system. On the other hand, Beale makes a most important addition to our knowledge in the reference of the contagious principles of disease to portions of degraded, but still living, protoplasm. This, if confirmed, will prove a discovery of practical interest that can hardly yet be estimated at its proper value, and will supersede animal and vegetable germ theories of disease which have of late obtained a kind of notoriety.

This theory of the living, and yet non-parasitical, nature of the contagious principles is an advance upon Fletcher, who looked upon them as diseased secretions, acting solely as preternatural stimuli; but if the self-multiplication of degraded protoplasm is to be admitted, it does not thereby exclude the necessary co-operation of the action of miasms as morbid stimuli. Else how are we to explain the long latent stage and sudden development of the active symptoms of contagious fevers?

Likewise Beale's discoveries in respect to the true nature of suppuration have anticipated the observations of Cohnheim and Stricker, and are, in all probability, a more correct interpretation of the phenomena.

Beale's paper in the *Microscopic Quarterly* for July, 1870,

and also his report on the cattle plague in the *Government Blue Book*, are of the greatest interest, and should be studied by every medical man.

§ 84. And now, after having gone so far, as it were hand in hand, I regret to find that Beale at last parts company with Fletcher. Although the former has all along insisted on the actual molecular change which matter undergoes on entering the living state, and the absence of any of the proximate principles therein, yet, to my astonishment, he after all holds that change to be insufficient to account for the new properties then acquired, and accordingly falls back upon the old vital principle. He not only rejects the physical, as contrasted with the teleological view, but rejects also vitalism in favour of animism. It is true he objects to being classed with believers in the old vital principle, but as his "vital power" is declared to be not a property of matter, nor a force, nor the result of any action, nor a phenomenon, it must be an entity, and if not the same as that of Barthez, it merely adds one more to the numerous varieties of the supposed vital principle. As expositor of Fletcher I feel compelled to vindicate his vitalism from the charge of inconsistency with the rules of sound philosophy, which certainly lies against the animism of Beale. Although, or rather because, the state of aggregation of matter and the properties thereby developed are utterly distinct in the organised state from the chemical state, Fletcher believes life to consist solely in the actions which are the necessary result of those properties. That life therefore corresponds to the *Zωή* of the Greeks, or *Vita* of the Romans, which meant living action, and not to the *Ψυχή*, or *Anima*, which designated life as a substantial entity, which, entering into certain compounds of matter, organised them, and continued to be the efficient cause of all their characteristic phenomena. The vitalism of Fletcher is, therefore, in harmony with the physical theory of life in the abstract, and with the canon of Bacon's philosophy which emancipates science from the consideration of final causes. But it is completely opposed to numerous *false* physical theories of life, such as John Hunter's galvanic or Lamarck's com-

pound of electricity and light, theories ; and to the not less absurd theory at present prevailing, that life is nothing but certain actions produced in albumen by a force or forces correlative with heat and motion. Fletcher's chapter on the nature of life is not only the most complete and exhaustive *resumé* of the arguments against the theory of a substantial vital principle as the efficient cause of the infinitely varied phenomena of life, but it is likewise equally opposed to the admission of any teleological cause of these. They must be referred, in fact, equally with all physical phenomena, to the properties of (organised) matter acted on by corresponding powers, and therefore not to one self-sufficient substance or cause, " but to countless irrational and unconscious forces incessantly at work in every point of the system, in blind but implicit obedience to laws imposed upon them by the Supreme Being, and adapted everywhere to the end to be fulfilled " (35). And in answer to the then and still widely held opinion that the conspicuous evidence of design shown in living beings surely points to the existence of a special power acting in them with a definite purpose, he says, undoubtedly there is design, deep and wonderful, " but this design is that of the Great First Cause of all things, who has adapted in every case the physical causes—the immediate means—to the end to be fulfilled ; not that of this miserable means, which acts, and can act, only in blind obedience to the laws imposed upon it. Nor, perhaps, are the evidences of design really greater in the motions, sensible or insensible, of the organic than of the inorganic kingdom of nature, nor is the employment of the terms means and end—not merely cause and effect—more appropriate in the former case than in the latter. The final causes are often no less obvious in the actions of inorganised than of organised matter. Can we behold the revolutions of the planets, the alterations of the seasons, and of day and night—

Hunc solem et stellas, et decedentia certis
Tempora momentis ;

at one time the ascent of the waters into the atmosphere, at another its descent in rain or snow, the motions of the

sea, the rivers, and the springs, the occasional changes on the surface of the earth itself, without recognising an allwise and omnipotent design? Nor are the molecular actions here also—the aggregation of a crystal or stalactite for example, or the feathery condensation of vapour—less indicative of an end in view in their operation, or of consummate skill in regulating and adapting the means to this end. In the latter class of cases, however, we unhesitatingly admit that the design is not that of the means—of the mere so-called forces of attraction and repulsion—but that of the Great Author of these forces; and why should we doubt that the same is the case in the former also?” (II, p.28.)

To illustrate farther the true physical theory of life I may adduce the testimony of the distinguished founder of the cellular theory.

Three years after Fletcher, Schwann (*Cell Theory*, p. 186) reduces the various opinions entertained with respect to the fundamental powers of organised bodies into two, viz. the teleological and the physical. In the former every organ is supposed to “originate with an internal power, which models it into conformity with a predominant idea, arranging the molecules in the relation necessary for accomplishing certain purposes held forth by this idea. And here, therefore, that which arranges and combines the molecules is a power acting with a definite purpose.” “The other view is that the fundamental powers of organised bodies agree essentially with those of inorganic nature—that they work altogether blindly according to the laws of necessity and irrespective of any purpose—that they are powers which are as much established with the existence of matter as the physical powers are.” It cannot be denied that adaptation to a particular purpose is characteristic of every organism, but the source of this adaptation does not depend upon each organism being developed by the operation of its own power in obedience to that purpose, “but it originates, as in inorganic nature, in the creation of the matter with its blind powers by a rational Being.” The evidence of design is quite as great in the inorganic as in the organic world. “We know, for instance, the powers which operate

in our planetary system. They operate, like all physical powers, in accordance with blind laws of necessity, and yet is the planetary system remarkable for its adaptation to a purpose. The ground of this adaptation does not lie in the powers, but in Him who has so constituted matter with its powers that in blindly obeying its laws it produces a whole suited to fulfil an intended purpose" (p. 187). In physics all those explanations which were suggested by a teleological view of nature, as the "horror of a vacuum," have long since been discarded. But when we speak of the inflammation and suppuration which removes a foreign body as the effort of the organism to remove the foreign body, or as a proof of the "autocracy of the organism," or of the "vis medicatrix," that is as little of an explanation, according to the physical view, as it would be to say that the motion of the earth around the sun is an effort of the fundamental power of the planetary system to produce a change of seasons on the planets, or to say that the ebb and flood are the reaction of the organism of the earth upon the moon. Finally, "when speaking of a physical explanation of organic phenomena, it is not necessary to understand an explanation by known physical powers, such, for instance, as that universal refuge electricity, and the like [he would probably now say a vague use of the word Force]; but an explanation by means of powers which operate like the physical powers in accordance with strict laws of blind necessity, whether they be also to be found in inorganic nature or not" (190). This agrees completely with Fletcher's idea of the living matter being merely a compound, although its properties are entirely *sui generis*; but as if to make the argument still more complete he adds, "As the elementary materials of organic nature are not different from those of the inorganic kingdom, the source of the organic phenomena can only reside in another combination of these materials."

He does not merely mean a vague statement that in the living state the elements, or even protein-compounds, are in a somewhat modified state of combination, but, like

Fletcher, that that difference of combination is *all-sufficient* to produce the phenomena of life.

Both these authors wrote before the promulgation of the new doctrine of force, and the question now is whether that may help to reconcile the different parties by discriminating between true and false physical theories of life. At present we see Fletcher agreeing entirely with Liebig, Bence Jones, and the physio-chemical school, in the physical view, as distinguished from the animist and all teleological views, while, as a vitalist, he entirely disagrees with the particular physio-chemical theory of that school. In this last respect Beale is entirely at one with Fletcher, but refuses to admit any such thing as life without a special teleological power.

If we test the theory of the chemicalist school by the laws of matter and force applied rigidly, it will, I think, appear that it does not fulfil the conditions of a true physical theory, because the "directing idea or agency," or "vital force," which they are all compelled to fall back upon, sooner or later resolves itself into a teleological cause, either of the nature of the *horror vacui* or of a substantial principle. Unless, therefore, we admit the sufficiency of the metabolic or vital state of affinity, we cannot uphold the physical view; but if we do, then Beale's animism becomes superfluous.

§ 85. From what has been said at § 60, it must be obvious that in my opinion the explanation of vital action on the physical view must not be sought in any form of force known or to be discovered, but solely in the development of a peculiar kind of chemical affinity, called here metabolic. Likewise, as shown at § 60 and 63, the law of equivalence of force is maintained in all molecular changes, whether under metabolic or chemical affinity, because the force that unites the atoms is the same, whether determined by the property of the one or the other affinity. Just as weight is the product of the property of mass and the force of gravity, so the so-called attraction of chemical affinity may be more strictly spoken of as the product of the *property* of chemical *affinity* and the *force* of chemical *attraction*. To enable us to distinguish different genera of affinities with the same force of attraction, we may call the

latter intra-molecular attraction. The tendencies of bodies, at sensible distances, to fall, or be pushed, or attracted to each other, is equal for all kinds of matter ; hence mass can be determined simply by weight ; but it is different with respect to intra-molecular attraction, and whether two atoms of different kinds will have the tendency to fall towards each other at all, and thus combine, and how much of "that which is expended in the production of motion" will be required in that combination, depends entirely on the specific, inherent, unalterable, inexhaustible property called affinity. When two bodies have combined, they form a new substance with new properties of affinity, quite distinct from those of the original elements ; and this process of development of new affinities may go on to an extent to which we know no limit, while, at the same time, the force of intra-molecular attraction, that pushes and binds the atoms together, is the same, and can undergo no variation except that of quantity. It is therefore quite conceivable that, besides the immense variety of substances developed by the ordinary degrees of chemical affinity, including ternary and even quaternary compounds, which can be made, unmade, and made again by ordinary chemical processes, there may exist a whole genus or class of affinities produced by an atomic constitution of a complexity at present incomprehensible, and far surpassing that of the ordinary chemical compounds, and over which we could have no power by ordinary chemical means, except for destruction. This would be the metabolic or vital state of aggregation of matter. Here force would stand exactly in the same relation as to ordinary chemical compounds, and we would express this state as the product of the property of metabolic affinity and the force of intra-molecular attraction. In this state force must play exactly the same part as in chemical processes, and will be evolved as active force, or consumed in counteracting intra-molecular attraction, in proportion as the product of any transformation possesses lower or higher potential energy than its factors.

In many ordinary chemical processes no change of temperature and no transformation of force take place ; in fact, no

work is done, and the molecular motion required is furnished by the molecular velocities, conditioned by temperature. In like manner, many vital actions connected with growth and development, and probably perception and thought, are not work, and require no direct expenditure of force, although, as a matter of fact, every stage of them does require some expenditure of force for the molecular motions accompanying them, and the internal stimuli essential for every vital action not dependent on external stimuli.

When we thus separate distinctly the ideas of property and force, and confine the latter to its strict meaning in physics, we perceive the completely subordinate, although still essential, part it plays. Force can have no power in producing, or developing, or altering the character of chemical affinity in itself. Nor has it any influence (unless for destruction) in chemical processes, except in furnishing 'that which is expended in the production of motion,' and thus giving the means of the display of the properties of the elements. Force is the mainspring of the cosmical machine, but not the works. It is in the properties of matter that lie the wonder and mystery of the universe. It is therefore superfluous to insist upon the error of attributing to force in any possible form the power of forming or building structure, or of producing development in the organic world. It is altogether an erroneous use of the word force, and it can hardly be the idea of those who have so spoken. I believe that they really mean simply to uphold the physical view of life as distinguished from the teleological; but from a loose and inaccurate use of the word force, they fall into a mode of expression, such as that the sun forms, or builds, or constructs organised beings or particular parts, &c.

Nevertheless, the language of some of the earlier authors who brought the force doctrine into physiology in this country is certainly chargeable with being the source of much of this vagueness. Here I may notice a little more in detail the essay of Dr. Carpenter, alluded to at § 61.

He first states that the characteristic of organised matter is its *germinal capacity*, which enables it to develop into its proper

form and no other. If this is derived in whole from the parent, then the aggregate of all the germ forces of all the descendants, however remote, must have existed in the original progenitors, which is a *reductio ad absurdum*. Therefore "we may consider it proved that, in some way or other, fresh *organising force* is constantly being supplied from without during the whole period of the exercise of its activity." Again, "the actual *constructive force* is supplied by heat." "The heat, acting through the germ, supplies the constructive force or power by which the vegetable fabric is built up." Also, "an additional source of organising force" is to be found in the retrograde metamorphosis of organic compounds. He says of plants, that a part of the ternary compounds are brought back to binary compounds, and in thus falling to a lower plane the power consumed in their elevation is given forth in the form of "heat and organising force," which help to raise the other portions to a higher level, as is seen in germination. These expressions are very liable to mislead, although we may understand them to mean that the force is merely that which is necessary for the work done, for he says, "When we look carefully into the question, however, we find that what the germ really supplies is not the force, but the *directive agency*," so the expressions "constructive and organising" are not accurate as regards the force, which is merely used as a power guided by another, to which the word constructive and organising more properly apply. But what are we to say to this passage applied to animals?—"The source of the various forms of Vital Force—which may be distinguished as constructive, sensori-motor and generative—that are manifested in the different stages of the life of an insect, we find them to lie on the one hand in the heat with which the organism is supplied from external sources, and on the other in the food provided for it" (p. 261). Again, in speaking of the development of the higher animals, he notices the frequent retrograde metamorphosis and removal of parts, and substitution of different ones; he says, "And such removal can scarcely be accomplished without a retrograde metamorphosis, which may be considered, with great probability, as *setting free constructive force* to be applied in the production of new tissue" (p. 264).

These last I cannot explain or reconcile in any way with the nature of the physical forces, and they must have had a most confusing and misleading effect. What he here calls "forms of vital force" can be nothing but the vital, natural, and animal spirits of Aristotle and Galen, or the Anima of Harvey. No force in the physical sense could perform the functions above enumerated, and be correlative with heat and motion. Nor could any "constructive" force be set free by retrograde metamorphosis. Any force

so set free could only be one of the active physical forces we know, and, although that might be used by the living action in construction, it would not be a "constructive force." Further, the "directing agency" above spoken of is not explained. Is it a property of organised matter, and is that matter only chemically combined protein; or is it a teleological cause without substance, like the horror of a vacuum, or is it merely one of those "forms of vital force" themselves which are said to be constructive and generative as well as sensori-motor, and which have their source in heat and chemical decomposition of food? Here is a confusion which I am totally unable to unravel; and I think it forms a type of the spurious "physical explanations of organic phenomena, such as by that universal refuge electricity and the like," alluded to by Schwann. These difficulties pervade, more or less, all the chemicalist theories of life, and I do not think it possible to escape them as long as we look upon the living matter as albuminates animated by any form of physical force correlative with heat and motion. All these theorists, without exception, have in the background an "idée directrice," directive agency, &c., which must be some power called force, which is incompatible with the known laws of the physical forces, or a teleological cause in the air, like the horror of a vacuum, and thus, while professing the physical theory of life, they really abandon it. I quite agree with the remark of Beale in answer to Moxon, that the supposition of any "peculiar agency" is irreconcilable with the ordinary physio-chemical theories of life (*Med. Times*, 1866, p. 631).

I can see no middle way between the metabolic change of the living matter of Fletcher (and may we say Schwann?), as the only representative of the physical theory, and the animism of the vital principle school representing the teleological view. We must have no more halting between two opinions. Let us banish the "directive agency," the "subtle influence," and the "mysterious powers" of the present physio-chemical school to the limbo of the vital spark and subtle fluid of former days, and acknowledge that living action, growth, and development, are as much the result of

the action of pabulum, conditions, and stimuli, on a peculiar aggregation of matter now called protoplasm, as the most ordinary physical or chemical process is of its particular agencies. We know not the ultimate how or why of the very simplest of these; and till we do it is idle to speculate on the infinitely complex phenomena of life, and decree that all we cannot explain depends on a special principle. Nevertheless, between the chemicalist hypothesis of albumen animated by a vital force, and the old-fashioned animism now revived by Beale, I would certainly prefer the latter, because it is at least a respectable hypothesis, inasmuch as it has in its favour the analogy of the immortal soul, which is an immaterial substance added to matter, and transmissible from parent to offspring, whereas the former rests on a mere blunder as to the nature of force. Force is, in fact, to life, no more than the organ-blower is to the musician—essential, indeed, but utterly subordinate.

§ 86. Is, then, Beale's animism superfluous? We have seen that, with Fletcher, he believes that in the protoplasm the proximate principles do not exist, but only their elements differently arranged; that matter is never living matter when any combination into proximate principles exists, and presumably whenever the elements are arranged in the same way as they are in protoplasm, life must exist; why, then, does he require that, besides, a spiritual substance must be added to that peculiar compound in order to make it alive?

It is very difficult to find Dr. Beale's real meaning, as he has not yet given any regular and formal exposition of it, but leaves it to be gathered from incidental remarks and ironical questions, interspersed in discussions on his physiological views and in arguments against the mere chemical school. Now, in respect to this last, as we have seen, he and Fletcher are at one, so it will not be necessary to allude to them. Likewise, there is a large part of his controversial writings devoted to arguments against the physical view of nature in general as hostile to the belief in a personal, designing, superintending God, and to the Christian religion. Into these, of course, I need not enter, as they

do not specially apply to our subject, nor, indeed, to physiology in particular.

The rest of his arguments may, I think, be condensed into these two positions:—1st. The difference between the phenomena of life and those of ordinary physical and chemical action is so immense that it is impossible, or at least inconceivable to him subjectively, that any combination of matter, however different from the ordinary chemical state, can develop a property like vitality. Hence we must suppose the existence of a power superadded to matter in a quite peculiar state of combination only; capable of transmission from one portion of matter to another; capable of indefinite multiplication without loss of power, and acting as a “forming, guiding, directing power or agency” on the material forces of the body, so as to produce the phenomena of life. 2nd. Such an immaterial substance or principle we know to exist as the immortal soul, and if we allow life to be possible without this or a similar principle, it would shake our belief in the immortality of the soul and all revealed religion.

From his definition of the vital power Dr. Beale does not seem to differ from the teleological theorists of the vital principle school in general, and his language bears considerable resemblance to that of Thomson, the chemist, who in 1817 speaks of the common chemical powers as “the servants of a Superior Agent, who directs them so as to accomplish always one particular end.” Dr. Beale is, however, unwilling to be classed among the old vital principle school, and protests against the idea of a central guiding power, such as the *Ενορμῶν* of Hippocrates; and, in truth, his vital power resembles the *Archæi insiti* of Van Helmont, considering the specific differences of the several kinds of protoplasm. But Van Helmont conceived his several *Archæi* to be all subordinated to one sovereign principle, like that of the Greeks, so it is difficult to see how Beale is to account for the harmony of design of whole individual organised beings without it. It is impossible for Beale to take up any new position on this subject, for the world of thought has long been explored and mapped out, and there

is no new region left for him to discover; he can merely choose in which he will take up his abode. As precision of terms is here of supreme importance, I will again quote an appropriate passage from Fletcher:

"It is very important to keep constantly in mind the physiological distinctions between a Property, faculty, quality, or capability, a Power, stimulus, or agent, and an Action or function; the first, like irritability, sensibility, and so forth, signifying only a susceptibility of excitement; the second, like caloric, light, sympathy, &c., only a means by which this susceptibility may be called into action; while the last, like irritation and sensation, signifies the phenomena resulting from the two in co-operation. It would have appeared almost superfluous to notice these distinctions had they not been so frequently lost sight of, not only by the *οἱ πολλοί*, but even by authors of merited celebrity; Barclay, for instance, speaking continually of the *property* of sympathy; Bostock, of the *power* of sensation; and Adelon, of the *function* of sensibility. Life is a property with some authors, a power with others, and an action or series of actions with a third set; and the expression is in all these cases correct if in the first instance the term be used as synonymous with vitality, in the second as signifying a substantial vital principle, and in the last as indicating the sum of the functions—but not otherwise." (II, p. 5.)

According to these definitions Beale must be classed among the substantial vital-principilists, whether he will or not. There is one point, however, in which he differs from the majority of the school, but probably here he does not stand alone, as a similar view seems to have been held by a small section of the school, including Descartes, Needham, Darwin (the elder), Priestley, and others. This is that his vital power cannot enter into any ordinary chemical compound, such as albumen, but must first transform it into a combination *sui generis*. His living matter is thus protoplasm *plus* a portion of vital principle, just as Fletcher's living matter is the very same substance without any such superadded entity.

The ancients, who knew very little chemistry of any kind, did not trouble themselves about the difference between inorganic and organic chemistry, and it is only when the latter came up as a science that the difficulties began. One party boldly declares that the living parts are just the same

chemical principles as we find them on analysis, but *plus* the vital principle. This, whether true or not, is at least intelligible and consistent on their view that such exists. Another party is so vague on the subject that they cannot be classified. A third admit the proximate principles, deny the vital principle, and do not allow the sole vitality of the protoplasm. This is the physio-chemical school of the majority at present. A fourth, comprising Fletcher and Beale alone, deny the proximate principles, and hold the sole vitality of the protoplasm, but they differ about the vital principle.

It is, therefore, incumbent on Beale to show why there should be a vital principle added to a substance already totally changed from the proximate principles, and therefore presumed having totally different properties.

Among Dr. Beale's arguments the chief are the following:—He says vitality

“cannot be a property of matter, because it is in all respects essentially different in its actions from all acknowledged properties of matter.” (*Protoplasm*, p. 74.)

Again, “And what are we to understand by the nature or power or property of living germinal matter? With what is it comparable? Clearly not with the properties of non-living matter, for these belong to the matter itself, while living properties are transferred from one particle to others with the utmost rapidity, and the very same matter may exist with or without its vital properties.” (*Oxford Lectures*, p. 608.)

Again, “It has been argued that since the properties of water are due to its gases and not to ‘aquosity,’ the properties of protoplasm are due to its elements, oxygen, hydrogen, nitrogen, and carbon, and not to *vitality*. But the cases are by no means parallel. Of water there is only one kind; of protoplasm there are kinds innumerable. The constituent elements of the same particle of water may be separated and recombined again and again as many times as we please, but the elements of protoplasm, once separated from one another, can never be combined again to form any kind of protoplasm. But further, every kind of protoplasm differs from every other kind most remarkably in the results of its living, one producing man, another amœba, another dog, a fourth butterfly, and so on. . . . Have we not *identity of composition* in the living matter and marvellous difference in the results of the vital actions? How, then, can the difference be due to the ordinary properties of the elements?” (*Protoplasm*, p. 27.)

- In reply to the first of these quotations, do we require to say more than that vitality is certainly different from all *other* properties, developed by all *other* combinations of matter than the one in consideration? But to say therefore it cannot be a property, is simply to beg the question at issue. To the second, if he means that the same elements remain in exactly the same proportions, and in exactly the same constitution while yet life has gone from protoplasm, this would be in direct contradiction to his whole theory that the protoplasm dies into the tissue and proximate principles. To the aquosity argument I have to oppose what has been said at § 60 to 62, and only to add that, because a simple binary compound is not susceptible of the complexity of constitution necessary for the production of vitality, is that any reason why a compound of five or six elements and an enormous number of atoms should not be so? If two letters of the alphabet are insufficient for the wants of human thought, is that a reason why twenty-four should be? But we may turn the argument the other way, and ask why such a marvellous thing as the vital principle can *not* confer vitality on binary or ternary compounds? Again, we can make, unmake, and make again the chemical compounds, but we can neither make originally nor restore when destroyed any particle of living matter. Granted the last clause, but the former is not unconditionally true; we can neither make nor restore the diamond: is there, therefore, a diamond-anima? The same may be said of the vast majority of organic compounds, with which it is the rule and not the exception that, although you can destroy them all and make some out of others, yet you cannot make again one that has been destroyed.

The asserted identity of the protoplasm would be a most powerful argument, but on looking at that closely we see it rests on a pure assumption. We have as yet no comparative analysis of the different kinds of protoplasm, and it must be extremely difficult to obtain such from its being so intimately interwoven with all the tissues; but as far as the testimony of organic chemistry goes the discrepancies and imperfections in the total percentage of analysis of

different parts would lead us to presume the existence of difference in the dead products. And no method can as yet give us the least information as to any difference of atomic constitution in the inconceivably complex state of the living molecule. The only identity or unity we know is the anatomical one, which restricts vitality to the clear structureless stuff called protoplasm, but that may possess an infinite variety of metabolic constitution. Upon what is thus a pure assumption, see what a fabric Dr. Beale builds! The objections to the doctrine of a general vital principle, which, like the widow's cruse or the five loaves, can be multiplied infinitely in transmission from parent to offspring, and then annihilated at the death of each individual, have been sufficiently insisted on. But those applying to Beale's revival of the *Archæi insiti* are even more cogent. For if all protoplasm is identical, its metabolic composition can contribute nothing to the specific differences obtaining between different kinds of protoplasm. These can only depend on varieties of Archæi possessed by each; and not only must each species be represented by its specific Archæus, but each organ, and even each part of each individual, must have its own variety of Archæus. Likewise with respect to those degraded forms of protoplasm whose recognition may add additional lustre to Dr. Beale's name, are we to suppose the degradation resides in the substance of the Archæus in analogy with the moral degradation of the human soul by sin? Or that, besides the Archæus of health, the protoplasm possesses a variety of Archæi of a lower type in a dormant state, ready to be roused into activity by an exciting cause, and thus give rise to pus, cancer, tubercle, and the like? Surely it is unnecessary to pursue farther the contrast between this complicated system of entities and the simple view of Fletcher, that, as all vital properties depend on the metabolic composition of the protoplasm, so all deteriorations depend on mere alterations of that composition. We have so far seen no argument against the true physical theory of life except the enormous difference in kind between the merely physical and the vital properties, and as we pursue the

subject we find substantially the same argument reiterated in various forms. Under the head of "self-constructing properties of molecules" he speaks of the physical theory here given as if that implied that the mutual interaction of the properties and forces of matter must necessarily develop the metabolic state spontaneously, or, in other words, as if "spontaneous generation" were a necessary part of it. In opposition to that I need only refer to § 63 (p. 112), where Fletcher's opposition to the theory is quite as strong, and founded on exactly the same reasons, as Dr. Beale's. The miraculous interference of the Almighty is recognised quite as strongly as by Dr. Beale, but it is allowed to be a legitimate subject of speculation whether He then created a new spiritual entity or added new properties* to matter,

* I do not think we have any grounds for supposing that special properties were given to the four so-called organic elements which specially fitted them for life. We must remember there are fifteen other elements which take part in the structure of organised beings in our planet, and these may, on the protoplasmic theory, all have passed through the living state, for however brief a period. There is no reason to suppose that the remaining elements might not take part in the living structures, and contribute each its peculiar modifications to the metabolic state, if required for the conditions of life in our planet. We must therefore suppose that the potentiality of life exists in all the elements. Not that all inorganic as well as organic actions are to be considered a kind of life, as the ancients supposed, but simply that the elements have all the potentiality of taking part in metabolic compounds, just as oxygen and hydrogen have none of the properties of water, or of an abstract aquosity, but it is owing to their inherent properties that their compound has the properties of water. In the question of the plurality of worlds there has been apparently a too exclusive reference to the conditions of life in our planet. The essential conditions of a protoplasm, or of a metabolic state of matter, seem merely to be a certain uniformity or limited range of variation of temperature, so that some of the elements capable of manifold combinations with absorption or evolution of force, and in the state of solids, fluids, and gases, should exist. Within the ranges of absolute temperature in our planet it would seem that those best adapted for this are C, O, H, N, with some inorganic ingredients. These, therefore, must appear to us *par excellence* the organic elements, and to have some special aptitude for life. But it is not difficult to conceive that the metabolic state might be taken on at far other temperatures, say below the freezing-point of mercury or at the temperature of the sun's heat. But, of course, quite different elements would then play the part of solids, liquids, and gases, and extricate force by their combinations. It would not be difficult to construct a possible table of substances capable of fulfilling vital functions at very different temperatures from this of our planet.

or merely *fashioned* the then existing matter into a compound which, in virtue solely of that fashion, acquired the new properties of self-reproduction from heterogeneous particles which is the main characteristic of life. Dr. Beale, I must be allowed to say, rather dogmatically insists that we must believe that a vital principle was then created under penalty of being called infidels, but I rather think the last of the above modes is more consonant with the language of Holy Writ, if he will insist on bringing that into a scientific question.

Again, in spite of Dr. Beale's vehement protest against the worn-out crystal argument, I would repeat the question, why should not form spring in living things as well as in dead from the operation of causes acting in blind obedience to certain laws? A formless germ gradually grows, differentiates, and finally settles into a form of wonderful beauty, harmony of design, and adaptation to a purpose, in which every particle finds its appropriate place prepared for it long before a millionth part of these particles had any existence as such. But does not the same take place in the crystal? The individual particles of it have no power over the ultimate shape except as arranging themselves in blind obedience to the action of certain forces on their inherent properties. Is the result less beautiful or harmonious, or adapted to a purpose, and thus indicative of design on the part of the Creator? And yet does anybody imagine the existence of a teleological, prototypal, formative "power," which shall guide the particles into their places? Nor can there be any such thing as a "force" of crystallization; that is another loose and inaccurate use of this much abused word. To this Dr. Beale answers by enumerating the immense differences between a crystal and a mass of protoplasm, which have here been granted over and over again in every form of superlatives; but how does that touch the question? For if Dr. Beale will turn to any part of his own physiological writings where he describes the specific varieties of protoplasm—say, for example, that of bone, quoted here at § 83, p. 175—he will find that bone protoplasm, like many others, if taken from their natural position and supplied with

appropriate pabulum, stimuli, and conditions, will still go on forming bone, or what not, in blind obedience to its properties and the external causes, simply because it cannot help forming these tissues, although it may be to the death and destruction of the individual. In like manner the protoplasm of the eye or of the heart form these organs, marvelously perfect and adapted for their purpose as optical and hydraulic instruments, in blind obedience to the laws of their constitution, in virtue of which matter associated into their composition and acted on by the usual conditions cannot help making eyes and hearts. And if we go a step back, viz. to the germ, we find there a kind of protoplasm which cannot help differentiating into other kinds of protoplasm, which shall in turn make parts, organs, and tissues. Does it matter for the general question whether that obedience was paid to the fashion of the material particles or to the power of an equally blind and irrational fragment of Archæus? The evidence of design through the operation of blind and irrational forces and properties is thus exactly the same in the organic as in the inorganic world, only the processes in the case of the former are more complex and difficult for us to follow. It would be easy to pursue the subject to a greater length and to show that Dr. Beale's objections from the protoplasmic movements, the want of molecular machinery, the impossibility of matter guiding forces, and similar arguments, can be reconciled with the physical theory, and if not, that certainly neither they nor the previous ones prove that a vital principle could exist and be capable of functions such as are ascribed to it. The one sole and solitary argument for its existence is analogy with the immortal soul.

§ 87. But all, literally all, that we really know of the existence of the immortal soul of man is founded in faith in the revealed word of God in Scripture, and I entirely agree with those who protest against bringing arguments from that source into a scientific question. I am content to take religion wholly from the Bible, and science wholly from nature, in perfect trust that ultimately, when the interpretation of the Word of God is fully understood and when

nothing remains to be explored in science, the harmony between the two will be manifest. Before that time I am satisfied that all attempts to bring them forcibly into harmony will remain, as they have been hitherto, injurious and derogatory to religion and destructive to the progress of science. In particular the wish to support our faith in the immortality of the soul by calling in the aid of the heathen idea of a vital principle is here to be objected to. For it is nothing but an idea of heathen origin. If we trace it back to its source we find that Beale's vital power differs in no way from the immaterial entity of Alison, Kirby, Bell, Prout, Barclay, and others of the last generation, nor in any essential particulars from the Principle, Archæus, Anima, or what not, of Barthez, Whytt, Stahl, Van Helmont, Harvey, and others of former generations. Beale is, in fact, perhaps, most nearly represented by Barthez and Van Helmont. But there is no essential difference between all these and the philosophers of the pre-Christian period. In fact, the vital principle of the moderns is, as Fletcher says, just the same thing or the same nothing as the Πνεῦμα of Galen, after Chrysippus, the Ψυχὴ of Plato, or the Φύσις of Pythagoras, who borrowed it from the Egyptians, and they in turn from the Brahmins of India, with their Elementary Fire or the Soul of the World, a portion of which—a *divinæ particula auræ*—was supposed to be imparted to every living creature and constitute its proper life. Thus, the vital principle rests on, not only a heathen, but a pantheistic idea. Dr. Beale here treads delicately. No one supposes him to hold the pantheistic notion, but he nowhere, that I can find, distinctly states what becomes of his "vital power" at death, so I assume him to mean that it is annihilated. It is, therefore, a species of mortal soul, and why that which passes away as completely as the metabolic fashion of each portion of protoplasm should lend support to our faith in the immortality of the human soul (if any support is needed from such a source), I am at a loss to perceive.

The truth is, the false use of the word materialism is a mere bugbear, although it certainly has still some influence

against the physical theory even in the minds of the learned, while it largely influences the numerous half-instructed readers of the semi-popular literature of the day. I have no pretensions to argue this or the full question involved in the bearing of science upon spiritual religion as revealed in the Scriptures, but I shall content myself by recalling the deliverance on the subject before us of a man of large and philosophic grasp of mind like Fletcher, who was a member of the Church of England, and whose orthodoxy was never, as far as I am aware, called in question.

"There is in the minds of many persons, not only among the uninformed, but also among the educated, a vague indefinite kind of impression that the vital principle, the sensitive principle, the rational principle, and the immortal principle, are all identical; and that he who denies the substantiality of the first does the same with respect to all the rest. This impression appears to have originated in the ancient complicated absurdity of applying to the three supposed principles of life, of sensation and of thought, and to the one real principle of immortality, the same name as soul, spirit, and so forth." (II, p. 32.)

"That the soul is something entirely independent of either a sensitive or a rational principle will be as far as possible demonstrated in future; and that it is independent also of a vital principle must in the mean time be obvious to any one who considers for a moment that this latter principle is ascribed indiscriminately to every organised being—a quadruped, a bird, a reptile, a fish, an insect, a worm, a zoophyte, the lowest fungus—while a soul is imputed to man alone. The differences in the vital phenomena displayed by the most abject tribes of organised beings and by man are only in a degree; in this respect *Faba est cognata Pythagoræ*; the principle which we concede to the latter as the cause of these phenomena must be conceded equally to the former; and if in the one case this principle "*Redit in nihilum quod fuit ante nihil*," if it be in the one case "the be-all and end-all," it must be so in the other also; so that to connect man's hopes of immortality with the admission of a vital principle within him is so far from favorable that it is directly opposed to all the best persuasions of religion, natural as well as revealed.

"But the hopes and expectations of man are founded, or should be founded, on a basis infinitely more sure than the supposed existence within him of any such principle—on a basis proper to him, not common to him and the vilest worm or weed—on the suggestions of his reason and on the strength of his faith, and as so long as these actuate him every other evidence of the existence of a soul must be superfluous, so were these once withdrawn every other evidence must be nugatory." (II, p. 33.)

“Nor is this view of the matter, as is sometimes vaguely supposed, in any degree hostile or inconsistent with the purest and loftiest religion. The hackneyed arguments against this opinion, founded upon its supposed immoral tendency and impiety, appear to proceed upon the principle, certainly erroneous, that the mind and soul are identical. Who that has watched for five minutes the action of a dog can be so blinded as to deny that he possesses attention, imagination, abstraction, judgment, desire, grief, in short all the intellectual faculties and passions in the display of which *thought* consists; but who will attribute to him an immortal *soul*? The existence of such a substance, attached during life to the body of responsible man, and surviving him to all eternity, we are at once intuitively led and explicitly taught to believe; but it is a question of morality and faith, not of physics and demonstration.” (III, p. 93.)

“‘I have no hope of a future existence,’ observes the late talented Regius Professor of Divinity in the University of Cambridge, ‘except that which is grounded on the truth of Christianity;’ and it was well remarked lately, ‘That if man be not satisfied to place his hopes of immortality in a Divine gift, he must confess that the difference between his own claims and those of many other animals is in a degree only, and that degree in some instances a very small one.’” (III, p. 94.)

“As often, then, as it shall be said that mind, or the faculty of thinking, is a property of living matter, as much as irritability or sensibility are properties of it—that it is born with the body, developed with the body, decays with the body, and dies with the body—it is to be understood to mean the mind only, and not the soul. The soul is certainly something not material, indeed, but substantial, a Divine gift to the highest alone of God’s creatures, responsible for all the actions of the mind, but as totally distinct from it as one thing can be from another.” (III, p. 95.)

It may be thought presumptuous in me to enter the lists against one who has made such great discoveries as Dr. Beale, but as expositor of Fletcher I am compelled to do so, because, as before said, they are the only two who hold the residence of life solely in the protoplasm, and I am bound to protest against the presumption of animism as necessary to that theory. For my own part, perhaps, I may be allowed to express my belief that Dr. Beale’s reversion to animism is only a temporary revulsion from the gallipot pathology of the mere chemicalists to which he expresses so much aversion. I can hardly think he has arrived at the matured stage of thought on the subject, especially as we see that on one point, viz. the argument from the proto-

plasmic movements he makes a mere slip which he will doubtless correct ere long of himself. I am not without hope that if he is induced by the coincidence of Fletcher's theory with his own to study the system of the former, he may be led to abandon his temporary reversion to hypotheses which are the fruit of the subtle imaginations of the Greeks and their imitators the schoolmen of the middle ages, and return to the masculine philosophy of Bacon and Newton.

FURTHER OBSERVATIONS UPON THE INTERMITTENT ACTION OF *SULPHUR*.

By Dr. R. T. COOPER.

As a further contribution to the study of what I believe to be a most important sphere in the curative action of our foremost polychrest, the cases subjoined are appended. Elsewhere I have expressed an opinion that, notwithstanding all our vaunted acquaintance with the symptoms of *Sulphur*, and in spite of the universal confidence it has enjoyed among homœopathic physicians for the past half century, there yet remains—and I assert it with the greatest respect and in true modesty—much to be investigated in regard to it.

The question of its relationship to neuralgia may still be considered *sub judice*, and we bring forward the following cases as a mere link—though for ourselves fully convinced—in the chain of evidence we hope some future day to adduce, in order, either way, to place the matter beyond dispute.

In my pamphlet* published on this subject I hazarded an expression of my belief that not alone in neuralgia, but as well in intermittent fever, *Sulphur* requires at our hands more distinction than it at present obtains. And at this moment of writing, instead of retracting I find myself

* *Sulphur as a remedy for Neuralgia and Intermittent Fever.* Lond., 1869.

obliged in all honesty to further assert, that its action upon the entire nervous system is imperfectly, very imperfectly known, and my sole reason for saying this is, that I have seen sufficient to justify such assertion at the bedside, a retrospect of which forces conviction upon me.

Here and there through our clinical records we meet with cases of epilepsy, chorea, and the like, cured by *Sulphur*, even after failure of the acknowledged specifics, but strange to say this has not led to that thorough sifting of the question of its action upon the nervous system the facts demanded, but on the contrary, their authors most unfortunately bowed to the pennant hoisted by Hahnemann, and contented themselves with explaining the cures effected by it on the hypothetical presence of a psoric diathesis.

I can hardly believe that Hahnemann intended his renowned theory to cramp like this all warrantable research, and certainly he could not have intended that *Sulphur* should be singled out as *the* antipsoric, and its cures explained in accordance with this dogma of his, while those cures effected by the other members of the antipsoric group were almost invariably, upon scientific principles, referred to a specific relationship existing between the morbid state and the pathogenetic effects. Theoretically it is our custom to prescribe on the principle of similars, but, practically, this valuable index is placed in abeyance when we allow ourselves to be guided by the very questionable hypothesis, that an obscure condition lies lurking in the system, though in what part of the body we cannot tell, only that it requires the internal administration of *Sulphur* for its successful eradication, and that it is often accompanied, always proximately, or remotely preceded by, skin disease.

We may accept as axiomatic that *Sulphur* never removes symptoms it is not in relationship with, and if it cause to disappear morbid manifestations which have not been developed in its provings, the reason is that these provings are incomplete, and not that the symptoms were accompaniments of an obscure miasm. This at least is the only explanation that accords with the principle expressed by the homœo-

pathic law, and is now being admitted by all rational homœopathic practitioners.

The statement is a plausible one that suppressed eruptions are followed by internal complaints, whatever Hebra and other modern pathologists may assert to the contrary, but it does not necessarily follow that, therefore, the majority of chronic complaints are consequent upon inherited or acquired skin disease. Nor are we justified in treating internal diseases on the supposition that they take their origin from them unless the further history of the cases plainly points to their having done so, and even then we have to prove that their psoric character must be accepted as the chief indication for the selection of the appropriate remedy.

If we wish as a school to obtain a knowledge of the pure effects of our drugs we must rest contented with simply observing the line of action they pursue in diseased and healthy bodies, and we must be careful not to allow our observations to be leavened by any theory, however plausible.

When Hahnemann asserted that the cure of intermittent fever by *Sulphur* was the result simply of the antipsoric properties it possessed he allowed a theory, in this instance most pernicious, to overweigh his proper judgment, and consequently to interfere with the only reasonable interpretation of the fact, which must have been that a homœopathic relationship existed between the intermittents and the effects inherent in, but undeveloped by proving of, the drug itself.

That subsequent inquiry will establish the truth of this affirmation I have little doubt, but whether or not it matters little so long as we can prove *Sulphur* to possess a power over intermittent diseases beyond what we had hitherto suspected.

If we acknowledge the truth of Hahnemann's statement that it is curative in many forms of ague, and if, as observers had proved, certain forms of ague were undoubtedly the homologues of certain forms of neuralgia, it will naturally follow that *Sulphur* will prove curative to those

neuralgias that are, as we might term them, *lesser* grades of the *Sulphur* agues. In this way we *explain* a fact on the principle of homœopathic relationship—the cure of ague; we enlarge this by further showing the conclusion deducible from it—the cure of neuralgia. Provided this latter is correct, and that *Sulphur* can be proved to possess the properties claimed, it is, as we said, of little consequence whether this be upon the principles of homœopathy or not; for our first and most sacred duty is to our patients, our second a purely conditional one, to homœopathy. Let our motto be: proud to be a homœopath, but before all things a physician.

The similarity existing between ague and neuralgia may be illustrated by two cases published in my pamphlet on this subject. At p. 18 we find the ague case, and at p. 24 the neuralgia. In the former the febrile attacks began at 8 or 9 a.m. and lasted eight or nine hours, beginning at nine, therefore the paroxysms would cease about five in the afternoon; in the latter the attack commenced at about 10 a.m. and lasted till about 5 p.m. It reached its acme at two o'clock; the ague fits came on every second day, and reached what may be accepted as their acme in two hours; the neuralgic fits came on every day, and reached their height in four hours. We see the likeness between the cases, therefore, in the time of day at which the paroxysms began and ended, in the hour at which the concentration of the symptoms took place, and, what is of great importance, though no mention is made of it in the reports, the similarity of the prostration and the malaise engendered by both diseases was very marked. The ague requires forty-eight hours, the neuralgia—the lesser ague—requires twelve hours, before, as it were, the system ceases any longer to resist its effects, and even these furnish a similarity, the one number being a multiple of the other.

In such cases the nervous system is excited by forces we are little acquainted with, and all we can say is that, whatever they are, they disturb the apparently electrical condition of the body in a way very often similar.

The following case bears upon the question of *Sulphur's*

power to subdue aguish symptoms, and though solitary is, so far as a solitary case can be, a decisive one.

A farm labourer of 73, once a stalwart able-bodied man, but now bending under years, who had been treated more or less successfully for headache and deafness at a homœopathic dispensary, complains that for two years he has been in the habit of having shivering fits like ague every three or four weeks. The fits last three or four hours; the duration of the cold stage depending mainly upon the way in which he wraps up. If he wraps up warmly he can calculate upon its cessation within the hour; if not, two or three hours is the average: this is succeeded by extraordinary heat of the entire body, but *no perspiration*. When the attack seizes him his custom is to roll himself up in blankets, piled one on the top of the other, until he begins to get warm; he then becomes sick, and vomits the contents of his stomach; there follows then a fearful pain in his head, which generally continues two or three days after each paroxysm has ceased.

During the first week of treatment, and when taking *Sulphur*, he had a slight shivering fit, which was not accompanied by sickness or followed by headache. He has now been under observation some months, and there has been no return of his ague-like symptoms.

The headache in this case presents us with a very common accompaniment of ague, and shows how intimately connected are the two diseases, neuralgia and intermittent fever.

The sickness of stomach—another common accompaniment—was probably an effort of nature to overcome the strain imposed upon the weakened nervous system by the digestion of food.

The brow ague of malarious districts, and the frontal headache of *Quinine*, can hardly be distinguished from each other. A short time ago, in conversation with a hair dresser, he assured me that he had seen the most marked frontal headache produced by the local application to the scalp of *Quinine* hair washes, although, as he said, they acted capitally in increasing the growth of the hair. His

description of the headache quite tallied with that of malarious brow ague.

During the winter of 1868-69 I had under my care at our dispensary perhaps as bad a case of neuralgia as it is possible to meet with. The subject of it was a female district visitor who had, at various periods of her life, been much exposed to inclemency of weather, and had thus, seven years before coming under treatment, contracted a very bad form of neuralgia, and although it now appears this affection arose from an irritation transmitted to the part through the presence of a tape worm, its narration will, I think, be instructive. As happens very often in such cases, this poor woman had suffered many things of many physicians, and was nothing better but rather grew worse; indeed, she had been under nearly every medical gentleman in the town without deriving the smallest benefit from their prescriptions.

Her general appearance betokened much suffering, and she had an unhealthy, pinched, and sallow look. For five successive winters she had endured intense agony with scarcely any abatement, especially when the weather was very cold. She is obliged to keep in doors and wrap up when the pains are violent, and she dare not stir out under penalty of excessive aggravation of all her sufferings. In the summer, too, the pains are liable to come on when she catches cold, so that she can hardly be said to be free from them all the year.

The right eye and adjoining parts form the seat of the principal pains, but the pains radiate in all directions, especially up the head. She is necessitated wearing spectacles whenever she goes out in order to protect the eyes both from cold and light, the admission of either bringing on a fresh accession of suffering, but in very cold weather even this is not sufficient, and she has to remain in doors. Last winter she did not attempt to go out for two months. In violent paroxysms she dare not open her eye, and sometimes, if the attack be very severe, not only the right but the left eye becomes affected.

I made a note of her symptoms as follows:—The pain is now confined to the forehead, and comes on at unlooked-for

times ; if it proceeds as far as the eye the rule is that it will last unabated for weeks, until it seems fairly to wear itself away from sheer exhaustion. When acute the pain is such as to deprive her of all reason, in fact, she dare not attempt to speak ; and already the sight, especially that of the right eye, is much impaired.

During the paroxysm of pain the eyes twitch and the skin of the temples becomes wrinkled, and after three or four hours the stomach becomes upset, and vomiting takes place. Aggravation occurs from the night air, a draft of cold air, or movement of the body. Nothing ameliorates. The bodily functions are natural, except that the bowels are inclined to constipation.

My first prescription for this patient was *Colocynth*, which was chosen on account of the general character of the pains, their extreme intensity, and the accompanying dispositions to cramp-like symptoms in the eyes and forehead. It was given in the 2nd decimal tincture, which produced a constant tight screwing pain in the left side of the chest, with a painful throbbing at the heart, and a too frequent movement of the bowels. Next week the 30th dilution was therefore selected in preference, but as it had no effect upon the neuralgic pains, after an interval of a week, the 6th dilution was given, and under this she improved for three or four weeks, and her report then was that she had not been so well at this time of year (November) for four years, but the moment she leaves off taking the medicine the pains return.

The 6th dil. was continued for another week—four weeks altogether—but seemed to lose its effects, and the 3rd dec. was then given ; it seemed valueless, and as the weather was getting colder, and the pains were evidently increasing, *Belladonna* 3rd dec. was prescribed, the symptoms being, “An intense pain over the right eye, though a somewhat different pain from what it was ; it is now a dull pain, and the upper eyelid is swollen. Darting burning pain in the vertex, which has become aggravated since the eye became worse. A little gland is swollen at the back of the ear, but the ball of the eye is not affected.”

The *Belladonna* did not relieve, and we then took the following note:—"Has been much worse. During the week she was much excited by hearing of her father's death, and the pain went away, but has returned worse than ever. An eruption came out on the forehead after taking the *Belladonna*, which seemed to keep the pain from attacking the eye."

She was now put upon the mother tincture of *Sulphur*, and a decided change for the better at once took place; the pains lessened, and she could soon venture out without any fear of the intense cold that then prevailed bringing on the pain. What further proved the happy influence of the *Sulphur* was, that the pains returned a day or two after she left it off.

Now and then, when under its influence, she would catch cold, and the pains return, but by steadily persevering with it, their violence became abated, and what further showed its power, her sight decidedly improved.

After two months of *Sulphur* the report was, "The pain has become more like an ordinary headache," and she has a cough which is worse at night, and is very hard and dry. *Phosphorus* in the 6th was then interpolated, but she presented herself again, complaining of not being able "to get on without her neuralgic medicine;" there is a soreness felt in the ball of the right eye, and "yesterday the skin of the temple was drawn down and stiffened from the violence of the pain." Much headache was complained of as well. The *Sulphur* was now resumed, and we find that at the end of the following fortnight the soreness in the eyeball and the pain in the head had gone; in fact, she was altogether better. "The medicine," as she says, "seems to stave away the pain." In the beginning of March she discontinued the treatment, declaring herself freer from pain than she had felt at any time for seven years. During last winter this sufferer underwent very much the same course of treatment, only that the pains were all through greatly less severe, and she was not obliged to protect her eyes as formerly. The *Colocynth* a second time succeeded in sub-

duing the pain, until very severe weather set in, and then *Sulphur* had to take its place.

It is to be hoped that under the influence of *Sulphur* the affliction will eventually wear away completely.

Thus far I had written when this patient came into my study complaining of having passed a large piece of worm, which, upon examination, turned out to be tape worm. So far from lessening, it greatly adds to my reverence for our polychrest—a drug I have often found to expel the larger intestinal worms, lumbrici and tænia, when their presence was not dreamt of; and surely the powerful control it exercised over the severe pains in spite of this grave irritation is satisfactory evidence of its “pain-killing” properties.

Every one conversant with neuralgia in its aggravated forms must admit that the probability is paramount, that the impairment of sight, already commenced, would, if the pains continued with such intensity, have eventually super-induced incurable amaurosis. The retina, its irritability evidenced by an inability to endure its natural stimulus, light, could not have borne the prolonged erethic condition the pains occasioned without loss of activity, ending, doubtless, in an incurable paralysis.

It is thus that we find an irritable condition of other organs, as the bladder, stomach, brain, &c., attended with symptoms pointing to a depression in their normal functions, and our surprise is awakened only when we consider the exemption from paralysis enjoyed by the dental nerves, though exposed more than any others to the causes calculated to produce it. As Graves remarks,* “It is very strange that no example of paralysis of the dental nerves has as yet been observed. This subject has engaged my attention for several years, and I have been in the habit of inquiring from all my paralytic patients whether the sensibility of the teeth was lessened, and in no one instance have I been able to detect anything approaching to loss of sensation in these organs, an immunity difficult to account for, and, I believe, unexampled, for I am not aware of any other nerve, either of sense or of motion, which is not occa-

* ‘*Clinical Medicine*,’ p. 810. Dubl., 1843.

sionally involved in the progress of paralytic affections; nay, I have more than once been obliged to direct the removal of teeth in hemiplegic persons, in consequence of toothache on the paralytic side. This immunity from paralysis, corroborated by the extensive experience of my friend Mr. McClean, seems the more surprising when we recollect how subject the dental nerves are to the opposite affection, or a morbidly increased and exalted state of sensibility, constituting the various forms of toothache."

Seeing this, it is not a little surprising that among the dental symptoms of *Sulphur* we should have a near approach to dental paralysis.

"When biting the teeth feel loose, and when eating as if PARALYSED."

"Dull Teeth."

"Dulness of the teeth, and pains only when biting, &c."

In the review of my pamphlet on *Sulphur as a Remedy for Neuralgia and Intermittent Fever* in this Journal (No. cxi, p. 192), one of the cases brought forward is objected to, because the reviewer whose remarks, I am compelled in fairness to add, were exceedingly just, and, considering the novelty of the subject, very flattering, opined that "*Arsenic* would have effected a cure in twenty-four hours," whereas my statement is that, "by the end of the week, the pains had vanished." Now, in the first place, any one conversant with dispensary practice must know that the patients usually attend but once a week, and if one finds the pains are gone by the end of this time, it is often as much information as can, in the hurry of the moment, be elicited; while, in the second place, my object was gained, even supposing *Sulphur* to have required a week, and *Arsenicum* but twenty-four hours, to bring about a successful issue, for the intention was simply to establish a proof of the power possessed by *Sulphur* to subdue certain nerve pains; and confining ourselves to this, we left unnoticed a like universally admitted property of its kinsman *Arsenicum*.

Indeed, the probability is in favour of *Arsenicum*—we
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are speaking of aguish diseases—especially when exhibited in infinitesimal doses, being of the two the quicker acting remedy, its effects sometimes manifest themselves with lightning-like rapidity; while we agree with Bahr, that “it is only exceptionally that *Sulphur* will develop its effects in a very short time” (*Sc. of Therapeutics*, vol. i, p. 418). Of all our remedies there is none perhaps that better merits the distinguishing epithet “brilliant” than *Arsenicum*, and therefore we readily concede that so far the reviewer’s remark was a just one. But *Sulphur* and *Arsenicum* are, it is obvious, different remedies, and affect the nervous system in a manner corresponding to the distinctive characters of each, and even though *Sulphur* may require a week to eradicate symptoms that easily succumb to *Arsenicum* in twenty-four hours, yet the fact that *Sulphur* can eradicate them is so much gained—it will do this *plus* something more. Admit this and for the moment I am content.

The remark, however, opens up a very interesting question, but one that we are unable to do more than glance at for the present, viz. the similarity existing between the neurotic property of *Arsenic* and that of *Sulphur*.

We allow the probability of *Arsenic* being the quicker acting remedy, but—and this is important—we believe we are correct in saying that it is so *only when administered in infinitesimal doses*. The allopathic experience of it is that (I am quoting from Toogood Downing’s prize essay on *Neuralgia*, p. 123, Lond., 1851) “it should be given in full doses, and continued until its effects are felt on the system. As soon as symptoms of poisoning appear it should be discontinued, *but not before*, as it is *then only that its specific effect is manifested*.” (Authority quoted, Hunt on *Tic Douloureux*). Dr. Downing gives a case of his own in point which, without conceit, we can assert would have succumbed in a day or two to a few harmless homœopathic doses of *Sulphur*, if not to the millionth potency of *Arsenic*. We give the case as it stands.

“A. H—, a lumper in the East India Docks, living in the Isle of Dogs, complained, September 11th, 1848, of violent

pain in the right temple, shooting from thence up to the top of the head, down the neck, and to the back of the ear. This had seized him suddenly upon returning home from work one afternoon about a fortnight before. Obligated to give up his employment. Could not sleep at night. Appetite gone altogether.

Upon inquiry, found that the pain was remittent. It never left him entirely, but came on with increased severity at times—about four in the morning, and from six to seven in the evening. The paroxysms returned, also, at uncertain intervals during the night. The pain not increased by warmth or pressure. Teeth sound. Tongue clean, but rather white.

Concluded that the *tic douloureux* originated in malaria; so, after regulating the secretions, I prescribed, September 14th—

R. Liq. Potassæ Arsenitis, ʒss;
Tinct. Zingib., ʒj.

Of this the patient took ten drops, gradually increased to twenty, three times a day, after meals.

September 21st.—The pains have considerably abated, but still are troublesome. Increased the dose to thirty drops.

28th.—Patient reports himself perfectly free from pain in the head and face, but complains of a burning sensation in the pit of the stomach, with feverishness. Urine scanty and high coloured. As these symptoms were considered to be indicative of the full action of the *Arsenic*, that medicine was discontinued and ordered to be resumed on their subsidence.

October 5th.—The pains in the head have returned with some severity. They vanished, however, as before as soon as the system was affected by the mineral.

Two other attacks were similarly treated.

By using great care not to push the action of the remedy too far (?), and by steady perseverance in its use, all symptoms of the disease yielded before the expiration of the month and did not return."

"This," says Dr. Downing, "is, perhaps, a favorable

instance of the remedial power of *Arsenic* in neuralgia. The effect is by no means always so perfect or so permanent. The disease generally returns after the symptoms of poisoning have passed off." From which, without any sacrifice of generosity, we may gather that, in the hands of old-school practitioners, *Arsenic* is not much to boast of as a remedy for neuralgia; and it is worthy of observation that the most recent allopathic authority on the actions of drugs—Sydney Ringer—speaks but slightly of it.

On the other hand, compare this with the assertion of Dr. Richard Hughes (*Man. of Pharmacodynamics*, p. 111), where he states very correctly the unanimous opinion of homœopathic practitioners—"it is one of the very few medicines which cause genuine neuralgia, and it far exceeds all others in the treatment of the idiopathic disorder;" and again, "*Arsenic* exerts a magical effect over pure neuralgiæ wherever occurring." This is strong language, but amply borne out by clinical experience.

In my pamphlet before referred to the characteristic neuralgia of *Sulphur* is thus described:—It is an intermittent periodic neuralgia, in which an aggravation takes place every twenty-four hours, generally at twelve or one o'clock, either in the middle of the day or at midnight."

Mark how this tallies with Bæhr's description of *Arsenic's specialité*. "It will be found the more effective the more purely nervous the pains are. . . . The paroxysms break out or *exacerbate about midnight*. . . . For prosopalgia, caused by miasmatic influences, *Arsenicum* occupies the first rank as a curative agent." (*Science of Therapeutics*, vol. i, pp. 232-3.)

As showing that this nightly exacerbation is common to both, we quote the following from our dispensary books:

Eliza O—, æt. 30. Ill two weeks with neuralgia; right side of face and head affected.

It came on "all of a sudden" in the cheek-bone when engaged in washing clothes. About a year ago this patient was treated by us for a neuralgic attack, then met by *Pulsatilla*.

There are two kinds of pain present—a shooting and a throbbing.

The shooting pain goes from the temple to the second molar tooth of same side, and then up the side of the head; just as she is going to bed, about 10 o'clock, the pain comes on most terrifically, and continues violent all through the night. Sleep is out of the question.

The throbbing pain is constant day and night, though worse at night.

The eyeballs ache and the sight is much impaired when the pains are at their violence. Aggravation when she is heated, and from wrapping the head up warm.

The bowels are moved but once a week; menstrual functions natural. Ordered *Sulphur* ϕ .

The following week a manifest improvement had taken place; she could sleep well, and, except for a slight throbbing in either temple and some tenderness of the bones, there was nothing amiss. Same continued.

The next week she came back again an invalid; she had caught fresh cold from exposure to damp, the old symptoms had returned,—with this remarkable difference, however, that whereas previously the pains were worse from the face being wrapped up and from warmth, they were now, although still worse at night, *eased by warmth*; and from being one-sided and confined to the face, they are now settled in both temples, and extended to the shoulders; they seemed, too, more to affect the bones, and to take on more of a dull aching character than before.

Arsenicum 3^x (trituration) was given, and by the end of the week there remained but a trace of the pain, which lingered on for some weeks and then went entirely away.

Further, the conclusion at which the Vienna provers arrived was that the pains produced by *Sulphur* to a great extent resembled those of *Arsenic*.

We are justified, therefore, it seems to us, in saying that *Sulphur* acts upon the nervous system in a manner very similiar to *Arsenic*. The finer effects of both drugs are very much alike, and for this reason we think we were right

in the opinion we hazarded in the very commencement of this investigation when we asserted that for diseases having a malarious origin, *Sulphur* as well as *Arsenic* must hold a distinguished position, and that inasmuch as *Sulphur* exerts an influence over cachectic states of the constitution much wider than that of *Arsenic*, there are grounds for believing that it will, when more extensively put to the trial, prove equal, if not superior, to that remedy for "eradicating the morbid principle that keeps up aguish symptoms after the patient was removed from malarious contagion."

We do not ask any one to accept this conclusion until the facts are such as to warrant their doing so, but for ourselves we are firmly convinced it is a correct one, and we hesitate the less to declare it from our becoming more and more decided in our belief as we proceed with this investigation.

The resemblance between *Sulphur* and *Arsenic* is strikingly apparent only when they are administered in doses inappreciable; bulk for bulk they are widely different remedies, and the action of the one in no way resembles that of the other, and this shows very plainly the necessity there is for observing how drugs affect the organism—diseased as well as healthy—in their crude as well as their attenuated forms.

To obtain success with *Arsenic* in miasmatic affections it is, we hold, by no means necessary "to develop the full effects of the drug," or, in more homely and unmistakable phrase, to poison the patient; and we most heartily agree with Richard Hughes when he says that the potencies from the 6th upward are the most serviceable in neuralgia and in chronic intermittents. Had the case we copied from Toogood Downing's work not yielded to a harmless dose of the 30th potency of *Arsenicum*, we are convinced that we should most certainly have succeeded in curing it with *Sulphur*, and we found this emphatic assertion entirely upon facts presented to our notice at the bedside.

Take, for example, such a case as the following—a case that is all the more flattering to myself personally from its being communicated to me by one who, if I mistake not,

looked with scant favour upon this investigation at its commencement, and whose testimony, thus freely given, will undoubtedly carry greater weight than anything I could offer. It can easily be imagined, therefore, that it is with feelings of pride I place on record the following case from the able pen of the author of the *Manual of Pharmacodynamics*.

“In a fairly vigorous man of middle age, twice daily, between 1 and 2, lasting till 6, pain (neuralgic) in jaws extending to temples. Of a fortnight’s standing. *Tinct. Sulphur* ϕ . Cured progressively in ten days.”

Observe, the age of the patient, the times of remission of the sufferings, the duration of the complaint previous to his presenting himself for advice, almost accurately corresponded with Toogood Downing’s case; the difference in the mode of treatment pursued is, however, very apparent. Under allopathy the patient had, as a first go-off, to be purged (“the secretions regulated”), then to be poisoned (the system being put under “the full action of the *Arsenic*”), and lastly, to be kept an invalid for nearly a month, and left at the end of that time without his neuralgia, but at the dearly purchased price of subjecting his system to the destructive influence of large quantities of *Arsenic*, and the risk, alas! too often fatal, of exceeding the prescribed dose.

It is true we have not been able to collect as many facts as our previous experience might entitle us to have anticipated,* of a character sufficiently satisfactory to merit its being adduced in evidence of *Sulphur*’s powers over neuralgia and other aguish disorders; but the proofs, few though they be, are unquestionably convincing.

A short time since, in conversation with an avowed unbeliever in homœopathy, this gentleman remarked that, though an enemy to the system, he could not be persuaded but that *Sulphur* was a capital remedy for neuralgia, and he instanced two cases that had been cured with it. The symptoms of one of these I did my utmost, but unsuccessfully, to obtain, and could only learn that the man, a dock-labourer, while taking *Quinine* for a chronic neuralgia, had, at the suggestion of a friend, simultaneously tried the mother

* We must now qualify this, as sequel sets forth.

tincture of *Sulphur*, and with immediate cessation of pain. Prejudice prevented his leaving off the *Quinine*, but as it had been useless up to the time of his commencing with *Sulphur*, we can reasonably credit this with the cure.

Besides this, a medical friend assured me he had cured with it a case of ague after failure with *Quinine* and *Arsenic*, and my predecessor, Dr. Wilmot, temporarily removed the pains of neuralgia in one of his female servants that nothing else would touch.

In this class of evidence I must rank the next case with the prefatory remark that my endeavour throughout this investigation has been, and will, I hope, continue to be, to act perfectly impartially, and unswayed by self-interest, to avoid apportioning an undue importance to any facts countenancing previous statements.

My sole object is the elucidation of truth, and I have no wish to bring forward facts in a way such as is calculated to enhance, unwarrantably, their value; much more do I repudiate the idea of wishing to invent theories devoid of foundation.

The case loses its value simply from the particulars not having been acquired till some months after discontinuing treatment, and consequently it is not to be supposed that the characteristic symptoms are given with strict accuracy. It is, however, none the less a convincing proof of the fact to which we wish to draw attention. I refer now to the case of a young man, an assistant of an allopathic chemist, who, hearing that the homœopathic chemist here had "a remedy for neuralgia," thought that, as the medicines he had already taken were unattended with the desired effect, he would try what homœopathy could accomplish. He therefore procured a bottle of mother tincture of *Sulphur*, five drops of which he took four times a day in water. The first day passed without any relief, but the following morning the pain seemed less severe than usual, and continued gradually decreasing during the day, until at night he was surprised and delighted to find that the usual exacerbation did not then occur, and the night was passed in comparative freedom from pain.

This patient, in reply to some written questions, furnished me with the following particulars:—Age 28; of a dark complexion and freely perspiring skin; never the subject of ague or any kind of fever, but has lived in a damp locality. A severe cold he contracted was followed by neuralgia of the jaws and temples; sometimes the pains were fixed in one side, sometimes in the other. At first the attacks came on at a regular hour each night, and ceased after lasting about an hour and a half—it was thus for the first ten days; he then caught a fresh cold, and the pains were almost incessant and more severe at night than during the day. Dull aching, often very severe, but no shooting or burning. Here we find the intermittent form of neuralgia running into the continuous, and we find, too, the indication for *Sulphur*—an exacerbation in (the middle of?) the night. We miss, however, the mention of whether there was a steady increase of pain up to about midnight; this is unfortunate, as the periodic nature of the attacks in the first instance and the nightly exacerbation all through presented much the features of some of the cases reported in my pamphlet. Observe, too, the first day passed without his finding any diminution in the violence of the pain; and it was not till the second day that amendment set in. Now, this goes to show that *Sulphur* does not act with that electric-like rapidity that, as we before mentioned, *Arsenicum* does; however, a statement like this is of weight only in proportion to the extent and satisfactory nature of the experience that prompted it; a single case, therefore, cannot carry much weight, and I feel certain of this, that *Sulphur* manifests its effects in a much shorter time in neuralgia than it does in most other affections for which it is homœopathic. Indeed, we often, in chronic diseases at least, expect an interval of eight or ten days to elapse before we can with certainty affirm whether *Sulphur* is acting beneficially.

A rule of practice experience teaches is, continue *Sulphur* if the case allows of delay, until you are quite sure there is no benefit to be derived from it. If, therefore, no amendment take place after the first week, and yet the case is an appropriate one for *Brimstone*, repeat it and in the same

potency ; and act likewise if the state of the patient be such as to give you the slightest encouragement. This rule I invariably follow, and believe my attention was first drawn to it by Mr. Dixon, of the London Homœopathic Hospital. Intermittence is a most remarkable property of *Sulphur*, and until we commenced our investigation, it did not seem to have been suspected, certainly not generally recognised, by any school of medicine. At the present stage of our inquiry it is, of course, unreasonable to suppose that premature speculations, depending upon this anti-miasmatic property, can be accepted by a profession whose literature teems with, and is sadly deteriorated by groundless assertions. While, therefore, we hesitate to add to the speculations we have already hazarded, we can hardly be blamed for indulging in a horoscopic view, and for declaring our conviction that this anti-aguish influence of *Sulphur*, and I may add of *Arsenicum* as well, will be proved to have much to do with their glorious effects in chronic diseases. Homœopathy certainly has taught that, for one remedy for ague previously existing, there are now a hundred and one, but it has not, nor could it be supposed to define accurately the extent to which each of these hundred and one remedies may be useful, neither did it tell us that its sheet-anchor, never-to-be-forgotten *Sulphur*, is a principal remedy for periodic complaints. If homœopathy had exhausted the provings of all its drugs, each proving would in itself constitute a limitation to our knowledge of the drug's therapeutic power, but it is evident that this is far from being accomplished, and much time has yet to be spent in order to unfold the hidden virtues of our simples.

The efficacy of *Sulphur* in intermittent neuralgia is well illustrated by the following case, and though it takes from the interest attaching to it, that the patient did not remain at the dispensary till quite cured, yet there is little doubt but complete cessation of the pain ultimately ensued. How else can we explain a patient racked with torture for three years, and deriving the greatest ease from the first week's treatment, failing to return supposing the pain to have continued ?

Charlotte G—, a girl æt. 21, who came in from the country to our dispensary accompanied by her mother; this was in the latter end of May, 1870.

She had suffered for three years from headache, which has latterly been getting much more violent; the symptoms always get worse and worse until the middle of the summer, and then they steadily decline. She cannot endure being excited, nor can she bear to remain long in a heated place. She frequently faints with the pain, and is extremely nervous.

The paroxysms come on at uncertain times, generally every two or three days, and the pain, which generally begins in the left side of the head, sometimes extends all over it, but never goes so far as the neck.

She cannot attempt to eat while the pain continues, though her appetite usually is very good. Stimulants, which often ease, or even take away the pain, at times have no effect.

The pains are almost unendurable, whatever position she is in; they nearly drive her out of her senses; never make her feel sick; warmth aggravates, cold palliates, aggravation from meditation and excitement, also after partaking of food, especially if she eats quickly, friction with a wet towel gives a little comfort. A paroxysm generally lasts twenty-four hours; if it comes on in the morning it may be expected to leave her about midnight, but not without a preceding violent increase, followed by a *gradual* diminution of pain, or accompanied by vertigo, flushing of the face, and a staring appearance of the eyes.

Catamenia regular, bowels also regular, but inclined to relaxation, and she has no pain in the back.

The mother tincture of *Sulphur* was prescribed, and the following week she unfortunately arrived too late to see me, but the dispenser made the following note:—"The fainting has ceased, and the pains are not so continuous, and not nearly so violent."

In estimating the value of this case we must take into account her living some miles out of town, as well as the

fact that she came to us expressing herself quite prepared for a long course of treatment.

It is worth while observing, in connection with our patient's case, that cold alone relieved the pain, while warmth aggravated it; and as we might expect in summer the symptoms reached their culminating point, the very opposite to what we found to hold good in the case reported at pp. 19—21 of my pamphlet, and of the tape-worm case at the beginning of this article, p. 19, in both which the climax was reached in winter; and this leads me to think we may divide the year as we have done the day,—midsummer will then represent noon, and Christmas midnight: we shall then have two prominent varieties of neuralgia, classifiable according to the times of their yearly increase, and two that may be grouped according to their daily aggravation, and all curable by *Sulphur*.

Yearly.

1. Aggravation from cold; worse at or about Christmas.
2. Aggravation from heat; worse at or about midsummer.

Daily.

1. Variously affected by temperature; worse at or about midnight.
2. The same; worse at or about noon.

Taken severally any one of these may combine with any other, and consequently the classification might be greatly extended, but without any practical advantage commensurate with the inconvenience attending its length. Indeed, if we remember the above, as we can easily do, most of the intermittent neuralgias we have found characteristic of *Sulphur* can be referred to them. And I cannot help thinking that it is the times, or perhaps the manner in which the pain reaches its height, and not so much the character of the pain itself (burning, aching, &c.), or its behaviour under the influence of temperature or moral conditions, that we shall ultimately fix upon as more characteristically indicative of

the intermittent pains of *Sulphur* (and perhaps of *Arsenicum* as well); and this, although I fully recognise the fact, that warmth of bed usually aggravates the pains of *Sulphur*, while it relieves those of *Arsenicum*.

It will be evident from these remarks that it is a pure neuralgia *Sulphur* cures; the presence of swollen or bleeding gums, extreme tenderness on pressure, an inflammatory redness of the skin, or acute gastric disturbance, will, though we can hardly say contra-indicate, probably point to other remedies. The symptoms, we repeat, of *Sulphur neuralgias* culminate at noon or midnight, but we have already suggested the possibility of our polychrest being appropriate to aguish affections undergoing any notable change at these times,* and consequently it need not surprise us to meet cases requiring *Sulphur* as their specific, whose paroxysms either culminate or commence at the same hour. And this was actually so in the second case reported by Dr. Lowder, and which will be found further on, which leads us to conclude, at the risk of being thought fanciful, that an explanation of such is quite possible, on the supposition that the intermittent neuralgic pains of *Sulphur* are always multiples of the number three, in respect to their intervals, and nearly so in respect to the duration of their paroxysms.

Possibly our assertion holds good in respect to all the lesser agues (neuralgias); the greater agues (intermittent fevers) we know, in regard to their intervals, to be multiples of twelve (see Graves' *Clinical Medicine*, p. 221, Dub., 1843). But besides intermittent varieties, we shall meet several *indefinite* neuralgias for which *Sulphur* is appropriate, whose symptoms are not suitable for classification, and with them we must content ourselves in endeavouring to lay hold of those features that especially point to *Sulphur* as their specific.

The subjoined is a very fair example of a remittent neuralgia of the chest, simulating ague; it is quite an *Arsenicum* case, and serves to show how nearly these polychrests approach in their minute effects.

* Page 80 of my pamphlet.

Alfred H—, a labourer from an adjoining country village, æt. 23, had inflammation on the chest last spring, for which he was attended by a country doctor. He has never felt thoroughly well since then, and latterly he has been getting very weak.

The symptoms are :—Burning pain all round the left side below the heart and across the stomach, continuing during the day, but getting much worse of an evening ; it sometimes extends down as far as the kidney ; it causes him to shake all over, his hands, legs, and, indeed, all his body tremble as though he had the ague, and this occasions an urgent, but ineffectual desire to be sick ; he heaves a great deal. He very often suffers from an aching in the forehead, which increases simultaneously with the pain in the side ; this latter is worse every third night, though every evening an aggravation is felt. It lasts from two to three hours, and then gradually leaves him ; he often feels it when drawing a deep breath, but it is quite different from a stitch. He finds his strength perceptibly declining, in fact he has been getting so weak of late that he fears he will soon have to knock up altogether ; besides he loses flesh rapidly.

General health is otherwise very good, and the bodily functions are regular. Ordered *Sulphur* ϕ .

3rd week—a Monday's report.

After an interval of a fortnight he came back to say he had been feeling wonderfully better. He had not felt anything of the pain, except for very slight attacks on Thursday and Friday. He attributes these to a cold caught about that time, and from the effects of which he is now suffering.

In spite of the cold, he feels a great deal stronger than he did before coming under treatment. Prescribed a week of *Acon.* 3^x for the cold, with *Sulph.* ϕ to follow for another week.

This was his last attendance.

Given *Arsenicum* and *Sulphur* for such a case, many would select the former on account of its well-proved analeptic properties, and we were induced to fix upon the latter to show how similar were their effects.

Sulphur is generally looked upon more in the light of a corrective, and it gets small credit for its tonic effects. Commenting upon its effects in chronic hepatitis, Bæhr remarks, "*Sulphur* must be assisted in its action by the strength required for the reactive endeavours of the organism."

Very true, but we must bear in mind that a like remark would apply to all our remedies; without "the strength required for the reactive endeavours of the organism," our prescriptions would be hopelessly directed. *Sulphur* will help to mature convalescence just as well as *Arsenic*, if it be in relationship with this disease; if not, little can be expected from it or any other remedy. The meaning intended to be conveyed by Bæhr we do not quarrel with, *Arsenic* does, speaking roughly, kindle the slumbering powers, and brace up the system much more strikingly than *Sulphur*; but this must not allow us to hesitate in prescribing *Sulphur*, even though the bodily powers be at a very low ebb, and we think it very necessary to be guarded against being unduly influenced by generalising conclusions.

One of the great drawbacks to dispensary practice compared with hospitals is the irregular attendance of the patients. Nothing can be more provoking than to find a patient whose case interested you failing to attend regularly.

Were it not for the adventitious circumstance of his having caught cold, the case just reported would undoubtedly have neglected to make a second appearance, and the success of our remedy would have been liable to be called in question, as it has already been in other cases that we reported.

Observe the marked dual remittance—a slight exacerbation every evening, and a greater one every third evening.

We have now the pleasure of bringing forward one of the most interesting as well as the most satisfactory cures of neuralgia we have ever met with, and one whose equal it would be difficult to find in the annals of medical literature. It is to the pen of an old and worthy hero in the cause of

scientific medicine, Dr. Lowder, of Ryde, that I stand indebted for it, and only hope that many more will be found to lend assistance as he has done in this investigation, attended, as it undoubtedly is, with facts of such importance. I need hardly add that it affords me the greatest possible gratification to think that one of our oldest and most accomplished physicians admits that *Sulphur* possesses properties quite beyond what had hitherto been suspected.

J. A— was admitted into the Infirmary at Ryde, about a twelvemonth ago, for paraplegia of some duration ; he only remained there a few weeks, and was dismissed as incurable ; but whilst there, was placed in a bed near a draught of cold air, which occasioned neuralgia of the left side of the face. This ailment received attention, but without any relief. He returned to his house a few miles in the country, where his pains became worse and worse, till he could masticate only with the greatest difficulty ; articulation also was at times impossible, and sleep at night greatly disturbed, as the pains were present as much by night as by day.

He tried many different remedies recommended by his friends, but without the slightest benefit, and in this deplorable state he gave up all hopes of relief, and abandoned himself to despair.

Having been urged to try what homœopathy could do for him, he applied to me on October 15th, 1869.

Staggering into the room with stammering speech, he looked more like a drunkard than an invalid, and, indeed, had been considered a confirmed one by the clergyman of the parish, at whose house he had recently called upon business.

He described the pains as resembling the sharp stabs of a knife, limited to the space below the left zygoma but at times darting up to the side of the head, implicating the gums and dental nerves, but the teeth were sound. He had two extracted without benefit.

There were intervals of relief, the pains going and coming suddenly, but at no special time of the day. Having

frequently given ease to these cases by gentle currents of galvanism, I commenced by using this agent for twenty minutes, and prescribed some *Belladonna*.

The following week the galvanism was repeated, as also the week after, the medicine being changed to *Phosphorus*, and *Spigelia*, and *Mezereum*. Upon November 5th, finding that he made no progress, beyond a slight lengthening of the intervals between the pains, which were quite as acute, and the galvanism rather aggravating than otherwise, I decided upon giving him some *Sulphur* (mother tincture), of which he had a drop to be taken four times a day.

Upon the 18th the report was that, notwithstanding the cold raw weather, he was nearly free from pain; could sleep all night; could eat with comfort, and converse without hindrance, and that the only remnant of his disease was no more than the slightest prick of a pin. He considered himself quite cured, and now hoped that his paralytic ailment might be as effectually remedied.

The above is an example of an indefinite form of neuralgia, and one in which it would be difficult to determine where the characteristic indication for *Sulphur* lay; the pains, however, were purely neuralgic, and it is with these that, as we before observed, *Sulphur* parries with effect. It was more of the Fothergill type than the others we have reported. In the constancy of the pains, their shooting character, their tendency to shift, and their entirely disappearing at uncertain intervals, the case might be placed in the same category with the following:—

Clementine Ham, æt. 21, a very pale delicate-looking girl, arrived late at the dispensary, and *Sulphur* ϕ was prescribed for her by the dispenser, Mr. Charles Rendall.

Her face was then very much swollen, and she was suffering very much pain. The prescription was attended with marked relief, but on seeing her a few days afterwards the symptoms seemed rather to indicate *Mercurius*, and accordingly I gave her the *Solubilis* in the 3rd decimal trituration; there was pain extending up the left side of

the face, teeth, and head, with considerable stiffness of the neck.

A week passed without relief, and *Sulphur* (mother tincture) was again given, the symptoms being :—stiffness in the neck and pain in the teeth remain as before, the pains are worse in cold air, and better in warmth of any sort; she is better when walking about, but as evening approaches the pains become intensified; the pain will often come on with a twinge when sucking at the teeth, and dart up the side of the head and then as suddenly leave.

The gums are not tender. The pains shoot up to the eyes, sometimes on one side, at others the opposite. There is a pain in the back of the head as if the scalp had been beaten.

If she warms one side of the face by the heat of a fire the pains will be driven to the other side, and sometimes to the back of the head, or even away altogether.

I saw this patient some days after giving her the *Sulphur*, and was pleased to learn the effect was immediate; the pains went away as if by magic and had not in the slightest degree returned.

Dr. Lowder of Ryde has also furnished me with notes of the next case.

Mrs. O—, æt. 65, of Ventnor, consulted me upon November 24th, 1869, for a severe form of neuralgic headache of a periodic character.

She said that she had recently had an attack of influenza cold, to which she was subject, and which had subsided under the use of the remedies she usually resorted to, and that it had left behind it the neuralgic ailment, which she had suffered from for years whenever she had been lowered by any attack of illness.

She describes the pain as affecting the greater part of the head, coming on about noon every day and lasting till evening, when it gradually subsided. Her general health, though always delicate, was otherwise pretty good. I prescribed for her tincture of *Sulphur* ϕ , a drop four times a day,

for three days, and then if better tincture of *Sulphur* 12 three times a day.

December 7th.—She wrote me in excellent spirits, No. 1 cured the headache rapidly, and No. 2 seemed to benefit her whole system.

She visited me the following April to tell me she had no return of the headache, and begged for another of the invaluable powders to keep by her as she was going to Bournemouth.

Hitherto the facts brought before us have limited us to facial and cranial neuralgias, but we would leave a wrong impression on the minds of our readers if they inferred from this that it was only in these varieties that *Sulphur* took precedence. On the contrary, wherever neuralgia can exist there, other circumstances indicating, we are firmly convinced *Sulphur* can act with effect.

A few cases of gastric neuralgia have lately come under treatment, and our readers will doubtless be interested by the narration of them. The first of these cases was treated with intercurrent doses of *Aconite* during the extreme violence of the pain. So far it is unsatisfactory, though without bias I may add my firm conviction of the inability of *Aconite* alone permanently to subdue such symptoms.

William C—, æt. 36, a wheelwright, came to our dispensary from the country to be prescribed for. He has suffered from agonising pains in the stomach for the last year, and has vainly sought relief from allopathy; during all this time he does not remember having had a single day's freedom from pain. He gets up in the morning with a severe frontal headache, which changes about ten o'clock to a most violent gastralgia; he has to bend forward for relief. Latterly the pain has been increasingly severe, and for the last three days it has extended to the hips.

Sometimes he is much troubled with flatulence, and when at stool his strength seems entirely to leave him—he “comes over powerless;” the character of the *fæces* is natural, but he passes a great quantity of thin white mucus

at times. The bowels are inclined to be constipated when not taking purgative medicines. There is no coating on the tongue.

Tincture of *Sulphur* ϕ three times a day, and *Aconitum* 1^x gtt. v—Aqua 3ij gtt. v every hour if the pain be very violent. In a fortnight he came back to report wonderful improvement; the pain had quite left the stomach, though there still remained a good deal of frontal headache in the morning. This I presume has since gone away.

The above might be looked upon as neuralgic (gastric) accompanied by intestinal catarrh.

Then, again, *Sulphur* removed in a man of very pale complexion, a boot closer, æt. 30, one who had suffered for six weeks a tight crampy pain in the stomach, that prevented him rising from his seat, felt when laughing and sneezing, worse when waking in the morning and attempting any muscular exertion. *Sulphur* only was used, and very speedy and permanent relief followed.

But the most convincing case of the kind that ever came under our notice, one that, like the former two, was met with since we commenced this article, awaits publication. The signally complete failure of the allopathic remedies in two of our largest provincial hospitals gives prominence to the case.

William K—, æt. 48, a carpenter, residing near Romsey, has suffered from fearful attacks of colic for eight years. He attributes his ailments to his having lived in a house surrounded with trees and exposed to damp exhalations; at all events, until then he had always enjoyed the best of health. He has sought, but fruitlessly, advice from the principal medical men in the neighbourhood, and for four years was under one of our leading consulting physicians. For one month he was an in-patient, and for three months an out-patient at the Royal South Hants Infirmary, where he was treated for lead colic, besides which he was interne patient in Winchester Hospital for a month. From both institutions he was discharged without experiencing any

relief, and at the earnest solicitation of one of our former dispensary patients he was induced to try what we could do for him at the Southampton Homœopathic Dispensary. His symptoms ran thus:—Pain in the chest for the last eight years like spasms and cramps, coming on generally before dinner at noon, relieved then though not removed by eating, getting worse again before tea, and ceasing on his partaking of it; he then enjoys freedom from pain till midnight, when the pain with fierce intensity wakes him up, but this midnight paroxysm is of but short duration. Eating warm things relieves him, he suffers much from flatulence, and there is often much audible rolling and gurgling in the abdomen.

These symptoms, he says, appeared to come on with slight dyspepsia, such as heartburn, &c.; for forty-two weeks he did not earn a halfpenny, owing to the prostration occasioned by his sufferings. After this he was free from pain for ten months; it seemed to go away of itself, but now he suffers as much as ever, and has done so for several months.

The bowels are regular, the appetite good, and the urine, though high coloured when the pain is on, presents to all appearance nothing abnormal.

The fact of the exacerbation at noon and midnight was not elicited at his first visit, and considering how many of his symptoms pointed to *Carbo animalis*, I gave it in the first decimal trituration.

He complained, when he came to the dispensary a fortnight afterwards, that he had been worse than ever, and he pointed to the situation of the diaphragm to show where the pain “drawed him double,” obliging him to lean against something hard in order to get relief. When the pain is bad the hands and arms seem to be paralysed; they are quite powerless, and he cannot use them at all until the pain has left. This has always been the case. His tongue presented a white irritated appearance, and his stomach was decidedly acid.

I prescribed the tincture of *Sulphur*, and now in a fortnight the scene changed; he returned, and with delight

assured me he was quite free from pain ; for the first week decided and steady relief, and for the second entire immunity from suffering was experienced.

Sulphur in the 30th I have found the best remedy we possess for that pain so frequently met with—a sharp pain at the heart, going through the chest, to between the shoulders. It is especially useful when dyspeptic symptoms accompany it. *Sulphur*, as Jahr tells us, appears to have an affinity for the left side of the chest.

It is said that there is a marked analogy between diseases of the liver and those of the kidney, that no pathological disorganization can be detected in the one that will not be found to prevail in the other ; and we are inclined to think that a certain analogy can likewise be traced between diseases affecting the stomach and those of the bladder, and that when we have found a remedy for any form of gastric disturbance we may, with some probability of success, administer it in vesical disorder as well.

Sarah S—, æt. 18, was admitted as a patient last January. She had been for some months a patient at the allopathic dispensary, besides having had private advice from a medical man in the town. She is of sanguine temperament, pock-marked, and has dark circles round her eyes. Ever since she was nine years of age she has suffered from urinary symptoms ; very distressing pain when urinating ; urinates day and night ; cannot retain her water over twenty minutes at a time, often not even so long. Urinates more frequently by day than by night.

Sometimes there is not much pain, but generally, and always when bad, there is a heavy bearing-down pain, with occasional burning and smarting in the passage. She is troubled with ascarides in the rectum, but she does not think that her symptoms are altogether referable to this cause ; is not aware that she grinds her teeth, but used to do so when a child. No nasal irritation. Urine is sometimes thick and cloudy.

Standing about does not increase her distress.

Catamenia scanty, and suffers much at these times ; has never been regular ; generally ill every two or three months. When unwell she has bearing down in the parts ; there is no leucorrhœa, though she has been subject to it—does not come with any increase in the bearing down. The monthly illness does not occasion any increase in the urinary distress.

Sulphur in the 30th was given with manifest effect in assuaging the pain, and enabling her the better to retain her water, although it did not seem to have had any power over the oxyurides. The patient remained three months at the dispensary, and left without any inconvenience in regard to her power of retaining water, but still suffering from threadworms. Just before she left *Cina* was given, but except for this, we kept her on *Sulphur* all through.

The above case I am inclined to regard as one of true vesical neuralgia, caused probably by the verminous irritation, and kept under control entirely by the *Sulphur*. It is to be observed that she used to suffer from paroxysms of intolerable pain, which, singularly enough, generally came on towards the end of the week, relief ensuing upon her urinating freely.

The only symptom pointing to irritability was the burning and smarting in the passage, and this is quite consistent with the existence of neuralgic affections.

We think it well to observe before concluding that the preparation of *Sulphur* we used, the mother tincture, was chosen simply from our experience of its singular efficacy in the treatment of these nerve affections, and that if we were to rely upon analogy,—if we were, in fact, to apply the rules to *Sulphur* that guide us in our dealings with most other remedies,—we should certainly give the preference to the higher dilutions. Neuralgic pains we believe to be better met by the very highest dilutions than by the lower and less attenuated preparations. But although, speaking generally, this statement can hardly be gainsaid, yet it is quite possible that *Sulphur* forms an exception ; and, according to Hahnemann, the mother tincture is a distinct

and separate preparation, and contains some only of *Sulphur's* valuable principles. Why this should be we cannot imagine, but the inference is plain; it may possess others that are additional and peculiar to it. This is one of the many questions opened up by our investigations, and one we must leave for the present unanswered. We ought to remember that one of our cases (see pamphlet, p. 28) was cured with the 2nd decimal trituration, another with the 30th; and that Dr. Berridge professes, as we shall see presently, to have produced neuralgia with a high potency of *Sulphur*, from which fact it would appear that other preparations as well as the mother tincture are curatives of neuralgia.

The more I become acquainted with *Sulphur's* curative action, the more I become convinced that we must administer it in all possible forms in order to secure its entire effects.

Bähr accomplished a cure of Acne rosacea with *Sulphur* by shaking up the crude substance in water, and applying this liquid as a lotion to the affected part; and surely the profession at large has never lost its confidence in sulphur ointment as a local application destructive of the itch parasite.

At one time I was in the habit of prescribing rather largely the 30th dilution of *Sulphur*, and certainly with very satisfactory results. If possible, I shall some day investigate how far its principal homœopathic preparations differ in regard to their curative powers; but the subject is a larger one than I could at present think—if, indeed, I ever feel equal to the task—of undertaking, besides having but few facts by me at present to bear upon it. It is a matter much to be regretted that Hahnemann has left us so greatly in the dark as to the individual effects developed in his provings of the several preparations of *Sulphur* employed, and the same remark, stereotyped as it is, applies to other drugs.

To many it will appear a weak point in homœopathy that *Sulphur* is found to possess the properties we claim for it, and they will naturally ask, Why was not all this ascer-

tained long ago? how comes it that homœopathic physicians, with all their boasted provings, and with a sixty years' experience, have scarce suspected the facts you have brought forward? To these I answer that homœopathy, like other sciences, is progressive, and that it does not profess to have exhausted any of its provings, more especially those of a comparatively inert substance like *Sulphur*. My friend Dr. Berridge, as I have just stated, assured me, in a communication I received some time since from him, that "*Sulphur* in a very high potency caused in a lady that exact kind of headache which you cure by it—a dull headache, commencing in the morning, gradually increasing till noon, or 1 o'clock, p.m., and then gradually decreasing;" and this, as I have already stated, is, I am convinced, characteristic.

I believe there is hardly a solitary instance on record of a symptom being removed by the direct action of a remedy without the drug being capable of producing a like symptom in health; and hence it is quite reasonable to suppose—nay, it necessarily follows—that *Sulphur* is in homœopathic *rapport* to all the symptoms we have cured by its means. By removing accumulations in the intestines, unloading the stomach, or exciting the secretions of the liver, kidneys, skin, &c., and by other derivative measures, we may, and undoubtedly do, conquer pains in various parts of the body, but there is nothing of a derivative nature in the power exercised by *Sulphur* over nerve-pains by which we can account for its beneficial effects, and its properties are, moreover, quite independent of its bulk, and show themselves only when administered uncombined and taken upon an empty stomach. Hence we can easily understand how it is that *Sulphur*, save in exceptional instances, disappoints us when exhibited in alternation with other drugs, and when given intercurrently, requires several hours to develop its proper effects; at least, this is my individual experience.

I am much inclined to think there are many acute diseases in which *Sulphur* may be given with advantage every now and then between the changes of the remedies, as well to counteract the disturbance caused by past, and

no longer useful, drug-action, as to alter any dyscrasic tendency on the part of the patient himself.

Those who have followed us through these pages must have seen from our remarks that there is an intimate connection between *Sulphur* and *Arsenic* in their capacity as antiperiodics; and hence it will follow, if we proceed with our investigations, we must do so not only with *Sulphur*, but *pari passu* with *Arsenic* as well. They cannot be separated, and, being so nearly allied, will require at every step we take to be compared and contrasted side by side in order that the appropriate and distinguishing sphere of each may be accurately apportioned. Without this a great and unnecessary amount of confusion must result, and it will be but obscurely apparent to what extent science has benefited by the introduction of another remedy for these excessively troublesome and unmanageable diseases. In all such investigations we are convinced that the proper mode of proceeding is that enunciated by the illustrious founder of homœopathy, to interrogate nature as we find her in health and disease; her responses alone can decide the question.

I have always maintained that it is much preferable to prove one fact thoroughly in regard to the action of a drug than to encumber our literature with the enumeration of the many that rest upon insecure and treacherous bases, or are derived from imperfect and equivocal observations.

An accurate science demands that there be no uncertainty or hesitancy as to the truth of the facts claimed on her behalf, and for the attainment of this much desired consummation it is evident we must advance with extreme caution, and before we draw improbable conclusions and indulge in far-fetched speculations, we must be able to point to trustworthy beginnings. It is much more attractive, and for a time, at least, brings much greater praise, to write exhaustive treatises, to laud drugs for all manner of diseases, lest some one might escape notice for which, perchance, it may some day be used at the suggestion of another; but those who act in this narrow-minded and selfish spirit have resting upon them a responsibility greater than they appear to realise, and such would do well, ere

they encumber science with their pretentious assertions, to think what grave consequences these may lead to, and how difficult it will be for their successors to expunge their treacherous errors.

When, therefore, we profess to have struck upon a vein of singular wealth in the regard to *Sulphur's* action, we do not ask any one to accept as final this or any other statement founded upon it that may have escaped us in the course of this essay; we would rather leave the matter to be decided by the experience of our colleagues, and the inferences they draw from this experience. To quote Fothergill's words, "it is with a view to promote some attention to this object," namely, the alleviation of the sufferings occasioned by that torturing enemy to mankind (Facial) neuralgia, "that I have troubled you with these reflections."

And now, if we be allowed a rather too lengthy postscript, a privilege usually confined to the fair sex, the more so as it contains the pith of the whole matter, we would observe that, although the pathogenesis of *Sulphur* does but very slightly lend colour to past observations, yet the same cannot be said of *Sulphuric acid*, whose proving eminently corroborates the most characteristic of them. We make no apology for the fact that until this moment we had not observed how strikingly the symptoms of *Acidum sulphuricum* accord with what we have, by relying upon practical experience alone, found to be part and parcel of the curative sphere of *Sulphur*. But so it is, *Sulphuric acid* produces upon the healthy symptoms that would lead us to suppose it possessed properties analogous to those that *Sulphur* exerts over the sick; which is much the same as saying that *Sulphur* and *Sulphuric acid* are principal remedies—aye, homœopathic remedies—for aguish diseases. The former has established its character by curing, the latter by producing, and the clinical experience required to raise *Sulphuric acid* to the foremost rank among the anti-periodics will, I trow, be soon forthcoming. Boënnighausen, it is remarkable, does not even mention it, though his pamphlet gets the credit of being almost exhaustive.

Whatever uncertainty there might have been at the commencement of our inquiry about the neuralgic action of *Sulphur*, there can none now remain upon the minds of any scientific homœopath when he reflects that a substance of which it is the principal compound produces the self-same principal symptoms upon the healthy body. We have only to look at the proving to see that *Sulphuric acid* produces—

Pressure in the right side of the forehead as from a blow, *increasing at first, afterwards disappearing suddenly.*

Constriction of the forehead, *increasing at first, afterwards disappearing suddenly.*

Pain over the left orbit, as if a plug had been pushed in, *increasing at first, afterwards disappearing suddenly.*

Dull stitch deep into the brain, below the left frontal eminence, INCREASING SUDDENLY, DECREASING AFTERWARDS, AND FINALLY DISAPPEARING ENTIRELY.

Pricking in the skin below the right eye, FIRST INCREASING, AFTERWARDS DISAPPEARING.

Bruised pain in the left malar bone, increasing at first, disappearing afterwards and suddenly.

Tearing in the left molar teeth from evening till midnight.

Every three seconds a pain like a shock in the left ulna, close to the wrist-joint; the pain begins of a sudden and violently, and irradiates into the arm, *becoming weaker and weaker as it proceeds, and finally ceasing altogether.*

Intensely painful pressure in the instep of the right foot, *increasing and decreasing.*

Pain in the left sole, *increasing at first, afterwards jerking, then disappearing suddenly.*

Besides others unnecessary to particularise.

From which we gather that *Sulphuric acid* produces symptoms that suddenly come and suddenly go, leaving the patient entirely free from their effects in the intervals—this a natural inference; in other words, it produces intermittent diseases, and as true neuralgic pains as any remedy in our pharmacopœia, and it requires but a slight push to bring it side by side with our old and well-tried anti-aguish drugs.

Sulphuric acid and *Sulphur*, and probably, too, their salts, are principal remedies for ague; if, therefore, we are making a selection for intermittent diseases, and if a choice offers, we ought to give the preference to the preparations containing the greatest quantity of *Sulphur* or of *Arsenic*.

The *Sulphate* and *Arseniate* of *Quinine* we duly appreciate. *Zinc*, *Iron*, and *Copper*, *et hoc genus omne*, have yet to establish the superiority of their sulphate compounds over their other salts.

Up to the present as a school we have had a very meagre experience with *Sulphuric acid*; it has scarcely ever been prescribed, but deserves, I am emboldened to say, greater respect than we have hitherto thought of paying it, and I hope soon to find that it has been thoroughly put to the test as a polychrest remedy.

But *Sulphur* has other near relations that, no doubt, in the event of her failure, will lend a helping hand. Among these we must give decided precedence to *Ipecacuanha* and *Stannum*; the former Teste places as the type of a group of which *Arsenic* is a member, and in the prefatory remarks to the group he says that their primary action is followed by "*intermittent fevers*, that never fail to make their appearance." He then, as is his wont, goes on to dwell upon the symptoms peculiar to the type, *Ipecacuanha*, and of his experience with it, and he quotes Hahnemann as saying that certain quotidian intermittents, with absence of thirst during the heat, cannot be cured as effectually with any medicine as with *Ipecacuanha*. Taken with this assertion and with our observations upon *Sulphur*, the following paragraph cannot fail to interest the intelligent reader. At p. 362, he says—

"I have used it with good effect in constrictive and contusive headaches seated in the left parietal region, coming on every day at 11 o'clock in the morning, increasing progressively until the pain became intolerable, then decreasing again in the same manner, and ceasing so completely about two in the afternoon that the patients only preserved a remembrance of their sufferings, and were perfectly able to resume their usual avocations."

This, as our readers are aware, is the identical headache for which we consider *Sulphur* indicated.

We possess in *Stannum* another closely allied drug. Observe its symptoms :

“ Pressure in the left temple, first weak, afterwards increasing, and then decreasing again as if the forehead were being pressed in.” And mark how this tallies with *Sulphuric acid*, which has—

“ Pain over the left orbit as if a plug had been pushed in, increasing at first, afterwards disappearing suddenly ;” in fact, the symptoms are as nearly as can be identical.

But what still further bears out the relationship is that they both produce pains that increase gradually and decrease in like manner. Thus, Hahnemann tells us that many pains (of *Stannum*), especially drawing pains with pressure, commence lightly, increase gradually to a very high degree, and decrease again as slowly.

This is a well-established characteristic of *Stannum* ; it was referred to in the last number of this Journal by Dr. Dudgeon, who gives a case cured by it, and also by Dr. Villers in vol. xvii, p. 165.

We have, then, placed before us a most interesting group of four remedies—*Sulphur*, *Sulphuric acid*, *Ipecacuanha*, and *Stannum*—bound together in the closest affiliation by that singular property that they have all the power of producing symptoms that come gradually, reach an intensity, and then decline, leaving the patient quite free in the interval ; but we have seen in the case of *Sulphuric acid*, at all events, that some of the pains come on suddenly, at once reaching their climax, and then gradually disappearing ; “ increasing suddenly, decreasing afterwards, and finally disappearing entirely ;” and it is quite possible, although we fail to find it in the provings, that all possess this characteristic. I am sure *Sulphur* does (see case at p. 690). Now this leads us at once to epilepsy and epileptiform affections ; diseases, especially the former, it would be very difficult to develop in any voluntary proving ; indeed, we have, as is well known, to rely chiefly upon the poisonings for information as to the drugs that produce these diseases, and consequently

their clinique is unsatisfactory. In treating epilepsy, therefore, we cannot always be guided in strict accordance with the prevailing symptoms; we have to depend upon contingent circumstances, and often find—at least, the writer has—that by meeting some apparently insignificant symptoms one frequently arrives at the specific remedy. Any clue to the treatment of these affections, even though it might be found not to be based upon scientific accuracy, ought to be hailed with delight. It is not without interest, therefore, that we read in Hempel's *Materia Medica* of a case of epilepsy in a female who had tried every known remedy in vain, having been cured completely at the end of three weeks by nothing but *Sulphuric acid*; that *Chorea* and convulsions have been cured by the same remedy; that Meyer Abraham has seen epilepsy *produced* by *Tin*, and that *Tin* has cured a young savant of epileptiform convulsions, without entire loss of consciousness, that came on in daily paroxysms; that *Ipecacuanha*, besides producing convulsive symptoms (see Imbert-Gourbeyre's article "*Ipecacuanha*" in this Journal, vol. xxviii, p. 9), acts remarkably in every kind of convulsions in infants, in tonic, and especially in chronic convulsions, from those of the eyes, face, and thumb to the most terrible epileptic fits; and that *Sulphur* has produced epileptic fits (Hahnemann), while several observers have found it curative in the same affection. My own experience with *Sulphur* in epilepsy is decidedly favorable; in one case of the most aggravated form of the disease where the patient seemed at death's door, its administration was succeeded by the most striking palliation, and even when given in the 30th dilution the patient, who is at present under my care, can distinguish it from other remedies, and always experiences relief from it.

Some time back we discharged as cured from the dispensary a case of epilepsy in which the fits came on every three months; but his subsequent history I am ignorant of, and therefore hesitate to put it forward as a complete case.

Another affection nearly allied, as the late Dr. Rutherford Russell has shown to epilepsy, is asthma, and in these four

remedies, together with *Arsenic*, we possess its chief specifics.

The statements made about *Sulphuric acid* in the concluding remarks appended to my paper, based as they are partly upon the hitherto unutilised symptoms in its strangely neglected proving, and partly upon the observations we ourselves have made upon *Sulphur*, find striking confirmation in a case we met with the other day at our dispensary. We may mention that we have to thank a recent, unexpected, and opportune influx of neuralgic patients for enabling us to confirm our suspicions that this investigation would result in bringing to light important facts before being concluded; and at the present moment evidence is accumulating of a nature sufficiently important to warrant the publication of a separate treatise.

A friend of William K—'s, whose case is given at p. 692, was induced by him to try a system of treatment that, he assured his friend, had given him more relief in a fortnight than he had obtained for years from ordinary drugs. The similarity of their cases added weight to his entreaties, and accordingly Thomas P— came down from Romsey and consulted—14th August—for the following symptoms.

He was apparently a strong healthy man, forty-six years of age, though his pinched expression showed plainly that he was no stranger to suffering. Like his friend Mr. K—, his case had been pronounced by his medical attendant as one of lead colic, though, like him, he could not account for his having in any way been brought into contact with this mineral, as he had always worked in the capacity of a brickmaker, and there was no discoloration of the gums.

He has suffered for two years from pains in the pit of the stomach, which first showed themselves with sickness and severe gastralgia; for months he was unable to walk about the house from the excessive weakness and painfulness of his limbs, and the cramp; pains across the stomach were something terrific, causing him to writhe in agony. He was too powerless even to get in and out of bed

without help. The worst attack of all, however, was last spring, when he was confined to the house for seven weeks, and this he has never recovered from, though of course not suffering the acute agony he then endured.

The pain, which is situated at the lower part of the chest, commences in the left, and extends round the hypochondria to the opposite side; it comes on every now and then during the day, preventing him engaging in any but very light work, but at night is at its worst.

The pain commences in earnest about an hour after he goes to bed, when it wakes him up before midnight, beginning about eleven, and lasting till about one or two next morning, sometimes longer, sometimes shorter than this. When it comes on it is continual, there is no variation in it, except that it commences very suddenly and with intense severity, and declines gradually.

The urine is thick and high coloured, with a red sediment in it, and the bowels are confined, but there is no flatulence.

The teeth, which, before he was seized in this way, were firm and sound, are now loose and decayed, and this he ascribes, and I suppose correctly, to the effect of the strong medicines he has taken.

I gave this man *Sulphuric acid* in the third decimal attenuation, and in a fortnight he returned with a very different tale; his night's rest had become peaceful and undisturbed from the third day of taking the medicine; the pain in the chest had quite left him, he felt much stronger, and except that he had some catching pains across the loins, there was nothing whatever amiss.

This case suggests a remark or two. The pains were of a character so intense, and had apparently become so firmly rooted in the system, that one could not wonder at its having been pronounced a case of, at all events, metallic poisoning.

Sulphuric acid has been recommended by Liebig as a chemical antidote to lead, which, by combining with it, sets free an insoluble salt in the system, and this recommendation has been well supported; and were we to proceed

with our investigation, we feel sure we should be led to exactly the same conclusion upon homœopathic that Liebig has arrived at upon chemical principles. We are convinced that the majority of cases of poisonings by the metals owe their origin to the absorption into the system of exceedingly minute metallic particles, and that these can be neutralised by equally minute atoms of their chemical antidotes.

And further, we have reason for saying that very often the chemical antidote is also homœopathic, and *vice versa*.

ON THE ACTION OF LEAD UPON THE UTERUS.

By D. DYCE BROWN, M.A., M.D.

IN the last number of this Journal Dr. Dudgeon, in appending some remarks upon a very interesting case cured by *Plumbum*, says, "Though there is no actual proving of *Plumbum*, there is no substance whose general physiological action is better known to us through the recorded cases of poisoning." If I am not mistaken, however, its action on the uterus, which I shall show is very decided, is not generally known, or, if it were, we should find it having a leading place among uterine medicines. From what I have seen of homœopathic literature, it is never, or almost never, made use of as a uterine remedy, and I, therefore, have thought it might not be amiss to draw the attention of the homœopathic portion of the profession to this important point.

Before quoting the scanty allusions to its uterine action to be found in homœopathic works on *Materia Medica*, I shall make extracts from a very interesting and valuable article in the eighth volume of the 'Transactions of the Obstetrical Society of London,' entitled, "On the Influence of Lead-poisoning in producing Abortion and Menorrhagia, with cases," by Benson Baker, M.R.C.S., District Medical Officer of Christ Church, Marylebone.

Mr. Baker says, "The subject of lead-poisoning in pre-

ducing abortion and menorrhagia has received but little attention in this country. I am indebted to Dr. Graily Hewitt for having my attention directed to it. In his recent work *On the Diseases of Women*, he says, 'Menorrhagia may be present in cases of lead-poisoning. It was first pointed out by Paul, in the *Archives Générales de Médecine*, that abortions are very frequently observed in women subjected to the influence of lead, and also in the same class of cases menorrhagia is very common. I have observed cases the facts relating to which are quite confirmative of Paul's statements. The subject of the influence of lead-poisoning in thus inducing menorrhagia is both novel and important.' The above is all the information that I have been able to collect on the subject from the medical literature of this country. I have therefore consulted M. Paul's paper, and from it have gathered some important facts, which are more or less substantiated by cases that have come under my own observation. M. Paul says, 'The first time my attention was drawn to the subject was in the month of February, 1859, when a woman that worked at cleaning printer's types applied at the Hospital Necker, suffering from menorrhagia. Coupled with this menorrhagia she also had the ordinary symptoms of chronic lead-poisoning. I learned from her that, previous to her present employment, she had been delivered of three healthy children at full term, still alive; but since her employment as a type-polisher she had suffered much from ill-health. Three months after taking to this employment she became tainted with lead-poison, and suffered from painter's colic. Four years later she had a second attack of colic, and suffered intense pain; shortly after she became pregnant, and was delivered of a dead child. Three years elapsed and she had a miscarriage at the fifth month of her pregnancy. Besides these two cases of pregnancy, she had become eight other times pregnant, and each time, after a short suppression of the menses and the delay of two or three months, she miscarried, characterised by an abundant menorrhagia, and accompanied with colicky pains at the time.'

“This patient also informed M. Paul that all the other females employed in the establishment suffered more or less from menorrhagia. Struck by this remarkable coincidence, M. Paul has collected the histories of 81 cases ; and he has come to this conclusion, that not only has the absorption of the lead-poison by the mother an influence on the offspring, but also, if the father be tainted with lead-poison, we may expect to find it influencing the foetus in utero, or the child when born. He gives a few cases of abortion and menorrhagia occurring where the women had nothing to do with lead in any form whatever. He affirms that the effects of lead are not only manifested by the symptoms generally recognised, but also (1) by the occurrence of severe hæmorrhage, which he considers as abortions in many cases, but which it is difficult to prove, although the signs of pregnancy have existed for some time. (2.) By recognised abortions, occurring between the third and six months of pregnancy. (3.) By premature delivery. (4.) By the death of the children within the first three years of their life. M. Paul gives in detail the history of four women who were married after having become tainted with lead-poison. These he calls the first series of cases, and from an analysis of them we obtain the following results. These 4 women had 15 pregnancies, distributed as follows, viz. 10 abortions, occurring between third and sixth month ; 2 premature births, the children dying soon after birth ; 1 child stillborn ; 1 delivery occurring at the full period, but the child died the same day. Out of these 15 cases, only one child was born alive, that did not show any symptoms of a lead-diathesis. Then, in order to prove that these cases were not merely remarkable coincidences, he gives the history of 5 women who had given birth to 9 children before they were subject to the influence of lead-poisoning ; the children were healthy and alive. Neither did the mothers suffer from any menstrual irregularity ; but after going into the type-cleaning works they had together 36 pregnancies, distributed as follows :—26 abortions, from the second to the sixth month of pregnancy ; 1 premature birth, the child dying soon after ; 2 children stillborn ; 7 at full term, of whom 4 died

in their first year, 1 in his second, and only 2 still alive, 1 of whom is very delicate and anæmic. This he calls his second series of cases. What he gives under the head of the third series is only a single case, viz. that of a woman who, after having 5 abortions, left the type-polishing work, and having recovered from the effects of lead-poisoning, gave birth to a healthy child, still living. Under the fourth series he relates the case of a woman who left the works for a time and then went back. It appears that during the time she was under the influence of lead-poisoning she frequently aborted, but during the interval she was absent from the works she gave birth to a healthy child. "The father," says M. Paul, "has a greater influence upon the offspring than is generally supposed. In support of this hypothesis he mentions the cases of 7 women who had nothing to do with lead whatever, but whose husbands were subjected to its influence. These 7 women had together 32 pregnancies, resulting as follows:—11 abortions, 1 still-born child, 8 full-term children, which died in their first year, 4 that died in their second year, 5 that died in their third year, and 2 only that are now alive, one of them only being twenty-one months old. . . . M. Paul makes a few remarks on the cases of some women who gave but slight evidence of lead-poisoning. Out of 29 pregnancies thus observed he has collected the following results, viz. 8 abortions, 1 premature birth, 12 at full term, which died in the first year of their life, 8 children still living."

Mr. Baker then proceeds to give the following cases which came under his own notice:—

"Mrs. S—, æt. 34, of a dark and rather sallow complexion, and of delicate appearance; rather below the middle height. She was in poor circumstances, and consequently had to help to keep her family by washing. I was called in to see her, September, 1864. About two years previously she had been an out-patient at St. Bartholomew's for some chest affection. She is the mother of four children. She had miscarried once previously, owing to a severe fright. After that she became pregnant, and was delivered at full term of her youngest child. Her husband was formerly a

plasterer and gilder, but not getting employment in that line of business, he has for the last two years been engaged in the painting trade. During the two years he has suffered seven times from painter's colic. His wife has been in the habit of washing his clothes, and she complained that the smell made her sick, and since her husband has followed the painting trade she has never felt well. Before she became pregnant she observed that her menses were more profuse than they had previously been. On the 22nd September she was suddenly seized with labour pains, and she lost a great deal of blood. The next morning I saw her; her pulse was feeble, and she was blanched. Those who were in waiting had thrown away the contents of the utensil, and with the clots, I presume, the embryo. On making an examination per vaginam I found the os uteri dilated about the size of a florin, and the secundines protruding through the os uteri; the cervix uteri was contracting tightly, and retained the placenta. I introduced my finger into the uterus and removed it. I made every inquiry of the woman as to the cause of the miscarriage, but she could assign none; and I was at a loss to discover the cause until I examined the gums, when I found the pathognomonic sign of the presence of lead-poisoning. She had advanced to the third or fourth month of her pregnancy.

"In this case there was persistent menorrhagia for five weeks, and as far as I could ascertain there was not any cause for it, except the lead-poison. I found that the local applications of cold and astringents failed to check the menorrhagia, and it was not until I treated the woman for chronic lead-poisoning that I succeeded in restoring the uterus to its normal functions. I may mention that at first the hæmorrhage seemed to be increased by the *Iodide of Potassium*, but that afterwards it subsided.

"My next case is as follows:—In May last I was called to see Mrs. W—, a painter's wife, who was dying from flooding. I found the bed saturated with blood, and it had run on to the floor; the foetus was hanging half way out of the os uteri, and a stream of blood was pouring away.

I removed the foetus, and induced the uterus to contract. This was the fourth time she had miscarried. She had suffered from painter's colic five or six times; had only been married two years, and never was in health when her husband was at work among the paint. She could give no reason for the present miscarriage, nor for the previous one. I examined the gums, and found a faint blue line. This woman was some time before she got round. I advised the husband to obtain some other employment. He has gone into the police; and I hear that his wife's health is decidedly improved, and she has now advanced to the fifth month of her pregnancy."

Mr. Baker mentions, without detail, a third case, where, however, there was syphilis on the father's side, so that he draws no conclusion from it.

Before making any remarks on the above, I shall bring together what we find in homœopathic writings on the subject.

In Jahr we find, given as its pathogenetic action, "Leucorrhœa—Abortion." There is no hint of its use therapeutically, except a very feeble one. We find in the repertory volume, under "Abortion," fifteen medicines in a primary list, and in a secondary list of eighteen others *Plumbum*.

In Hempel's *Mat. Med. and Therap.*, vol. 2, we find the following:—"Lead causes pulling, tearing, and contractive pains in the . . . uterus, also in the vagina and breasts. . . . We know that lead causes sterility; hence in a condition of the female organism indicating general symptoms of marasmus, when sterility constitutes a prominent symptom, we may recommend lead for sterility, more particularly if this seems to have resulted from, or if it was preceded by, frequent miscarriages, and a cachectic state of the system has been the result of these frequent losses. It is well known that women who live in silver-ore smelting huts become disposed to miscarriages" (p. 342).

Dr. Dudgeon has kindly given me the following reference from Noack and Trinks, under *Plumbum*:—"Labour-like pains in uterus and vagina. . . . Abortion in women is very frequent and habitual when they reside for

some time in the cottages of the lead miners (Silberhütten). Cows, sheep, and goats feeding on forage in the neighbourhood of lead-mines, frequently cast their young prematurely with bloody urine (Sonder in *Caspar's Med. Wochenschrift*, 1836, No. 2)."

The above facts prove very distinctly a marked action of lead upon the uterus. The death of the foetus which we have seen is so frequent might be thought to be the result of poisoning transmitted through the mother, especially as we have seen that the children which are born alive at the full time of mothers who are under the influence of lead die early in life, in far above the average proportion of children. But I do not think that, looking at the facts, this will alone explain the results. Besides the mere death of the foetus, menorrhagia is a very frequent condition in women exposed to the influence of lead, while contraction and labour-like pains are produced. M. Paul and Mr. Baker look upon many of the attacks of menorrhagia as unrecognised abortions. This may be the case in some instances, but it cannot possibly explain the majority, as it would seem to occur in single as well as married women engaged in lead-works, all of whom, according to the statement of one patient, suffered from menorrhagia. Nor would it account for the profuse and long-continued hæmorrhage after delivery. We must then admit that lead has a specific action on the uterus, independently of the poisoning of the foetus. This being the case, though hardly ever, as far as I am aware, prescribed by homœopaths in such cases, it ought to be of value as a medicine. It ought to be useful in menorrhagia, in profuse lochia, and in threatened abortion with or without hæmorrhage, and still more in recurrent abortion. In bearing-down and contractive pains in non-pregnant women it would perhaps also prove useful.

This view of the subject also brings out an interesting point, namely, that allopaths, in prescribing *Acetate of Lead* in menorrhagia, and in threatened abortions with hæmorrhage, are really, without being aware of it, practising homœopathically. *Lead* is a very frequent prescription of theirs in such cases, generally combined with *Opium*.

Although we have several most reliable medicines for these states, yet we are all the better of this addition to our *armamentarium*, and its neglect hitherto is my apology for the present contribution.

EXPERIMENTAL RESEARCHES ON THE NATURE
AND CAUSES OF CATARRHUS ÆSTIVUS (HAY-
FEVER, OR HAY-ASTHMA).

By CHARLES H. BLACKLEY, M.R.C.S. Eng.

(Continued from page 501.)

CHAP. III.—EXPERIMENTS WITH THE PRESUMED CAUSES
OF HAY-FEVER (*continued*).

F. Experiments with Pollen.

§ 122. In the preceding pages I have shown that pollen has been named by many authors as one of the principal causes of hay-fever. No one, however, has, so far as I am aware, put this agent to the test by following out a systematic course of experiments with it.

In the early history of the disease the nature of its cause was enveloped in great obscurity, and those who suffered from it were few in number; it is not, therefore, surprising that greater success should not have attended the earlier efforts made to clear up this obscurity. But when we remember the great differences of opinion which have of late years been held on the causes of the malady, it strikes one as a most remarkable circumstance that authors and patients should have been content to go on theorising upon the cause of the disorder when a few comparatively easy experiments would, so far as pollen is concerned, have set the matter at rest in any individual case. It is, however, much easier to theorise than to try experiments, and

especially when these would have to be tried on the theoriser's own person.

At the present time even, when so much more is known of the disorder than formerly, opinions are very much divided and also, in some cases, very loosely held. In some instances it is assumed that a disease which is as constant in its symptoms as almost any disease we might name owes its origin to causes which are as diverse in their nature as it is possible to bring together. It is, moreover, affirmed by some authors that at one time one of these dissimilar causes, and at another time another, may operate in producing an almost unvarying set of symptoms; or, in other words, that in this disease like effects are produced by unlike and totally different causes. I shall endeavour to show that these opinions are not supported by facts.

§ 123. In the experiments already detailed I have in most instances had to record only negative results. In the observations I am now about to describe we enter upon a very different course. Almost every experiment is followed by a greater or lesser amount of definite and unmistakeable effect which seems to point to pollen as the most powerful if not the only cause of the malady.

In investigating the action of pollen on the respiratory and other organs the questions which presented themselves were :—

1st. Can pollen produce the symptoms of hay-fever?

2nd. Does this property belong to all the pollens, or is it confined to the pollen of some one or more orders of plants? and, if so, to what natural orders does it belong?

3rd. To the pollen of which natural order, or of which species of this order, are the actual attacks of hay-fever, as they occur in early summer, due?

4th. Is this condition or property found in the dried as well as in the fresh pollen?

5th. To what special substance in pollen is this supposed action due?

Some of these questions can be answered more or less satisfactorily: others there are which will still have to be considered *sub judice*.

§ 124. There are certain conditions which are required in the case of any agent which is to be accepted as the exciting cause of hay-fever. These are better fulfilled in the case of pollen than in any of the other agents, the action of which we have already considered.

In the first place, it should be shown that this agent, whatever it may be, will, when brought into contact with the respiratory mucous membrane, produce the symptoms of the disease to which it is supposed to give rise. In the second place, it should be shown that the disorder manifests itself whenever this agent begins to be produced in large quantity.* In the third place the attacks of the disease should lessen in severity as the production of this agent diminishes, and should be entirely absent during those parts of the year in which the latter is not generated.

I shall as I proceed be able to show that these conditions are, in the case of pollen, very closely fulfilled.

§ 125. The first experiments were made with the pollen of the grasses, but the pollens of the plants belonging to thirty-five other natural orders were also tried. The experiments were made at all times of the year. In some cases the dried pollen was used after it had been kept some months, but for the most part this was used during the period in which the plants indigenous to this country were in flower, and whilst the pollen was fresh.†

* I have made "large quantity" a special condition because I shall have to show that the presence of a small quantity will not in some cases produce well-marked symptoms; but this term is, after all, only used in a relative sense.

† Pollen from the following plants was tested during the course of experimentation, viz.:—*Aconitum*; (?) *Helleborus niger*; *Ranunculus acris*; *R. flouaria*; *Anemone nemorosa*; *Caltha palustris*; *Aquilegia vulgaris* (Ranunculaceæ). *Papaver rhæas* (Papaveraceæ). *Fumaria capreolata* (Fumariaceæ). *Cardamine pratensis*; *Nasturtium*; (?) *Cheiranthus cheiri* (Cruciferae). *Viola tricolor*; *V. odorata* (Violariæ). *Silene maritima*; *Stellaria media*; *Agrostemma githago* (Caryophyllæ). *Malva sylvestris* (Malvaceæ). *Hypericum perforatum* (Hypericineæ). *Geranium* (?) (Geraniaceæ). *Ulex Europæus*; *Cytisus scoparius* (Leguminosæ). *Rubus fruticosus*; *Rosa canina* (Rosaceæ). *Crataegus oxyanthus*; *Pyrus* (?) (Pomaceæ). *Cucumis* (?) (Cucurbitaceæ). *Conium maculatum*; *Heraclæum sphondylium* (Umbelliferae). *Sambucus niger* (Caprifoliaceæ). *Scabiosa columbaria* (Dipsaceæ). *Arotium lappa*; *Centaurea*; (?) *Tussilago*

This was tried in five different ways, viz. 1st, by applying it to the mucous membrane of the nares; 2nd, by inhaling it, and thus bringing it into contact with the mucous membrane of the larynx, trachea, and bronchial tubes; 3rd, by applying a decoction of the pollen to the conjunctiva; 4th, by applying the fresh pollen to the tongue, lips, and fauces; 5th, by inoculating the upper and lower limbs with the fresh moistened pollen.

§ 126. The incident related at § 71 seemed to indicate that the pollen of grass was at least one of the causes of hay-fever. Subsequent experiments confirmed the results of this accidental trial, and furnished an answer to the first question given above.

The pollen of a number of the grasses was first tried, and in every one of these trials these gave distinct and unmistakable evidence of its power to disturb the healthy action of the respiratory mucous membrane. When a small portion of pollen, just sufficient to tinge the tip of the finger yellow, was applied to the mucous membrane of the nares, some of the symptoms of hay-fever were invariably developed, the severity and continuance of which were dependent upon the quantity, and upon the number of times it was used.* In an experiment made with the

farfara; *Senecio vulgaris*; *Chamomilla matricaria*; *Calendula officinalis* (Compositæ). *Campanula rotundifolia*; *C. hederacea* (Campanulacæ). *Vinca minor* (Apocynæ). *Convolvulus sepium* (Convolvulacæ). *Cynoglossum officinale* (Boraginæ). *Solanum dulcamara*; *Atropa belladonna*; *Solanum tuberosum* (Solanæ). *Veronica chamædrys*; *Euphrasia officinalis*; *Digitalis purpurea*; *Linaria cymbalaria* (Scrophularinæ). *Mentha piperita* (Labiata). *Primula vulgaris* (Primulacæ). *Plantago major*; *P. lanceolata* (Plantaginæ). *Polygonum persicaria*; *Rumex* (?) (Polygonacæ). *Euphorbium* (?) (Euphorbiacæ). *Betula alba*; *Castanea vulgaris* (Amentacæ). *Urtica urens* (Urticacæ). *Arum maculatum* (Aroidæ). *Tulipa* (?) (Liliacæ). *Hyacinthus non-scriptus*; *Allium ursinum* (Asphodeleæ). *Iris pseudacorus* (Iridæ). *Narcissus pseudo-narcissus* (Amaryllidæ). *Anthoxanthum odoratum*; *Alopecurus pratensis*; *Aira cæspitosa*; *Poa pratensis*; *P. nemoralis*; *P. trivialis*; *Lolium italicum*; *Triticum sativum*; *Secale cereale*; *Hordeum distichum*; *Avena sativa* (Graminacæ). *Eriophorum vaginatum* (Cyperacæ). The pollen of a number of exotic flowering plants, the names of which I had no time to ascertain, was tried and yielded results much like the average of those given above.

* In the dried state 1700 pollen grains of *Lolium italicum* were not heavy

pollen of the *Lolium italicum** the first sensation produced was that of a very slight degree of anæsthesia of the spot to which this had been applied. This was followed by a feeling of heat which gradually diffused itself over the whole cavity of the nostril, and was accompanied by a slight itching of the part. After some three or four minutes a discharge of serum came on, and continued at intervals for a couple of hours. The mucous membrane appeared to swell, and eventually became so tumid that the passage of air through the nostril was very much impeded. No sneezing occurred in this case, but this might partly be accounted for by the circumstance that the quantity of pollen applied was very small, and probably also by the fact that this was not fresh when applied.

§ 127. In another experiment tried with the pollen of the *Alopecurus pratensis* the lining membrane of one nostril was charged with the pollen by this being rubbed on with the point of the finger as far as this would reach. About one fiftieth of a grain was applied. Similar sensations to those described above came on, but showed themselves more rapidly than in the former case. In a few minutes a violent attack of sneezing came on; there was also a profuse discharge of serum, which continued for some hours, gradually diminishing towards the latter part of the time. In two hours after the experiment had commenced the mucous membrane had become so much swollen that no air could be drawn through the nostril in any attempt at inspiration.

§ 128. The pollen of *Secale cereale* (Rye) produced symptoms of a much more severe and lasting character than either of the grasses named above. In one experiment made with this pollen the sternutations were very strong and long-continued; the discharge from the nostrils was very copious, and produced more excoriation than in either of the other cases. The pollen was taken fresh from the growing plant, and applied on the spot, consequently I

enough to depress the scale of a balance which turns easily with the $\frac{1}{100}$ th of a grain, yet this small quantity produced a very perceptible effect when applied to the mucous membrane.

* A cultivated variety of *Lolium perenne*.

could make no attempt at ascertaining what was the exact quantity applied, but from the circumstance that fresh pollen is much more adhesive than it is when dried it is very probable that the quantity would be considerably larger than was usually applied.

The day on which the experiment was made was warm and moist, just such a day as would bring the pollen rapidly to maturity. A profuse coryza came on in less than a minute after the application. In thirty minutes the nostril was completely occluded, so that it was quite impossible to pass any air through it. During the day the sneezing and coryza kept up at intervals, and lasted for six or eight hours. During the night the nostril which had been closed became patent again once or twice, but curiously enough the nostril to which no pollen had been applied became almost entirely impervious to the passage of air,* and on each occasion violent attacks of sneezing came on. The experiments with *Rye* (*Secale*) were tried several times, and were always marked by decided symptoms varying in intensity according to the quantity of pollen used; having a milder character if this was used dry instead of fresh. The attack induced by three applications, at intervals of an hour, lasted twenty-four hours, and could even then be scarcely said to have cleared away. The discharge of the serum gradually altered by the latter becoming more inspissated, taking in this respect pretty much the same course as is seen in a case of ordinary catarrh, namely, by the discharge gradually changing to a puriform mucus, which, under the microscope, was seen to be much of the same character as that of the subsiding stage of ordinary catarrh.

§ 129. The action of the pollen of *Triticum* (Wheat) comes very near to that of the *Secale*, but is perhaps not quite so severe, nor does it develop its symptoms so rapidly. It is, however, quite as severe as that of any of the grasses, and seems to have a tendency to produce a more lasting impression than many of these.

In the case of the *Avena sativa* (Oats) the action seemed

* The cause of this phenomenon I shall be able to explain further on.

much the same as in some of the larger grasses. In two experiments which were tried with the pollen of this cereal, it was, as in the case of the *Secale*, applied whilst quite fresh. The symptoms were more severe and more rapidly developed than they are found to be when the plant is gathered and the pollen collected and dried before using.

Only one experiment was tried with the pollen of *Hordeum distichum* (Barley), consequently I am not able to speak so decidedly upon it as in the case of the other cereals and grasses. Nevertheless, its action was sufficiently well marked to show that it had, in common with the pollen of all the plants tried belonging to the order Graminaceæ, the property of disturbing the normal action of the respiratory mucous membrane in hay-fever patients.

In an experiment which was tried with the *infusion* of the pollen of the *Lolium italicum* the action was very distinct so far as it went, but in this case there was no sneezing, and only a very small quantity of serum was discharged. The most marked effect produced was the tumefaction of the lining membrane of the nostril. This was slow in developing and equally slow in disappearing.

§ 130. The pollen *Lolium italicum* was used with patient No. 6 on two occasions in a manner similar to that described above. In both experiments symptoms much the same as those produced in my own case were developed. There was sneezing, discharge of serum, and partial occlusion of the nostril.

One experiment was also tried on patient No. 7 with the pollen of *Alopecurus pratensis*. Profuse watery discharge from the nostril followed the application. There were also several attacks of sneezing in the course of a few hours, but in this case the occlusion of the nostril was not so marked as in my own.

In neither case was the patient aware of the nature of the substance used nor yet of the object of the experiment. In both instances the dried pollen was used. This was applied on one occasion early on in the year before the grasses were in bloom, and on the other in the early part of winter when few plants are in leaf.

The action of the pollen of the order Graminaceæ was on the whole very distinct and well marked. In some cases it was comparatively mild, and in other cases, as we have seen, somewhat severe.

§ 131. In the case of plants of some of the other natural orders the action was quite as well marked as in any of the grasses. The pollen of the *Corylus avellana*, for instance, developed its symptoms moderately rapidly. A small portion of this pollen was applied to the mucous membrane of one nostril in the usual manner. In ten minutes a violent fit of sneezing came on, and was quickly followed by a copious discharge of thin serum. There was a sensation of heat in the nostril, and there was also some oppression of the breathing, which in this case must have been caused by reflex action, as there was, so far as I could judge, no pollen inhaled. Why this should be the case with some pollens and not with others I am at present unable to say. I am, however, satisfied that asthmatic symptoms may be brought on as hay-asthma by reflex action. The mucous membrane of the nares was so much swollen in fifteen minutes after the pollen was applied that the air was with difficulty drawn through the nostril, and in thirty minutes the occlusion was complete.

The pollen of the common Tulip brought on a profuse discharge of serum, and produced great tumidity of the mucous membrane, but singularly enough there was no sneezing until the middle of the night, twelve hours after the experiment had commenced.

§ 132. One experiment was tried by using two different pollens for the two nostrils in as nearly the same quantity as possible. One nostril was charged with the pollen of the *Tilia media* (Linden tree), and the other with the pollen of the *Jasione* (Sheep's bit). The latter was the quickest in its action and also apparently the most powerful. There was first a sense of pressure in the nostril combined with slight itching; then swelling of the mucous membrane—or to speak more correctly of the sub-mucous cellular tissue—and discharge of serous fluid. The latter was most profuse in the nostril to which the pollen

of *Jasione* had been applied. The symptoms produced were much the same as those brought on by the use of the grass pollens, and the circumstance of both nostrils being affected at the same time did not seem to have any influence on the rapidity with which the symptoms disappeared. A walk in a park, where there were a great number of linden trees in bloom at the time, invariably brought on a smart attack of sneezing and coryza with me, and also seemed to have a tendency to produce more decided asthmatic phenomena than I have ever noticed when the grasses have been in flower.

§ 133. The inhalation of pollen, without permitting this to pass in any quantity through the nostril, generally brought on asthmatic symptoms, and in these cases only could I say that constitutional symptoms were developed. An example of this occurred accidentally in the month of March before any of the grasses were in flower.

In preparing the pollen of one of the Amentaceæ for the microscope a considerable quantity was accidentally inhaled before I was aware that it had been thrown off from the catkins so abundantly. A violent attack of sneezing came on in a few minutes, but this was not by any means so violent nor yet so persistent as I should have expected from the quantity of pollen which seemed to have been disengaged. Later on there was a moderately copious discharge of thin serum which kept up for some hours. After the sneezing and coryza had continued for a couple of hours the breathing became very difficult as if from constriction of the trachea or bronchial tubes, giving me just a slight experience of the misery those have to endure who suffer severely from the asthmatic form of hay-fever. In the course of five or six hours I began to have aching and a sense of weariness over the whole body, with pain in the head and spinal column. A very restless night was passed; the pulse rose from its usual number (68) to 100. Occasionally there was slight cough with expectoration of thin frothy sputum, and for twenty-four hours I felt as if passing through an unusually severe attack of influenza. During the succeeding night a violent perspiration set in, and as

this proceeded I began to feel more easy. The pain in the head and the sense of weariness gradually abated, and at the end of the second day I was fit for duty again. In the early part of the attack, as I have mentioned above, there was expectoration of frothy sputum. This gradually changed to a thick puriform mucus, which was during the latter part of the time brought up for the most part by the mere effort of hawking.

§ 134. Another occurrence of a similar character took place whilst preparing some of the pollen of *Alopecurus pratensis* for the microscope. In this case the symptoms were similar to those given above, but not by any means so severe, and in addition to the symptoms detailed above partial loss of voice came on accompanied by a sense of irritation just behind the pomum adami.

One experiment of a similar kind was voluntarily tried with pollen of *Lolium italicum*. A very small quantity was inhaled and was not allowed to pass through the nostrils in the process. No constitutional symptoms were developed. The only symptoms were difficulty of breathing, as if from narrowing of the bronchial tubes, with slight cough and expectoration of frothy sputum. Probably the mildness of the attack was due to the smallness of the quantity of pollen inhaled.* The suffering and inconvenience caused by the former experiment prevented me from carrying this latter to such an extent as to run the risk of producing as much disturbance as in the former case; it was, however, carried sufficiently far to show that the possibility of inducing the most violent asthmatic symptoms was only a question of quantity.†

§ 135. With the view of ascertaining what would be the effect of a watery and spirituous extract of pollen when applied to the nostril an infusion of that of the *Lilium tigrinum* was made. The infusion was filtered, and proof spirit was added to the residuum, and this was again filtered

* Whether the constitutional symptoms in the first experiment were caused by the pollen I am not at present prepared to say, but I have no reason to doubt that they were.

† Probably also to some extent a question of *kind* of pollen.

after macerating for a few days. The two filtrates were evaporated to a syrupy consistence and mixed together. A portion of this mixture was applied to the lining membrane of the nostril in the same manner as the pollen had been applied. No effect which could be attributed to the application followed. The pollen of *Lilium* is not very active when applied fresh, but still it is sufficiently so to show that it possesses the property of deranging the action of the mucous membrane of the air-passages.

§ 136. A decoction of the pollen of *Gladiolus* was made by boiling a portion of this with one hundred times its weight of distilled water. One drop of this liquid was placed in contact with the conjunctiva of the right eye. The effect was almost instantaneons. The first sensation was that of intense burning and smarting, coupled with a feeling such as might be imagined to be caused by fine sand being blown into the eye. The photophobia was so severe that for some minutes the eye could not be opened for more than a single second at a time. In about thirty seconds the capillary vessels of the conjunctiva were seen to be greatly distended. With the aid of a lens the larger vessels of the conjunctiva could be seen to be raised above the surface. Movement of the eyeball gave great pain, just as is felt when dust has been blown into the eye. In six minutes the conjunctiva had become quite œdematous, and showed the circular line of attachment around the outer margin of the iris. The œdema increased until very severe chemosis was set up. The eyelids also became much swollen. In two hours after the fluid had been applied the smarting and burning had much abated, and the congestion of the conjunctival vessels had considerably lessened, but the chemosis remained and was even more marked than it had been an hour before. There was a moderately copious discharge of fluid from the eye and also some little from the nostril. In six hours the eye still felt uneasy, but there was very little pain on moving the eyeball, although the vessels of the conjunctiva were still injected. The chemosis still remained as severe as before. In eighteen hours there was scarcely any congestion of the vessels remaining, but the chemosis was still

very distinct. In thirty-two hours all traces of the derangement had disappeared. During the course of this experiment no effect was produced on the sclerotic coat of the eyeball, nor yet, so far as could be seen or felt, on the deeper structures. The action seemed to expend itself upon the conjunctiva and upon the cellular tissue of the eyelids.

§ 137. One grain of the pollen of *Alopecurus pratensis* was applied to the fauces for the purpose of ascertaining if it would have any effect upon the tonsils and upon the mucous membrane of the mouth and fauces. Slight itching came on in the course of a few minutes, and in half an hour the mucous membrane of the fauces was seen to be somewhat congested, but this was more seen in the engorgement of the larger capillaries than in a diffused redness. The itching was quickly followed by a sensation as if some hard angular bodies stuck in the throat. There was also a feeling of constriction about the fauces, although there was no swelling perceptible on examining the throat. I am, however, convinced from subsequent experience that the sense of constriction was caused by œdema of the sub-mucous cellular tissue of the pharynx. The symptoms remained stationary for two or three hours and then gradually diminished.

§ 138. Whilst I was still suffering from my usual attack of hay-fever, during the summer of 1865,* as much pollen as could be obtained from two anthers of the *Lolium italicum* was applied to the centre of the anterior surface of one forearm after the skin had been abraded as in the ordinary mode of vaccinating. A space of about a quarter of an inch was abraded, and to this the quantity of pollen named was applied after being placed on a piece of wet lint the size of the abrasion. This was covered with a piece of thin gutta percha, and the whole was held in position by a strip of adhesive plaster. The centre of the other forearm was treated in exactly the same manner save and except that no pollen was applied to it. The scratching with the lancet raised a wheal such as is seen in urticaria or in stinging

* July 13th.

with nettles. In a few minutes after the pollen had been applied the abraded spot began to itch intensely ; the parts immediately around the abrasion began to swell, but this was not apparently due to any action on the *cutis vera*. The swelling seemed to be entirely due to effusion into the subcutaneous cellular tissue. There was no heat or redness more than what had been caused by the abrasion, and these were much the same on each arm. Although the swelling had the appearance of œdema, it located itself at first exactly around the abrasion to which the pollen had been applied, and gradually spread from this point and formed a flattened tumour, which had its centre at the abraded spot. There was no tenderness, and any part of the swelling was easily made to pit on pressure. The wheal caused by the lancet did not increase much in size after the pollen was applied. The tumour increased in size until it measured two and a half inches in length, by one inch and a half in breadth, and was raised above the ordinary level of the surface nearly three quarters of an inch. No pain was felt in the limb, nor was there any heat or redness present at any time, beyond the very slight amount to which the abrading of the cuticle gave rise. The swelling attained its maximum in six hours, and then remained stationary for other eight hours ; after this it gradually subsided, and in forty-eight hours it had entirely disappeared. The arm to which no pollen had been applied did not exhibit any sign of swelling or irritation.

§ 139. In the latter part of September in the same year another experiment of a similar character was tried. Much the same effect as was seen in the former one followed, but the swelling was not so great, and subsided more readily.

In the following year the experiment was repeated by applying the pollen to the integument covering the centre of the right tibia. This spot was chosen because we have here only skin and cellular tissue, with a hard unyielding surface of bone behind these ; consequently, whatever increase there is in the bulk of the part operated on it must declare itself more distinctly and more exactly than it is

possible for it to do when there are soft structures underlying it. The same conditions were observed as in the experiment described above. When the pollen had been applied for a few minutes, the same intense itching of the part came on, as in the former case, but owing probably to the circumstance of the pollen being larger in quantity, this was more severe, and longer continued.

In about fifteen minutes after the experiment commenced the limb began to swell, and gradually increased in size, until the tumour measured four inches in length by two inches in width, whilst the centre was raised quite three quarters of an inch above the surface of the bone. The increase in the size of the tumour was much slower than in the other cases named. The swelling attained its maximum in about twelve hours, after which it remained stationary for other twelve or fourteen hours. At the end of four days the limb had assumed its usual size and form, the derangement having entirely passed away. During the time the tumour had somewhat changed its location, as if the fluid contained in the subcutaneous cellular tissue had gravitated; the portion of the swelling which disappeared last being quite an inch below the spot where the pollen had been applied. There was neither pain, heat, nor redness present during the whole time.

§ 140. The examples of the action of pollen given above have been selected from a large number. These give a fair idea of the nature and extent of this action. In most cases the experiments were repeated several times, and in some instances, where the details were not noted down at the time, the action was quite as severe as in those I have cited.

Extending as they do over a good number of years, the trials must have been made in very varied physical and mental conditions, even within the limits of health; but some of the experiments have been made when the system has been in a state bordering upon, if not actually suffering from, disease. I have not found, so far as I have noticed, that these varying conditions have had any influence in increasing or diminishing the intensity of the symptoms

produced. It is, however, only right to say that my attention has not been specially directed to this point.

§ 141. With some rare exceptions, the action of the pollen of the plants named in the list given was sufficiently perceptible to show that this action was in some degree common to all. The intensity of the symptoms, however, varied in using the pollen of different plants, and also in that from the same plant at different times. There seemed, in fact, to be some circumstances which had a controlling or modifying influence upon the production, as well as upon the activity of pollen. Probably some of these modifying influences consist in those subtle atmospheric conditions, of the nature of which we are at present profoundly ignorant.

There are, however, some circumstances which exercise an important influence, and with whose nature we are tolerably well acquainted. One of these is temperature. A high temperature is in itself favorable to the generation of pollen, but a high temperature with severe drought will, in the case of the grasses, check their growth, and thus prevent the formation of pollen. In proportion as temperature and moisture are suitably combined, so will be the production of pollen, but where these happen to be unusually favorable, we may have the grass arriving at maturity rapidly, and as a consequence this may be quickly cut and converted into hay and housed. Under such circumstances, hay-fever patients may have a short season of attack, but the symptoms may be very severe whilst they last.

§ 142. Low temperature operates in quite another manner with the majority of the grasses. Growth may go on moderately well with a comparatively low temperature, especially in *some* of the grasses, but a temperature below a certain point will not permit the flowering process to go on in a normal manner. Not only will the quantity of pollen thrown off by a given number of plants be lessened, but that which is generated will have much less vigour than it has in favorable seasons. In the same manner pollen obtained from plants which have flowered prematurely does

not seem to possess the same activity as that which is generated later. The pollen of *Bellis perennis* (common daisy) gathered during the earliest period of flowering, in the month of March, did not seem to have the same power as that which was gathered in the middle of summer, although this plant blooms from early spring to autumn.

Some of the cereals, however, will arrive at maturity, and maintain a vigorous and healthy condition during their period of growth, with a much drier state of the atmosphere and soil than is borne by many of the grasses. It is well known that wheat will thrive and do well with much less moisture than the grasses need. Thus it happens that in cold and wet summers hay-fever patients will suffer much less than in other and better seasons ; whilst in a very hot summer, with continued drought, patients may almost escape the disease, even if they reside in a part of the country where hay-grass is largely cultivated. But when the cereals come to be in flower they may suffer very severely for a time. I think it was in this way that the attacks which Bostock had in the Isle of Thanet might be accounted for, and the mistake he made was in supposing that grass in flower was the only thing, besides heat, which could bring on the disorder.

Overgrown pollen also seems not to be as active as that which is fresher and younger. The pollen of *Atropa belladonna* (deadly nightshade) showed very little activity when gathered whilst the plant was in fruit, and when only a few half faded flowers were found on it. From the fact, however, that the pollens of *Solanum dulcamara* and *Solanum tuberosum* have only a mild action, it is probable that the pollen of *Belladonna* will not be found very active even when gathered at the height of its flowering season. This, however, cannot be considered a settled point ; but certain it is that in some cases pollen is not as active when gathered too early or too late, as it is when taken at the middle of the flowering season ; and whatever circumstances interfere with the usual course of this process, these will alter the quality of the pollen produced.

Whatever may be the nature of the influences which

modify the activity or power of pollen in producing hay-fever, this power was always present in a greater or lesser degree in all the plants experimented with. In some it was so mild that it was necessary to repeat the experiment several times and under varying circumstances in order to be certain that it was present. In others the action was both rapid and vigorous, and such that, if continued, would have led to severe suffering, if not to dangerous symptoms.

§ 143. Before proceeding to inquire what particular portion of the pollen grain or what particular substance contained in this is the active agent in producing hay-fever, it will perhaps be well to notice some conditions which seem to have no influence whatever either in bringing on the attacks or in modifying their intensity when once produced.

The pollen grains of different orders of plants vary much in size and in weight; in some cases not being more than one twentieth the size or weight of others. This circumstance, however, does not directly affect their power of producing hay-fever. The size of the pollen grain has no relation to the intensity of the symptoms.* A large pollen grain may produce a mild attack, whilst a smaller one may produce much more severe derangement.

Pollen grains also vary in shape and in the roughness or smoothness of their outer coat. In the state in which the pollen comes to be when in contact with the mucous membrane of the nares, or of the trachea and bronchial tubes, in some cases the outer coat will be perfectly smooth and even,† such as for instance in the cereals or the grasses. In others the surface is studded over with sharp points, as in the order Compositæ; and whatever may be the varying conditions such pollen is placed under, with regard to excess or deficiency of moisture, this roughness is never entirely got rid of. Between these two extreme characters of surface there are all degrees, but in no case have I noticed that the shape or the degree of roughness of the surface has

* When equal quantities by weight are applied.

† So far as a magnifying power of 220 diameters will show.

any influence in regulating the intensity of the symptoms produced.

We have seen that in the list given there are several plants of a poisonous character. This circumstance, however, seems not to affect the quality of the pollen, so far at any rate as the production of hay-fever is concerned. In one of the most poisonous families (Solanæ), the pollen produced even milder symptoms than that of the grasses.

§ 144. In commencing the inquiry into the question as to what constituent of pollen is the exciting cause of hay-fever, we encounter some difficulties which are not easily removed. Whatever part the pollen-cell may take in the generation of the varied and beautiful forms of plant life, even with the aid of the most powerful instruments it gives to the vegetable physiologist no indication of the possession of those wonderful powers which belong to it and which lie hidden in its apparently structureless granular matter.

In like manner, if we seek for any sign of pollen being the cause of this disease, we find no special character in any portion of the cell which will enable to account for the phenomena of hay-fever.

Pollen is, in its recent state, a living structure, and if we attempt to examine it by chemical means or by any mode of manipulation which alters the relation of its separate parts, we may change its character and lessen its vitality. It will no longer be the active and living organism it was before our examination began, and the changes we have brought about may involve the alteration or destruction of those very qualities upon which the production of morbid phenomena depends. These may and probably do to some extent depend upon the vitality which it possesses in common with all bodies of the same class; consequently in all our examinations we are constantly met by the difficulty there is in keeping intact the disease-producing quality whilst we isolate that portion of the pollen to which it belongs.

There are, however, some of the symptoms of hay-fever which are probably due to mechanical causes, but which are nevertheless produced by pollen.

§ 145. Examined under the microscope a pollen cell is a simple cell with granular contents. Its cell wall is generally composed of two layers only—an *intine* or inner coat and an *extine* or outer coat.* In addition to these two membranes pollen of almost all kinds is covered with a substance which resembles an oleo-resin, and in some instances I have thought that a portion of this substance has come from the interior of the pollen grain. In some cases this oleo-resin is of a rich amber colour, when seen under the microscope, whilst in others it is of a pale straw colour. It varies in quantity in different pollens. In that of *Lilium album* it is especially abundant and also in that of the *Calendula officinalis*, as well as in several other plants of the order Compositæ. It is very little soluble in water or in proof spirit, but dissolves readily in ether and in oil of turpentine. I believe it slowly volatilises at ordinary temperature, but of this I cannot speak very positively. It is this substance which causes pollen to adhere to any body with which it comes in contact when fresh.† Some of the pollens which have the largest quantity of this oleo-resin about them are not so active in producing hay-fever as those which contain much less of it. All the pollens of the grasses that I have examined have a very small quantity about them, and yet they are amongst the most active in producing the symp-

* There is also a third coat in some pollens. In the pollen of the *vegetable marrow* I have found this third coat to consist of an exceedingly delicate membrane which seems to lie between the inner and outer coat. It is not seen until the pollen has been immersed in water, or in water and glycerine, for two or three days; it then protrudes and lifts up the minute lids which cover the pores in this pollen grain.

† Referring to this subject Lindley says:—"In all cases where there are asperities of the surface or angles in the outline, pollen is asserted by Guillemin to have a mucous surface, which was first observed in Proteaceæ by Brown; but Mohl finds that the presence of mucosity upon pollen is a constant character, at least, when the grains first quit the anther; and that a power of secreting a viscid substance is one of their functions when perfectly smooth, as well as when covered with points and inequalities. He, however, admits that bispid pollen is generally more viscid than that which is smooth." This supposed *mucus*, or *mucosity*, is really the *oleo-resin*, which I have described above. Any substance of the nature of mucus would not be soluble in oil of turpentine,—C. H. B.

toms of hay-fever. From these considerations I conclude that this body has little or no influence on the disease, and that we must look to some other portion of the pollen grain for the exciting cause of the malady.

§ 146. Seen under the microscope, when fresh, pollen grains have generally a definite and regular shape. When dried they usually also shrink into a shape more or less regular in character, but in a certain portion of the contents of any anther the dried grain will have the appearance of an amorphous particle of silica. The outer coat is seen to be pierced with small round holes or slits—pores. The inner membrane stretches across these openings and never in any case, so far as I have observed, allows the interior of the cell or its contents to be exposed to the atmosphere until this membrane has been ruptured.

§ 147. When water* is allowed to come into contact with the dried pollen this quickly swells and assumes its normal shape, and if the quantity of moisture is not too great it will retain its natural shape for a considerable time. If moisture continues to be supplied it loses its form, and whatever may have been its shape previously it tends to become more or less spherical. Carried still further, the granular contents of the cells are seen to alter their position; the inner membrane is seen to protrude more or less through the pores, and to form in this way minute mastoid processes, in some cases bulging very considerably beyond the outer coat. The granular contents will, in the case of the pollens of the grasses, move to the end at which the single pore is, in this pollen, situated, leaving one third or one half of the cell comparatively empty. After a short time, varying according to the condition of the pollen, when placed in contact with water the portion of the granular contents of the cell which is nearest to the pore is expelled with great force—so much so that I have frequently seen a pollen grain driven by degrees half way across the field of the microscope by this expulsive force.†

* Other fluids will also act in a similar manner, but none more quickly than water.

† Lindley says this enlargement and bursting of the pollen grain is the

After the granular matter has escaped it diffuses itself gradually in the surrounding fluid. When carefully observed the granules are seen to vary very much in size. According to Lindley they are, in some cases, not more than the 30,000th of an inch in diameter.* Immersed in water or in any fluid which is not of greater density than water they readily take on molecular motion.† This is most perceptible in the smallest granules and seems to be more sluggish as the granules get larger, until in some pollens we find the largest granules to have little or none of this movement. It is said to be caused by the particles "moving on their axes," and setting up in this way a sort of rotary motion. A little observation will, however, show that in addition to this they have a vibratory motion, and that they slowly move in different directions, and sometimes I have noticed that they seem to work together in small groups as if attracted by some force which operates to bring certain of the granules together.‡

effect of *endosmosis*. This, however, would not force the granular matter to one end of the pollen sac unless some special apparatus adapted for this purpose was within the sac. This I believe to be the case. I have on three occasions seen what appeared to me to be a small membranous sac within the inner coat of the pollen grain, and situated at the end farthest from the pore. When the water has been applied for some time, this sac has been seen to expand gradually, and by this expansion to force the granular matter against the end where the pore is situated. After the inner membrane is ruptured and the granular matter has begun to escape the sac seems to collapse or in some way to become so indistinct as not to be seen. I have seen it twice in the pollen of grass and once in that of geranium; but although I have carefully watched for the phenomenon on several other occasions, I have not been able to detect it except in the three instances named. When it is seen it is perceptible for a very short time only.

* Lindley's 'Introduction to Botany,' p. 190.

† This was first noticed by Gleichen, and subsequently by Amici, Guillemin, and Brown.

‡ Molecular motion is seen in almost all animal or vegetable fluids if these are not too viscid to permit the molecules to move. Granular matter is especially abundant and active in vaccine lymph if taken on or before the eighth day, and also in the fluid thrown off by perspiration in rheumatic, typhoid, and other fevers. It is also seen, but to a less extent, in the perspiration of health.

Any watery fluid which holds in suspension finely divided mineral or earthy matter will also show the molecular movement, but in this case I have never

§ 148. With a solution of *Iodine* the granular matter becomes dark blue, showing by this change that it is an *amyloid* substance. The finer particles colour, in proportion to their size, more deeply than the larger particles. Immersion in antiseptics has no influence on the motion unless these are strong enough to destroy the form of the granules.

Boiling the granular matter, also, does not interfere with the motion, but any liquid more viscid than water lessens the extent of the motion in proportion to the density or viscosity of the liquid. Immersion in glycerine will almost entirely put a stop to it.

§ 149. If instead of bringing the pollen grain into direct contact with water we allow the *vapour* of water to act upon it, the changes described above occur much more slowly. We reproduce, in fact, the condition in which pollen is placed when it is brought into contact with the respiratory mucous membrane by being inhaled, and we are with suitable appliances* able to watch changes such as occur when pollen is brought into contact with the mucous membranes. In the one case, however, we have *mucus* and *watery vapour* acting upon the pollen, whilst in the other

found it to be as vigorous or as long continued as it is when the granular matter has been derived from animal or vegetable bodies.

* One of the readiest methods of observing the changes which occur in pollen under the influence of watery vapour is to use what may, for convenience, be called a *water cell*. This may be constructed in the following manner. A drop of distilled water is placed, whilst still warm, in a microscopic cell about one tenth of an inch deep by half an inch in diameter. The upper surface of the cell wall is coated with black varnish, and this is allowed partially to dry. A disc of thin microscopic glass, on which a portion of fresh pollen has been dusted, is then placed on the cell in the form of a lid, care being taken that the surface on which the pollen has been placed is turned downwards. If the varnish has not been allowed to become too dry the edges of the thin glass will adhere to it, and thus a cell will be formed which is hermetically sealed. Care should also be taken that the water is only just sufficient to cover the bottom of the cell.

If it is desired to have the pollen as a permanent preparation after the changes have taken place in it, it is necessary to leave two or three spaces on the surface of the cell wall untouched with varnish. The water will then slowly evaporate, and the changed condition of the pollen is rendered permanent.

we have only watery vapour present. Nevertheless, from what I have been able to learn, by immersing pollen in liquids of similar density and viscosity to those of mucus it is very improbable that the latter has any effect which would not be produced by the vapour given off from the lungs during expiration.

§ 150. The pollen which is found floating in the atmosphere during the prevalence of hay-fever is, as I shall have to show further on, dry and shrivelled. A few minutes' exposure to the air and the sun on a summer's day is sufficient to deprive it of the moisture which it contains in its normal state whilst enclosed within the walls of the anther. If we imitate this action by allowing the pollen to dry before we subject it to the influence of the vapour, we can then observe all the physical changes which take place when pollen is inhaled during the hay season.

One of the first changes is that the pollen grain begins to swell. In contact with water this is accomplished somewhat suddenly, but under the influence of vapour it is produced more slowly and with accompaniments which may possibly help us to account for some of the phenomena of hay-fever.

The enlargement is not brought about by a steady increase in size. The change is produced in many of the pollen grains by a series of jerks, as if the cell wall resisted for a time the expansive force of the moisture which had condensed around it and then suddenly gave way, and in doing so not only altered its shape but also slightly changed its position. After this change has taken place the granular matter begins to escape, but it does so much more slowly and less vigorously than when in contact with water, though occasionally this sudden and spasmodic mode of exit is seen. In both cases the granules have a tendency to diffuse themselves in the surrounding fluid, but with water they do so more completely than with vapour. In both instances a portion of the matter will remain in contact with the granules, grouped together as if held in close contact by the viscid mucus in which they are imbedded whilst in the pollen grain; the least mechanical disturbance, however,

will set a large number of them free, and in such case they at once commence the molecular motion. If placed under a micrometer* whilst the granular matter is being ejected the pollen grain is seen to move slowly across the field of the microscope, or if this remains stationary the stream of granules is pushed in an irregular or zigzag line away from it. In some cases a dozen or more pollen grains may be seen undergoing this change in a single field, and when we consider that in the space of one square centimètre ($=\frac{10}{25}$ ths of an English inch) we have about six hundred fields, some idea may be formed of the extent of mechanical action which goes in a comparatively small surface of the mucous membrane of the nares, trachea, and bronchial tubes, during the time when grass is in flower.

§ 151. When pollen is immersed in water, if it is quite ripe some of the grains will burst in a few seconds, others will take an hour or two, and some will not discharge their contents however long they are kept moist. Sometimes the granular matter is seen in close contact with the pollen sac from which it has been ejected. In some instances the whole of the contents of a pollen grain may be discharged at a single stroke as it were, whilst in others it may go on more moderately; in the latter case it sometimes happens that a large group of granules may partially block up the pore, whilst smaller detachments are being discharged by pushing their way past the side of the larger group.

As in the other case the pollen grains are sometimes seen to move across the field of the microscope, or to push the granular matter from them, but this they generally do more suddenly and more vigorously than when under the influence of vapour only. The granular matter diffuses more rapidly in the surrounding fluid, and the number of granules which take on molecular motion is larger, whilst at the same time this is more vigorous; but the chief difference between the two modes of operating consists in the difference in the time taken to accomplish the results.

(To be continued.)

* The squares on the micrometer enable us to measure the distance travelled however small, and however slow the movement may be.

HOMŒOPATHIC JOURNALISM IN AMERICA.

(Concluded from page 445.)

But besides these monthly journals, the "Great West" needed its own quarterly: and this it obtained in 1865, in the shape of the *United States Medical and Surgical Journal*. It was published at Chicago, and edited by Dr. G. E. Shipman. In his introductory address he stated that the character with which he should endeavour to impress it would be "a Hahnemannian character." And this he explained as follows:—"He will seek to make it a journal of progress, upholding nothing false because it is old, and has received the sanction of all past ages,—embracing nothing false because it is new, however endorsed by great names, or bedecked with specious pretences; seeking to imitate Hahnemann in his patient yet vigorous search for truth, in his casting off all trammels of caste or custom which could impede his progress, but not feeling at all bound to believe everything which he believed, or to do everything which he did. The homœopathic school is not *done* yet—not ready to be fenced in and covered over—not ready to devise a bed on which every one must lie, nor set up a standard to which every one must conform. That which she demands—freedom of thought and action—she should be ready to grant, and welcome all as fellow-workers in the great labour of improving the science and art of medicine, who sincerely devote themselves to this holy purpose."

The *United States Medical and Surgical Journal* did not belie its name or its prospectus. It gave every quarter some 112 pages, good in type and paper (a rare excellence in transatlantic publications), and full of valuable material. Its reviews were especially excellent. With the commencement of the third volume the specially western character of the journal seems to have ceased; and we find the names of Drs. Dunham, Wells, and Allen, of New York, associated with those of Chicago and St. Louis men in Dr. Shipman's staff of coadjutors. As may be supposed, its excellence

was not impaired by this combination, though the later numbers hardly sustain the promise of the earlier. With the sixth volume we find that another change has come over the *personnel* of the journal; Dr. Shipman has retired, and Drs. Small, Ludlam, and Danforth, all of Chicago, appear on the title-page as its "Editors and Proprietors." Dr. Ludlam's contributions have been amongst its prime features hitherto: and we are glad that he retains his interest in it.

From the No. for October, 1870, we make the following extracts.

Dysentery. By PROFESSOR C. C. SMITH, M.D.

I wish to offer briefly some clinical experience in relation to the disease we term dysentery. At the same time I will call attention to the characteristic or "key-note" symptoms of some of the more important drugs used in this disorder; these characteristics having been found by me true guides to their proper application in disease. Among homœopathic physicians there is, I am sorry to say, a great tendency to routine treatment, and this is especially noticeable in the disease now under consideration. There is, we all know, a large number of remedies which produce a condition similar to dysentery, and it is, therefore, a very grave error, to say the least, for any physician to select out of this long list of agents two or three drugs, and make them do duty in every case of this disease to which he may be called.

Yet this, I am sorry to say, is of frequent occurrence. Consequently, many are the painful failures, bringing reproach upon our system, and causing a feeling of disgust in the mind of the physician himself, because he imagines, overlooking his own carelessness, that homœopathy is powerless to cure. *Mercurius corr.* is a standard prescription with many practitioners in this disorder, but it will not by any means cure all cases of dysentery. Indeed, as far as I know, it is homœopathic to a very few cases, and its too frequent use when not indicated has caused, undoubtedly, a vast amount of harm.

Arsenicum is another favorite prescription with many, and I verily believe is given quite as often when not indicated as when it is. It has been terribly abused; and I will, just here, caution

my brethren against the injudicious use of this powerful agent in large and repeated doses, for, I am satisfied, it has rendered some cases almost incurable.

Now, if we have so many drugs producing a condition resembling dysentery, how are we to find the proper remedy in a given case?

The answer is, we must individualise both the symptoms of the disease and also the symptoms of those drugs which have that peculiar power of producing this condition in the healthy system, and select that remedy which contains the characteristic symptoms corresponding to the characteristic symptoms of the sick individual with whom we have to deal.

There is no other method which I know of but this that will insure success, and no other way but this of practising homœopathy intelligently.

It really seems to me that homœopathic physicians are too frequently troubled about nomenclature of diseases, and accordingly attempt to treat diseases by name, instead of, as Dr. Wells remarks, treating the individual sick man.

If I am called to a case of dysentery, and find the patient with the following symptoms: *shooting and boring* pains in the region of the navel, *increased* by *pressure*, the lower part of the abdomen swollen, and sensitive to pressure; distension in the *left side*, and along the track of the colon, made worse after eating; *fainting during the stool*; the stools are frequent and consist principally of bloody water. The tenesmus is *violent*; fresh air ameliorates the symptoms, and yet the patient rebels against it; hunger during the stool; *cutting pains* with *pinching* in rectum and loins; heaviness and sometimes numbness in the thighs: I at once gave *Aloes*, and the relief to the patient is prompt and decided.

If, again, I find a case presenting the following characteristic symptoms: loud rumbling in the bowels as if empty; stools composed of *fæces* and blood; pain in the back, as if bruised; *right side* of the abdomen swollen and hard, with pain as if a sharp instrument cut into a wound when pressed upon; relieved by escaping flatus; taste slimy and putrid, as of spoiled eggs; whole surface of the body very sensitive; wants to drink almost constantly, but does not know what; all drinks taste badly to him; fruitless urgency to pass water; chilliness of the back;—*Arnica* is the remedy I administer, and improvement soon sets in under its use.

Now, if a patient requires *Arsenicum* he will have these symptoms: *great thirst*, while the patient drinks but little at a time; stools have an odour like foul ulcers; urine has a greenish tinge; tongue looks blue; sticky perspiration; patient tosses about, cannot keep still; will move from one bed into another; despairs of his life; *before the stool*, feels as if the abdomen would burst open; *during stool*, feeling of contraction above the anus; *after stool*, burning in rectum, and trembling in all the limbs, also palpitation of the heart; the tenesmus is accompanied *with burning*; after every stool, exhaustion; eruptions often appear on the skin; cold sweat. These are the characteristic symptoms of this drug, and if the remedy is given in such a case, a cure will be the result.

Colocynth will be the remedy if, along with the generic symptoms of dysentery, we find the patient *doubled up* with the pains, pressing something against his abdomen for relief, sometimes a pillow; and, again, if he is up, pressing his bowels against the corner of a table, or throwing himself over the foot-board of the bed; with fruitless efforts to vomit; prostration *after* each stool; burning along the sacral region. When called in the early stages of this disease during the past season, I found *Belladonna* indicated in many cases where the pains in the abdomen appeared *suddenly*, and *disappeared* as *quickly* as they came; sharp, shooting pains. There was also great bearing down, as if everything would fall out of the abdomen, with tenderness of the abdomen to external pressure. This sudden coming and going of the pains is characteristic of this drug, and from its use in such cases I obtained the most satisfactory curative results. I found *Bryonia* promptly curative when all the pains were made worse by the least movement—even the raising of an arm over the head, with great thirst, and drinking large *quantities of water* at a time. *Merc. corr.* I used but twice last season, the only instances where it was indicated. My guide for the use of this drug is as follows:—The patient is passing frequently almost pure blood; has complete suppression of urine, or the urine is passed with great difficulty, with severe tenesmus of the bladder.

Now, notwithstanding that the patient to all appearances is undergoing severe suffering, according to all the objective symptoms, yet he lies *perfectly quiet* and *composed*. Here we have a case for *Merc. corr.*, and under its use the urinary troubles soon

pass away, and the patient is rapidly relieved of the dysenteric symptoms.

Now, *Arsenicum* has urinary difficulties among its symptoms ; but it is characteristic of this drug that all its symptoms are of the most violent kind.

In the case of a lady to whom I was called early in the morning some time ago, I found her thrashing about the bed, throwing her arms about, moaning terribly, and refusing to be comforted. Her husband told me that she had been in that condition all night. I gave her a dose of the 200th potency of *Arsenicum*. In a short time she became as quiet as a lamb ; the bloody passages ceased, and returned no more ; she went to sleep, and slept five hours, and was a well woman on the following day, except the usual prostration.

Summoned to see a lad, aged 14, I found that he was passing—and had been for a number of days—stools like water in which meat had been washed, and his mother told me that his passages and sufferings were invariably worse from early in the evening until three o'clock in the morning, when he became more comfortable. The time of exacerbation pointed to two well-known drugs, viz., *Merc.* and *Rhus* ; but as only one of these has this characteristic stool, the *Rhus. tox.*, I administered it in the 200th potency, and had the satisfaction of seeing the lad improve from the time the first dose was taken.

When I am called to cases that have been running along some time under allopathic treatment, I invariably prescribe *Nux vom.* as an antidote to the drugging which the patient has received, and continue it at least twenty-four hours, after which I carefully examine my patient, and select the proper remedy for the symptoms as they then present themselves.

Though *Nux* is one of our leading medicines in constipation, it is, notwithstanding, a most valuable agent in dysentery, and is often indicated. Its characteristic symptoms are:—*Small* and frequent evacuations with violent tenesmus, with the peculiar *pain* in the *back*, as if it were *broken*, in the region of the sacrum. When these symptoms are present, this drug will have a very happy effect.

There are two drugs almost exactly like *Nux* as regards the character of the discharges ; but there need be no difficulty in choosing between them when we come to note their characteristics. I allude to *Capsicum* and *Mercurius*. But these two drugs do not

have the peculiar back pain of *Nux*; and while with *Nux* the pains and tenesmus *cease* with the evacuations, under *Caps.* and *Merc.* they continue for some time *after*.

It may be proper to state, before closing this hurriedly prepared paper, that in each and every case of this disease which I had in my care during the summer of 1869, I invariably used the 200th potency, and that too with the most signal success. Indeed, my success far surpassed former periods, when I did not adhere exclusively to the higher dilutions.

And now, in conclusion, allow me to add, that if we wish to succeed as homœopathic practitioners, we must make these distinctions in using the multitude of remedies we have at our command. Otherwise our prescriptions are virtually no better than those of the opposite school of practice. And we soon find ourselves, almost unconsciously, falling into the deplorable habit of prescribing according to guess-work, in direct opposition to those rules laid down by Hahnemann, which, if rightly followed, lead to the utmost precision—a precision hitherto altogether unattainable in medicine.

Deafness from Scarlatina. By A. H. HULL, M.D.

Scarlatina can never prevail as an epidemic without leaving behind its terrible foot-prints. Deafness from scarlatina is so frequent and its results so unfortunate and distressing, that we cannot weary in relating our individual experiences, hoping each time to add something to our knowledge which may be of future use. The case to which I would invite your attention is typical of a large number of cases, which will be found among the sequelæ of every epidemic of scarlatina.

CASE.—C. W—, aged 11, has had a catarrhal discharge from the nose and ears for five years, a sequela of scarlet fever. He has a deformity of the roof of the mouth and enlarged tonsils with impediment of speech, he also has a great roughness of skin, worse in cold weather. He was for a long time treated by other schools of practice, and finally came into my hands, on the 25th of January, 1870. The speculum reveals the following: Right ear discharged a profuse, waxy, transparent, white substance; the membrana tympani was perfect, though very red and thickened; the dermis of the external meatus was thickened, red and corrugated; the orifice of the meatus was very small. The

Eustachian explorer could not be introduced, from the thickening of the mucous lining of the tube. The ear was quite deaf, could not hear ordinary conversation; the ticking of the watch was heard only by pressing the watch firmly upon the external ear. There had been severe pain in the ear occasionally since his recovery from the fever, after which the discharge was more profuse. The left ear discharged a bloody, offensive smelling matter, which was apparently from the surface of an ulcer. Upon introducing the speculum there was found a deep circumscribed ulcer within the latter third of the canal, which nearly reached the tympanum; the ulcer was deep and the edges were very red. The tympanum was red and deeply congested, the small arterial ramifications were so engorged that the membrane had the appearance of being an interlacing of fine, brightly polished copper wires. In this ear there was constant dull pain, with an occasional darting of sharp pain. The hearing power was entirely gone; could not hear the ticking of a watch if pressed against the external ear ever so firmly. I ordered,

R. Ars. iod., 2nd dec., 3ss.

Dose two grains once in 3 hours; and for the right ear as a local application.

R. Hydrastin mur., gr. xxx;
Ac. mur. dilut., gtt. xv;
Boiling water, ʒvj.

Let it stand four hours and decant, put a teaspoonful of this into a teacup of warm water and inject at one sitting; repeat three times each day.

For the left ear as a local application, I ordered the following.

R. Carbolic acid sol., ʒij;
Glycerine, ʒij;
Distilled water, ʒij. Mix.

Inject one of Lewis's syringes full once in four hours. Within an hour after syringing the ear I wiped it dry by means of a small piece of sponge, attached to a gum elastic staff, which was carefully introduced into the ear and gently turned around and withdrawn. I followed the wiping with a few drops of English glycerine directly into the canal, allowing the patient to hold the head on one side until the glycerine could be felt to touch the tympanum, and then stopped the ear with fine lamb's wool. I

persisted in this plan of treatment for four weeks, and within that time the diminution of the discharge was easily perceived, and the hearing in the right ear was decidedly improved. The ulcer in the left ear was less congested and the fetor was removed, but still the tympanum looked red and irritated. I did not observe any improvement in the tonsils. The obstructed Eustachian tube was relieved, as the hearing power on that side was greatly increased. I gave internally.

℞ Merc. iod., ʒij.

Dose, one powder of two grains, once in four hours.

Continued my treatment, locally in the right ear as previously described. For the left ear I prescribed :

℞ Tannic acid, gr. x ;
English glycerine, ʒij.

Dose, a few drops in the ear, turning the head well to one side, to allow the medicine to come in contact with the ulcer, and stop the ear with lamb's wool ; repeat three times each day.

From this prescription I observed the ulcer to assume a different character at once ; in forty-eight hours there was a decided improvement in this ear. These prescriptions were continued for two weeks with general improvement. The enlarged tonsils were sensibly diminished, the discharge was only slight in either ear. In the right ear scarcely any discharge was seen, and the hearing power was good. The child could hear ordinary conversation.

The changeable weather was very trying to my patient, and it was just the time when we had a prevailing epidemic of a species of influenza ; many were sick in bed with it, and the patient had an attack of it. It assumed the form of violent coryza, excoriating discharge of watery thinness from the nose and ears, pain in the forehead, pain in the bones, fever, dark-coated tongue, soreness of throat and enlargement of tonsils. These symptoms rapidly disappeared with the exception of the discharge from the nose and ears. For the nose locally I used the nasal douche, with the following prescription.

℞ Permanganate of potash, ʒj ;
Distilled water, Oij.

Put a tablespoonful into one quart of water and pass through the nose at one sitting ; repeat this three times per day.

Internally I gave *Lycopodium* 6th dec. two grains, once in two hours. The discharge rapidly subsided, but the patient complained of a noise in the ears, resembling the washing of waves upon the beach. For this I gave: *Phos.* 3rd dec. in pellets, No. 5. Six pills once in two hours. The case improved under this treatment, until a complete recovery took place. The ulcer has entirely disappeared, the tympana have resumed their natural appearance, the hearing is completely restored, the tonsils are of normal size. The impediment of speech cannot, of course, be overcome, as it is of a congenital nature; otherwise the case is a complete success.

Polypus of the Rectum treated by Bromide of Potassium.

Reported by W. TOD HELMUTH, M.D.

The patient, a woman of twenty years of age, admitted into the Good Samaritan Hospital, January 25th, 1869, had never menstruated, was short of stature, and dwarfish in appearance. She stated that eight years ago she had suffered from a red, bleeding substance in the rectum, which had been removed by a physician, but that similar growths had apparently returned. She was pale and sickly-looking, though not much emaciated, but was troubled with a constant diarrhœa, and more or less tenesmus, and the passage of a good deal of blood. Upon a mere external examination of the parts, nothing particular could be noted; but upon desiring her to attempt to expel the contents of the rectum, there would protrude several elongated bodies, resembling earth-worms in shape, but of a much more brilliant red colour. They presented a soft, vascular shreddy appearance, bearing some resemblance to sarcomatous growths. With this expulsion there was always a yellow, very fetid discharge. Upon examination of the fæces they were flattened, and there was flatulent distension of the bowels. Knowing the value of the *Bromide of Potassium* in the removal of several varieties of morbid growths, I determined to try the medicine upon this patient. I gave the following prescription:

R. Potassæ bromide, ʒj;
Aque fontanæ, ʒvj. M. ft. sol.
A teaspoonful three times a day.

She continued the treatment from early in February until the

middle of May. She began to improve in health shortly after taking the medicine; the diarrhœa ceased; she performed household duties in the hospital, and on the 13th of May, though the fetid fluid was expelled, she was unable to protrude any polypi. The remains of the growths could be felt, but otherwise the patient was in good health. I had endeavoured to draw down the polypi and ligate them, but they were too friable, and broke away so easily that I gave up the attempt.

We have only one more journal on our list. It is the *New England Medical Gazette*. Its name imports the locality where it is rooted and grows. It is published at Boston, and issued monthly. Its editor was at first Dr. H. C. Angell, whose excellent *Treatise on Diseases of the Eye* we have lately reviewed: now it is Dr. J. T. Talbot. The following cuttings from this Journal seem to us worth preserving.

Borax in Membranous Dysmenorrhœa.

By E. M. HALE, M.D., Chicago.

A lady, aged thirty-two, has had one child and several miscarriages. A few months after her last miscarriage, which occurred when her child was a year old, she observed that the menstrual flow contained shreds of a tenacious nature, and a skinny substance in large quantities. At one period of the day the discharge would be profuse; at another time scanty, and with great pain, like labour pains, in the back, hips, and hypogastric region. The amount of membranous substance expelled at the menstrual period was considerable; "half a teacupful," she said. This occurred several months in succession before I was consulted. To satisfy myself of the actual presence of membranous products, I examined the excretion. It had the appearance of a thin, skinny substance, in some instances almost resembling broken-down hydatids. Pieces as large as a dollar, mixed with coagula, were observed every day. The menstrual flow lasted eight or ten days, leaving her weak and anæmic.

The cases of membranous dysmenorrhœa, reported cured, are very rare. In the *Journal of Obstetrics* (1869) is reported a very unique, obstinate, and interesting case, which was *not* cured by any means which the most eminent allopathic physicians of

England and this country could devise. This case was the second which had ever come under my observation. The first I failed to cure, after five months' careful treatment with all the remedies recommended except *Borax*. Dr. Guernsey (*Obstetrios*) recommends *Bromine*, *Bryonia*, *Calcarea carb.*, *Chamomilla*, and *Cantharis*, but he does not assert that any actual cures have been made with them.

I tried all of them in my first case, also *Kali bich.*, *Sanguinaria*, *Bromide of Potassa*, and *Ammonia mur.*, because of the assertion by some homœopathic writers, that remedies homœopathic to false membranes in the respiratory mucous membrane, would probably cure membranous dysmenorrhœa.

But the idea that this membrane is croupous in its character is obsolete. It corresponds exactly with the decidua vera, in the early weeks of pregnancy. The pieces I examined were smooth on one side and rough and villous on the other.

The ovarian pain, which Grailey Hewitt mentions, was a constant phenomenon during the menses in this case.

Dr. Ludlam * very properly says, "Since we understand the origin and structure of the decidua menstrualis, the stereotyped advice to employ such remedies for the cure of this disease as are given in pseudo-membranous croup and diphtheria, would be of very doubtful service."

Deweese recommends the *Ammoniated tincture of Guaiacum* in membranous dysmenorrhœa. Bennett highly recommends *Borax* and gives one remarkable case of a woman, aged twenty-six, subject for two years, since marriage, to painful menstruation. The menses were regular as to time, but usually lasted a week, and were attended with such great suffering that she had always been compelled to lie down for the greater part of that time; the discharges were generally very dark in colour, and mixed with clots of blood and *numerous flakes of whitish membrane*. In the interval there was more or less of leucorrhœa, with dull aching pain in the lower part of the back, her tongue was redder than natural, pulse 96, and sharp. After taking the *Biborate of Soda* for twenty days, menstruation occurred with very little pain, and was unmixed with either shreds or flakes. During the whole period she had been able to pursue her usual avocations, and had enjoyed a degree of comfort to which she had been a stranger for years. The next month she also menstruated without material suffering;

* 'Lectures on Diseases of Women, &c.' Part II.

the aching in her back had also disappeared, together with a chronic cough which had persisted for months.

My patient had all the symptoms mentioned in Bennett's case, and I felt justified, even though the objective symptom was not mentioned in the proving, in giving her *Borax*, of which during the fourteen days previous to the menses, she took five grains three times a day, in a tablespoonful of water. At the next appearance all the painful symptoms were much alleviated, and a less quantity of membrane was observed.

So soon as the menses ceased she was ordered to take the *Borax* again in the following manner: Five grains once a day for a week; then twice a day for a week, then three times a day until the menstrual flow ceased. The result was that she had the most comfortable time she had known for months; the loss of blood was not in excess, and the membranous shreds and pieces few in number. While taking the *Borax*, instead of feeling any unpleasant or pathogenetic effects, her general health greatly improved; even the condition of her nervous system, which was almost as bad as possible, was much benefited. Another month passed under the same treatment, and the menses came on in a perfectly natural manner.

No more *Borax* was given, and up to this date—nearly a year—her menses have not been attended by any abnormal symptoms, and her health is better than it has been for several years.

I have used *Borax* considerably in many cases of amenorrhœa, dysmenorrhœa, and various uterine diseases, as well as ovarian, and I believe it has not received the attention in such diseases that its merits demand.

Hahnemann placed *Borax* in his Chronic Diseases under the name of *Natrum boracicum*.

Noack and Trinks observe that it is "especially suitable to sensitive, lax temperaments, and nervous constitutions, especially to females and children . . . for diseases of the mucous membrane, and the diseases of the female parts." Clinically, they recommend it for "irritated conditions of the sexual organs; various kinds of menstrual irregularities; sterility; leucorrhœa."

The symptoms of the female sexual organs belonging to *Borax* are numerous and important. It has the power of profoundly modifying the functions of the uterus and the menstrual flow. It does not have the "membranous shreds and pieces among its symptoms, but it has "*leucorrhœa, thick as paste, and white,*" also

"white mucus, albuminous, profuse and thick." This is almost sufficient to lead us to believe that *Borax* would cause membranous substances to be formed abnormally, if a proving sufficiently heroic could be made. All the symptoms mentioned by Bennett, and of my own patient, can be found in the pathogenesis of this drug.

We believe the proofs adduced are now sufficient to allow us to place *Borax* among the few remedies for this rare and obstinate disease.

Dr. C. E. SANFORD, Chairman of the Committee on Clinical Medicine, read a very interesting report upon *Cactus grandiflorus* in diseases of the heart. One case was that of a woman, aged forty-eight years, of lymphatico-bilious temperament. She had been unable to lie down for nearly two years; the least exertion made it almost impossible for her to breathe. She had been under the care of eminent physicians who considered her incurable. The one last called refused even to prescribe, stating to the friends that medicine could do her no good. Dr. Sanford found, upon examination, great irregularity of the heart's action—intermittent at times and of varying character—great frequency of action, alternating with slowness. He called it enlargement of the left ventricle, with extreme irritability of the cardiac nerves. *Cactus*, was prescribed; a dose every hour until there should be some relief; then with lengthened intervals, as the alarming symptoms yielded. The patient was relieved after *two* doses, and the following night slept quietly in bed. In one week from the first visit she "did the washing and other household work." She has had no recurrence of the heart troubles since. Dr. Sanford has also successfully prescribed *Cactus* in cases of irregular action of the heart through sympathy, in enlargement of the heart, and in valvular diseases. One case of palpitation, accompanied with vertigo, dyspnœa, and almost complete loss of consciousness, has been entirely cured by the use of *Cactus*.

Practical Observations on Calcareo Phosphorica. By CRO S. VERDI, M.D., Mount Vernon, Ohio.

Calcareo phosphorica, or bone phosphate of lime, is soluble in muriatic acid; nitric and acetic acids will also dissolve it. It is insoluble in water.

Hypophosphite of lime has attracted much attention in the profession. In consequence of its deoxidizing power, substances which readily part with oxygen convert its hypophosphorous acid into phosphoric, and the hypophosphite is supposed to become a phosphate after being conveyed into the system. Hypophosphite of lime is formed by boiling milk of lime and phosphorus, stirring the mixture until the combination is formed. Part of the phosphorus is oxidized at the expense of water, while the rest escapes combined with hydrogen, the gas taking fire as it reaches the air. This salt is soluble in six parts of water, more so in boiling water. It may be converted by heat into phosphate of lime. I give the chemical composition of each of these two salts, so as to render our subject more comprehensible. *Calcareæ phosphorica* is simply bone phosphate of lime $(PO^4)^2Ca^2$; the hypophosphite of lime is a chemical compound consisting of $CaH^4(PO^2)^2$.

What, then, was the original idea of introducing these agents into therapeutics? It was to supply a deficiency of lime in the human economy in the different forms of mollities ossium; and as it was found afterwards to promote cellular organization in the vegetable kingdom, it was thought therefore an excellent adjuvant in scrofulous diseases, and particularly in those of the lungs. Of course, as usual, the allopathic school combined this simple agent with potassa, ammonia, soda, cinchona, arsenic, cod-liver oil, and iron; so far as my knowledge extends, however, all these preparations have been used empirically, upon suppositions, and not provings.

An Italian physician, Dr. Polli, has suggested in the meantime, and not without good reasons, the use of another class of preparations, called the hyposulphites, particularly the hyposulphite sulphite and bisulphite of soda, following out an old theory of Hahnemann as to a class of medicines called by him *antipsorics*. The idea of this distinguished Italian seems to be that of disintegrating blood-poisons by the influence of these agents, thus decomposing the *materies morbi* existing in the blood, and setting its components free to be excreted through the emunctories. It is to be regretted that so little resulted from all these interesting researches, and, too, that very little success has followed this method of treatment, although it has sometimes proved advantageous. The failures are due to the imperfect knowledge of the pathogenesis of the agent; for although we know the general morbid

effects of these medicines, yet we do not know the exact *modus operandi* of them upon the blood; hence the unsatisfactory feeling as to the reliability of these agents.

CASE 1.—Some six years ago I used *Calc. phosph.* 3rd dec. in a case of tubercular phthisis, which, to all appearance, seemed to be in its last stage. This case was that of Mr. W., aged 70, of an active disposition and of a nervo-lymphatic temperament, the nervous predominating; complexion florid, blue eyes, white, tender skin; frame strong, muscular; voice deep. By physical examination I found dulness on percussion at the middle half of the right lung, with crepitating sound; at a certain spot, in the same lobe, I heard a cavernous gurgling murmur, distinctly indicating a vomica. The respiration was hurried and short, the expiration laboured: in the first instance there was a difficulty in filling the air-cells; in the second, it was difficult to discharge air from the lungs. The sputa were very scanty, but certainly grayish and granular. There was no pain, but he had a fever every afternoon from 8 p.m. to 7 p.m. He was very restless, had no appetite, lost flesh rapidly, accompanied by great prostration. The feet and hands were burning hot. The cough was very troublesome, and, in paroxysms, often hacking. This patient had consulted many physicians without much encouragement. I gave him *Aconite* for the fever, and *Calc. phosph.* 3rd dec., one powder of two grains every hour when he was free from fever.

In connection, I directed bathing in tepid water saturated with salt, frictions, and electricity. The total absence of appetite was a serious difficulty to overcome, for such patients must eat. Happily I suggested that he should eat what he had been accustomed to on the mountains of Vermont, which was cheese and bacon. He liked the diet, and "thought he could eat that." Sure enough, he improved in flesh under it, and gained every day in strength. I prevailed upon him to keep out of doors all he could. As for the effect of the medicine, nothing could be more desirable. The sputa became more fluid and easily raised, the voice less resonant, respiration almost natural, pulse gaining in fullness; there was no fever, and the whole condition of the patient announced returning health. Perfect convalescence took place in two months. During the treatment with *Calc. phosph.*, I tried at intervals, *Iodium*, five drops in half a tumbler of water. I must say, I found it a great dissolvent of tubercular nuclei.

CASE 2.—Miss S. G—, 31 years old, of a delicate texture and blond complexion, with large blue eyes, and a highly nervo-lymphatic temperament. She is of a small delicate frame and a strumous constitution, where lymph predominates.

About fifteen years ago she took a severe cold, which resulted in pneumonia of the left lung; since then, she has suffered more or less every winter with pulmonary trouble, which has increased, until it resulted in a true case of scrofulous pneumonia. Mucopurulent phthisis set in permanently some years ago. Such was the history of the case. The crepitant râles were heard all over her left lung, with dulness on percussion; the cheesy expectoration, indicative of yellow tubercle, the slow but steady progress of the disease, the pain in the lungs and great loss of strength, appetite, and flesh, with fever at intervals, though yet able to be about, more or less, and generally quite comfortable in summer, the enormous expectorations of a viscid, yellow-white character,—these peculiarities belong to scrofulous phthisis. When I examined her the first time,—two years ago,—I found her left lung entirely hepatized, with the exception of a few spots; her voice was hoarse, and the breathing bronchial. This lung was much contracted, with little expansion in breathing. The right lung I found in a better condition, but crepitating under the axilla, and painful at times. Sydney Ringer says: “The tubular breathing, the crepitant râles on a large space of the lung-tissue, while yet there is dulness on percussion, are the true pathognomic signs of this peculiar consumption.” The pulse was natural in the morning; high in the afternoon, till about 9 p.m. At night she was restless; the cough being paroxysmal and very distressing.

Treatment.—*Aconite* during the fever; *Hyoscyamus* and *Pulsatilla* during the nervous paroxysms, which often came on after a prolonged period of coughing; hot applications of hops upon the painful lung. Often I had to resort to a stimulant as the only calming agent,—even better than *Pulsatilla*. I must say that, while a stimulant would increase her pulse, and enliven her countenance, it had the most soothing influence when she suffered with what she called “nervous attacks,” which were indeed fearful to witness. Her respiration became short, panting and difficult; her eyes and nostrils dilated, and she seemed struggling for life. Coffee, whiskey, and ether were the only available means of relief in such an emergency. Hot cloths and camphor were also used

for friction of the extremities. Indeed, I am convinced that these agencies have often been the means of keeping her from sinking. The arsenite of potassa—"Fowler's solution"—proved very beneficial in the case. *Ferrum oxydum*, *Phosphorus*, *Ipecac.*, *Iodine*, *Kali bich.*, &c., were used symptomatically with temporary good results. I finally prescribed *Calc. phosph.* 3rd dec., one powder of two grains, every two hours. After a few days I found her expectoration much diminished in quantity and more liquid; the crepitant râle became less audible, the sonorous rhonchus disappeared, and the sibilant râle here and there was often heard. The voice became stronger and less tremulous; fever disappeared, the night-sweat also became less troublesome. I may say that her general condition gave sign of decided improvement. I immediately took this opportunity to order a daily drive in the open air, and a hydrochloric acid bath every evening. Her appetite also improved as she went on with the treatment. Her diet consisted of oysters and beef-tea, with raw pounded beef in old whiskey, to be taken three times a day. At intervals I used hypophosphite of lime by inhalation, which seemed to answer very well. As she became anæmic, I prescribed hypophosphite of iron and *Arsenicum*. The *Calc. phos.*, even in this desperate case, proved a very efficient remedy.

Case 3.—Mrs. J. L—, 43 years old, married, but childless. A large muscular woman, with large bones and a heavy frame. Her complexion is of a peculiar pale and dark cast. She is of a bilious-lymphatic temperament, with blue eyes, and evidently of a scrofulous constitution. Her sister and mother died of pulmonary disease.

At the time I saw her she was suffering from dysmenorrhœa and profuse menstruation. Her lungs were large and well developed; her chest was symmetrical, with no apparent signs of pulmonary disease. Percussion revealed no abnormal sound. Auscultation detected a peculiar sound at the apex of the right lung. The respiration was slow, with prolonged inspiration. There was pain at the apex; a kind of ache. The cough was very troublesome—at times, paroxysmal. The temperature was natural, the pulse equal and weak. There was fever, or pyrexia every afternoon, with flushed cheeks, and general excitement. She then became very loquacious and declared that "she felt well enough, only her cough troubled her more during this fever time." The left lung was in a normal condition, although the vesicular

murmur was very indistinct, thus showing a want of free cellular action. There was restlessness at night, but no night-sweats. She has a constant, hacking, irritable cough, dejection of spirit, and great prostration, with constipation and want of appetite. Her general condition was that of prostration. The base of the right lung gave no abnormal sign whatever. My diagnosis was, gray tubercle—forming, perhaps—in both lungs. The expectoration was abundant, of a viscid, grayish, nucleated character, much like an oyster. Her cough was cavernous; her voice very deep at times, sharp at others; ægophony was heard at the apex of the right lung.

She also suffered with laryngeal trouble. The trachea and upper part of the bronchia, I thought at one time, gave evident signs of ulceration; for her voice became hoarse, and sunk to a whisper; there was pain in the trachea, with great irritation of the larynx. She had had hæmoptysis long before I saw her. The history of the case was as usual: she had had a severe cold for several consecutive winters past; it seemed to come on worse and worse at every change of season. To a practical physician, these are the true signs of a progressive pulmonary disease.

The treatment was somewhat symptomatic. The excessive metrorrhagia demanded the first attention, as it was an open floodgate by which life was gradually passing away. I gave *Secale* 3rd dec. without any good effect. I then gave the first decimal potency with no good result—or at least not sufficient. Then, I used the mother-tincture, one drop in a spoonful of water, every hour. It had the desired effect, but the cerebro-spinal excitement compelled me to discontinue the medicine. She was losing her memory and sight for a few moments, and complained of twitching in the uterine region. Then I tried *Rhatany* 1st dec., twenty drops in half a tumbler of water, and one teaspoonful every two hours, beginning three days before the time of her menstrual period. The remedy had the desired effect.

For the afternoon fever, I gave *Aconite*, with evident good effect. *Pulsatilla* was given her for nervousness, from which she suffered when tired. During the period of six months, while she was under treatment, she never took to her bed, but daily performed some light household duties. At times she took *Phosphorus*, a few drops of Fowler's solution, and *Kali bichrom.* (for the trachea) with benefit. Finally, she was put under *Calc.*

phosph. 3rd dec., one powder of two grains every four hours, from which she experienced relief.

Kali hydriod. was also given with the effect of liquifying the nuclei of the sputa; inhalations of hypophosphite of lime were used for a period of two months, with decided benefit, as it seemed to diminish the great expectoration and relieve that general lassitude so common in pulmonary diseases. She used tepid salt baths every other night. The patient improved,—not immediately, but after several months of constant attention and care. I will add, that these cases require perseverance, patience, and encouragement.

Mrs. L.'s physical condition improved as the expectoration diminished in quantity and became of better character. The constant ache at the apex of the lung ceased, the fever left her entirely, her appetite returned, her menses became more regular and less in quantity. But a hacking cough still remains. Of course she will finally die of the disease; but I shall hope to have shown the benefit of a careful treatment. The hypophosphite of iron was used in her case with good result.

Case 4.—Mr. M., a young man of thirty, carpenter by trade, contracted acute pneumonia some two years ago; his frame was large, the chest broad; but he was of a tubercular diathesis, and although the acute disease was apparently cured, the seed of tubercle was left.

The treatment was the same as in the other cases, yet his disease was too deeply seated, and he died in a few months after I first saw him. Even in this case, the *Calc. phosph.* and *Kali hydriod.* were beneficial.

Case 5.—Rev. Mr. N., forty-seven years old, of a spare frame, with light complexion, blue eyes, and a large head. He is decidedly nervo-lymphatic, and evidently scrofulous. This gentleman had had repeated hæmoptysis, with cough and severe pain in apex of the left lung. On percussion, I found dulness extending all over the upper lobe of the lung. On auscultation, I found bronchophony, with a sharp thrilling sound, as though it was metallic. The lower portion of the lung was undoubtedly partially infiltrated; still there was no marked dulness; only during inspiration, I could not hear the natural vesicular expansion of the air-cells, yet a certain amount of air was making its way to them. The right lung was apparently in a normal condition. He had a hacking cough; the expectoration was little in

quantity, but of a rusty colour, containing many small mucopurulent points. When I first saw him, he had just had an hæmorrhage; perhaps he coughed up a couple of ounces of blood. He had fever, and severe pain in the apex of the left lung. I found also a gurgling sound at this point, indicating that there was bloody mucus in the cells. This fearful condition of the lung extended only to a circumscribed space, as large as a silver dollar.

Treatment.—He was put in a recumbent position; cold applications with saturated cloths were constantly kept on the part; *Aconite* and *Secale* were given alternately every hour. The case progressed favorably; a week after, he brought me a piece of calcareous matter which he had coughed up; it resembled a small piece of coral, very hard and sharp—calcareous tubercle. He was directed to use inhalations of hypophosphite of lime, which relieved the severer symptoms.

This is a very interesting case for its peculiarities. The gentleman, although his lungs are in a critical condition, has never ceased to attend to his duties; he has had no night-sweats; has a good appetite and seems to enjoy tolerable good health.

Case 6.—The daughter of this gentleman, a young lady of seventeen years, has been suffering with bronchitis, from which she has entirely recovered; she, however, suffers intensely with small carbuncles in various parts of her body. I have given her *Calc. phosph.*, with benefit.

The want of space compels me to omit other cases of the same kind. However, I trust that my conclusions from the above may be confirmed by the experience of others.

I know that the agents which I earnestly put before the profession do not come under the class of our provings; I mean the *Calc. phos.*, the arsenical solution, and the hypophosphite of iron; still, I have used them on a physiological principle,—as some of them pertain to the material of which the animal frame is made,—and moreover, their use is based on physiological and pathological considerations. Homœopathic physicians understand the general pathogenesis of lime: cough, dryness of the fauces, with stinging, burning pain of the larynx; inflammation, ulceration of the trachea, irritation of the salivary and bronchial glands, bronchial irritation, extending even to the air-cells, with a painful suffocation; great scantiness of secretions of the lungs as well as of the skin and kidneys; pain in the stomach and bowels, with diarrhoea and pyrosis; great thirst, loss of appetite,

pain in the abdomen, with mucous discharges ; emaciation, pain in the joints, mental depression, carious ulcers, coryza, ulceration of the cornea, and conjunctivitis, pain in the meatus internus, aphthae, laryngitis, and phthisis trachealis. These outlines will suffice to point out the homœopathic relation of the drug to the disease. The above symptoms I have observed amongst the peasants in Italy, who sowed wheat mingled with lime.

The oxide of iron, or hypophosphite of iron—or, in other words, phosphorus and iron—are substances largely contained in the human economy. Iron is as capable of producing diseases as any other metal ; although it is not to be ranked amongst poisons proper. The inconveniences suffered by people who live near iron springs, are sufficient evidences of the pathogenetic effects of *Iron*. Teste says : “ In regions of country where all the water is somewhat impregnated with iron, nearly all the inhabitants bear traces of deleterious influence, *i. e.* a general or partial debility bordering on paralysis ; vomiting of food, pulmonary phthisis, or phthisis florida, with violent hæmoptysis ; want of animal heat, menstrual suppression.” I have further noted constipation, loss of appetite, loss of the power of assimilation, and a general depressing influence. Professor Costa observed the following effects of iron upon rabbits and other animals. He placed a number of dogs and rabbits in an exposed condition, most favorable for producing tubercular diseases, being cold, damp cellars without light, crowded, and exposed to the most unwholesome atmosphere. Some of these animals were fed on ordinary food, others upon bread containing half an ounce of iron for each pound of bread ; the former, with one or two exceptions, became tuberculous, while not one of those fed upon ferruginous bread presented even a trace of tubercular disposition. This candid statement is enough to explain the *modus operandi* of this agent. It proved a prophylactic against cacoplastic exudations, as it would prove the reverse under different and opposite circumstances. In a normal condition it proves a disorganising element, as any other foreign substance would. In all diseases of a leucophlegmatic origin, *Iron, Calc. Phosph., Arsenic, Kali, Merc. sol.*, are strongly indicated.

It thus appears that the United States possess at the present time two quarterly and five monthly homœopathic periodicals :—the quarterlies being—

The North American Journal of Homœopathy.

The United States Medical and Surgical Journal.

the monthlies—

The Medical Investigator.

The American Observer.

*The Hahnemannian Monthly.**

The New England Medical Gazette.

The American Journal of Homœopathic Materia Medica.

Not very many, it may be said, in proportion to the number of practitioners. But it must be remembered that it is the few always who write. We find the same names appearing again and again in the various journals. On the other hand, there seems to be no lack of readers: and so all that is fairly written is sure of support.

From the extracts we have given, it will be seen that the tendencies of literary American Homœopathy are towards what is called "Hahnemannianism." How long they will continue to be so is a question; and how far they represent the general run of American practice is another. In the meantime, it is hardly to be regretted that this school, so sparsely at the present day represented in the Old World, should have full opportunities of developing its theses in the new. Its extravagancies, as seen in Dr. Fincke, and its bitterness, as exhibited by Dr. Lippe, have only to be seen to be condemned. But Hering, Dunham, Wells, and Pearson are names of a different order. Dr. Guernsey's "characteristics" and "key-notes," however one-sided, are a legitimate hypothesis which we cannot but respect. And the reports of cases are an accumulating store of material on which the future judgment as between Hahnemannianism and progressive homœopathy must be founded.

* We have received from Dr. MacClatchey, the present editor of the *Hahnemannian Monthly*, a letter objecting to our statement that that periodical is under the control of Dr. Lippe. For the last two years he tells us it has been an impartial and independent journal. We are quite willing to believe this, and to give publicity to Dr. MacClatchey's reclamation. Our reason for the statement we made is that no copy of the journal has been forwarded to us since Dr. Lippe ceased to have the control of it, so that we were not aware of the change that had been effected in the character and editing of the *Hahnemannian Monthly*, and we need not say how pleased we feel at the change.

While this is so, moreover, writers like Drs. Ludlam, Holcombe, and Hale exhibit doctrines and practice more like those to which we are accustomed in this country; and exhibit them with force and even (in the case of the two former) elegance. The great impetus of late given to the institution of hospitals and dispensaries in the States, and the many benevolent establishments which are being transferred to the care of homœopathic practitioners, are supplying opportunities of experience which are being turned to good account. Homœopathy is already much indebted to America; and the obligation bids fair to increase as years go on. Any crudities which have marked the youth of its periodical literature may be expected to disappear as the several journals proceed to maturity; and the band of students now ripening into practitioners of our art at the various colleges ought one day to give us provings and practisings in abundance. So, with much appreciation of its past and present, and good hope for its future, we end our survey of homœopathic journalism in America.

ON SOME CAUSES OF THE VARIETIES OF HOMŒOPATHIC PRACTICE.

By R. E. DUDGEON, M.D.

(Read before the British Homœopathic Society.)

It can hardly fail to strike one as rather paradoxical that while theoretically we claim for our system of medicine the character of an almost exact science, practically we are very far from being agreed as to the proper remedies for concrete cases of disease. It is common enough to find at meetings of societies such as this one member saying that such and such a medicine has never failed him in such and such a disease, while another member will incontinently rise and say that he has given the said remedy a fair trial in the said disease, and has never seen it of the slightest use. A similar discrepancy is observed in our text-books of therapeutics, though not by any means to such a degree as in the less

cautious unpremeditated remarks of individual practitioners. The reason of this seemingly greater harmony among the text-books I believe to be partly owing to the circumstance that a good deal of our therapeutics is now of a traditional nature, and that it has come to be taken for granted that a certain remedy is the proper one for a certain morbid condition, though the writer himself may have no sure knowledge from his own experience that such is the case, just as in the old school to this very day text-books are published which allege that, *e. g.*, bloodletting and blisters are the remedies for pleurisy, though, perhaps, the author himself has never treated this disease by those means.*

Another reason for the greater harmony of text-books seems to be that the writers have little or no practical experience of many of the diseases they must write about ; so if they do not actually copy other writers, they have to consult the same sources as these, and from the *materia medica* or the records of clinical experience they derive the remedies for these diseases. But even in these works we find with regard to diseases of which the authors have

* I remember a remarkable instance of the tenacity of a traditional idea respecting the suitable remedy for a disease. In my youth our family doctor was a very Sangrado in his ideas as to the remedy for measles. He believed bleeding to be the one proper remedy for every case ; and every case was bled accordingly, with what effect you may imagine. His assistant, whom I knew in after-life, and who was (alas ! was) one of our most distinguished and talented homœopaths, told me that it was the rarest accident when any measles-patient recovered under his principal's hands. They died like flies in October. And the most curious circumstance was that our doctor, like his Spanish prototype, profoundly believed that the patients died because he had not bled them early enough or copiously enough. Of course *we* know that unassisted measles will hardly kill the most delicate infant, so you may imagine how powerfully the treatment of our doctor must have contributed to the result obtained. I well remember his smug clean-shaved face surmounting a white choker ; the customary suit of solemn black, with which the solemnity of his expression, doubtless caused by the grave nature of his proceedings, accorded, giving him an air of the most unquestionable respectability. His irreproachable respectability gained him the confidence of a large clientèle, and he ultimately retired with a considerable fortune to his native village, where it is to be hoped that, like the retired butcher who used to treat himself to an occasional day of sheep killing, he sometimes had an opportunity of exhibiting his medical skill on a measly pig.

had much experience, the same discrepancy as we observe in the *viva voce* communications of members of our societies.

If our system is theoretically so exact as we profess to consider it, how comes it that we do not all agree as to the remedies we ought to administer in concrete cases of disease? I think that the discrepancy may be accounted for and explained without detracting from the theoretical exactness of the principle *similia similibus*.

1. Many diseases which we think we have cured have simply recovered in the natural course of things. That this is so we should scarcely discover were our attention directed solely to our own practice, in which, probably, we never leave a disease to its own unassisted course, but give a remedy for every case we see; for why were we doctors else?

But there is no fact better ascertained in medicine than this, that many acute diseases recover perfectly and rapidly without any drug-treatment whatever—in fact, more quickly without drugs than with drugs of a perturbative action. Hence we may easily infer, if in our own practice such a case makes a speedy and perfect recovery, that we have a brilliant instance of the cure of this disease by a remedy which, in fact, has had nothing to do with the result; which, indeed, had no action whatsoever, or which had never been taken. How often do patients tell us with a chuckle of enjoyment, which we are inclined to re-echo, that they never took any of the medicines prescribed by their last doctor! We forget Brabantio's caution to his son-in-law, "She has deceived her father and may thee." But without supposing a deceptive patient, we may easily understand how the patient may not have swallowed what we prescribed if our prescription was made up at that shop where we were lately told nothing but simple spirit and un-medicated globules were employed to represent all dilutions beyond the 1st.

Every one must admit that, in most cases, when a medicine not indicated by the disease is given in our small doses, it remains without effect, therapeutic or pathogenetic. That such diseases form a large proportion of every one's

practice is a fact at once fortunate for the patients, and unfortunate for the precision of the therapeutic art. It looks almost like a providential arrangement for the non-medical world that when they are far away from their doctor they, though subject to the same amount of disease, seldom require the assistance of their medical adviser; whilst, on the other hand, it looks like a providential arrangement for the doctor that when he is within easy hail these diseases should seem so urgently to require his aid. Here, contrary to the ordinary maxim of commercial economy, the supply appears to create the demand.

No wonder that in such diseases each doctor has his own favourite remedies, and no wonder that each seems to be equally successful, for, probably, the disease was materially altered in intensity and duration by none, but ran its own wilful course altogether irrespective of the innocuous doses administered with the greatest gravity at regular periods. The action of the doctor in many such cases, if we could only see things as they are, is akin to that of the Chinese during an eclipse, who beat drums and make other discordant noises to frighten away the monster that is seeking to devour their luminary. The dark shadow pursues the even tenor of its way and takes its own time, but when it has disappeared the happy celestials imagine its disappearance is due to their futile clamour, just as we take credit to ourselves for the disappearance of many a morbid shadow that is merely temporarily eclipsing the health of our clients, and would pass away in its own due time.

It is difficult to form a guess as to the number of diseases of greater or less severity that belong to this category, but probably our vanity would receive a severe shock were we to be assured that in the list are included many diseases that we flatter ourselves are among the greatest illustrations of our triumphant success,—perhaps pneumonias, pleurisies, dysenteries, diarrhoeas, and very many of those whose technical names terminate in “itis.”

The private practice of no physician could enable him to form a correct estimate of the value of his treatment in those acute diseases, and if we look at the results of

treatment on a large scale in hospitals, in such a disease, for example, as pneumonia, we find that the difference in the results of the homœopathic treatment of Fleischmann, Wurmb, Tessier, and others, and of the expectant treatment of Dietl and Bennett of Edinburgh, scarcely admits of very triumphant expressions respecting the superiority of the homœopathic method of treatment. And if this is the case in regard to a disease of such a definite character as pneumonia, we may well hesitate about asserting the undoubted vast superiority of our method over the expectant system in many other acute diseases which from their nature are less adapted for comparison.

These considerations, while they account for the discrepancies among practitioners of our school, in respect of the remedies used and recommended for one and the same acute disease, detract materially from the value of their observations, for if, as we know is the case, the natural tendency of most of those diseases is towards recovery, we have no sure evidence that in the cases they bring forward the recovery was due to or materially promoted by the remedies they administered.

Another element of doubt is imported into the subject by the circumstance that in the treatment of acute disease no practitioner relies solely on the administration of drugs, but adopts simultaneously many other means, such as poultices, fomentations, inhalations, baths, frictions, compresses, and various artifices of diet and regimen, any one of which, or all together, may have contributed more to the recovery than the medicinal substances administered, in fact, we may easily suppose that they may have done good in spite of absolute inappropriateness of the medicine to which the practitioner attributes the cure. "Judgment is difficult," said Hippocrates, who used no drugs and very few other means; *à fortiori*, it must be more difficult for the practitioner who employs many drugs and many other means to form a correct judgment as to which of the means employed was the really efficient agent, if any was.

It is astonishing how apt we are to see what we expect to see, even when conducting experiments of a simple and

uncomplicated character. I was never more forcibly struck with the truth of this, and of the necessity of divesting one's self of all preconceived notions, than recently, when making some experiments with regard to the refractive power of various transparent substances. I found it necessary to eliminate from my mental attitude, so to speak, all expectation of what was to occur, before I could get correct results; and several times I detected myself actually seeing my preconceived notions in my instruments, which in a more cautious and less eager frame of mind I discovered to be quite erroneous. Here repeated experiment enabled me to detect and rectify the error, but how much more apt must one be to see one's preconceived notions realised in the case of the very complicated experiments of clinical observation, and how much more difficult must it be to detect and rectify an error in them of the kind I have alluded to.

Patients, too, are particularly liable to be deceived and unwittingly to deceive their doctor with respect to the action of medicines. We all remember the oft-cited case of the patient suffering from violent neuralgia, on whom Sir Humphrey Davy wished to try the effect of *Nitrous oxide*, and who lost all his pain the moment a thermometer was placed under his tongue for the purpose of making a preliminary observation respecting his temperature. In like manner our patients are not seldom wrought up to a similar expectation of a certain result which is realised by the expected result ensuing, though it may be noways attributable to the medicine given, which may have been a mere placebo or an unmedicated globule. Patients have generally a very erroneous idea respecting the gravity of their own complaints. How often it happens that we or our medicines are accredited with almost miraculous powers in having cured a disease our conscience tells us would likely have done quite as well without our aid. And on the other hand, how often are patients profoundly ignorant and incredulous as to the danger they ran in some complaints which we flattered ourselves we had treated in a most masterly manner. A short time since a lady whom I

had treated some months previously for quite a trifling attack of lobular pneumonia, almost caused me to blush, were such an evidence of emotion possible in a hardened practitioner, by exclaiming, "I believe you to be the most skilful of doctors, for last year when you treated me so successfully I was as nearly dead as any person could be." As a pendant to this picture of undeserved praise I may give one of undeserved obloquy. A gentleman whom I had brought safely through the severest case of confluent smallpox it had been my misfortune to witness, and had also treated successfully for caries of nearly the entire half of the inferior maxilla and of about one fourth of the tibia, just when the last small fragment of bone had come away and the last remnant of the opening in the leg was finally healing, got a slight attack of sciatica, and as the remedies I prescribed did not succeed in removing the pain entirely in two days, he wrote a note to tell me that he was quite convinced that homœopathic remedies had no curative power, so he had decided to put himself under the care of an allopathic practitioner. I thought it odd he had not discovered the powerlessness of homœopathic medicines all the time I had been treating him for confluent smallpox and caries of jaw and leg. But so it is with patients, they are often as bad judges of the power as of the powerlessness of the means used, and we should often go very far wrong were we to trust to their impressions. Still these impressions are often the only thing we have to go by in estimating the effects of our treatment, and hence it is that we often give a remedy credit for a curative or a pernicious effect on a malady which is quite undeserved. It is hard to eliminate all sources of fallacy in judging of the value of remedial means from the patient's impressions.

2. Another reason for the discrepancies among practitioners as to the best remedies for many diseases is, that with the best will we do not repeat each other's experiments accurately. "Macht's genau nach!" cried Hahnemann. "Repeat my experiment exactly and you will obtain the same result as I have." More easily said than done. Though we may not diverge so widely from the experiment

we profess to copy as that comic allopath cited by Dr. Bayes in his late admirable work, who applied *Arnica* to an ecchymosis on a corpse and pointed triumphantly to its non-effect as a complete refutation of the vulnerary power ascribed to that drug, yet we do in part fail to repeat the experiment precisely as it was performed by our colleague. So many varieties of disease are included under the same nosological name, that our case which we imagine to be similar may, in fact, be very dissimilar to his. 'Ὅμοιος παθος ὁμοιον φαρμακον, the watchword of the old empirical school, "for like disease like remedy" is undoubtedly, in theory, a correct maxim, but in order that it should prove true in practice we must be sure that we have to do with the like disease before we can be certain that our like remedy is applicable to it. And again, we must be sure that our remedy is the like also; without knowing it we may be giving a totally different remedy, or we may be administering it in an unlike dose, in an unlike manner, of an unlike strength, at unlike intervals, or with unlike accompaniments. If our experiment differs from that we wish to imitate in any of these particulars, in all probability the result will be dissimilar.

There is an old story, often quoted in medical works, about a simple Arab who was cured of some disease by following the directions of a sage of his tribe, and who, having unbounded faith in the remedial means employed, promised an equally happy result to a fellow-sufferer if he would follow implicitly the sage's directions, which were somewhat of this kind. The patient was to recite several verses of the Koran, and at a certain phase of the moon make for a certain spring in the desert; on arriving there he was to make certain obeisances, to lie down on the east side of the spring, raise himself on his right elbow, invoke the aid of Allah, imbibe the water from the stream without using a vessel to convey it to his lips, drink so many mouthfuls, and then turn his face towards Mecca, recite so many more verses of the Koran, and so on through a number of apparently trivial observances, every one of which was essential to the cure. Our modern teachers who quote this anecdote

earn from their disciples a cheap reputation for sagacity by declaring that the spring water was here the sole curative agent, and that all the rest was mere superfluous superstition; in Oriental language—bosh. But it is not clear to my mind that this is the correct view of the case, and, perhaps, the superstitious observances may have had more, or as much, to do with the cure as the water; at all events, no one would pretend that to drink the water of that desert spring out of a glass goblet, when comfortably seated in one's arm chair, would be a fair repetition of the experiment.

In the old system the neglect of the accessories in the repetition of medical experiments has been fatal to the acquisition of any certain experience in regard to the remedial powers of drugs. Thus, one practitioner will announce that such and such a medicine cures such and such a disease, but we find that he has not administered the medicine alone, but in combination with excipients, correctives, and adjuvants, of which he made no account, and others who pretend to repeat his experiments employ other correctives, excipients, and adjuvants according to their own fancy, without a suspicion that they are completely altering the conditions of the experiment. And so it is, though may be in a minor degree, with practitioners of our own school. One announces that this remedy cures that disease "in his hands," and we all set to work and try the remedy in a different dose, a different form, at different intervals, and perhaps in a different stage of the disease, and then we exclaim that it has not succeeded "in our hands," and thereby we unjustly discredit the experience of our colleague, because we have not repeated his experiment accurately.

But there is yet another reason why we are unable to repeat exactly the experiment of a colleague, and that is, that we are unable to place ourselves in the same moral relations towards the patients. "Faith" is usually held, and rightly so, to be a powerful adjuvant to cure in many cases. It is generally said to be faith in the system, but it is more frequently faith in the doctor that is the curative

agent. Now, we cannot inspire the patient with faith in us unless he and we are in some way congenial—"simpatico," as our Italian friends have it. With this quality alone we may often succeed in curing disease almost irrespective of the medicine prescribed; without this quality it will require a very well-adapted remedy to do any good.

Now, though St. Paul talks of being all things to all men, no doctor can be "simpatico" to all patients. How often do we hear one patient saying he cannot bear Dr. X—, he looks so serious over his disease; another, that he is disgusted with Dr. Y—, because he makes so light of his complaint; a third that he hates Dr. Z—, because he only thinks of the medicine he has to prescribe, and neglects all directions about diet and regimen; a fourth, that Dr. Blank fatigues him by his minute directions respecting what is to be eaten, drunk, or avoided; a fifth objects to Dr. Sevenstars without being able to give any reason for his aversion. It is a case of

"I do not like you, Dr. Fell,
The reason why I cannot tell."

When there is no congeniality between patient and doctor, it is by sheer force of medicinal power, or of hygienic or dietetic measures, that the disease is cured.

It is curious to note the differences among doctors in their relations to their patients. These variations are so great that it seems almost like what the good folks would call a providential arrangement to meet the varying humours and dispositions of the patient world. It would be easy to arrange medical men into classes, genera, and species, founded on their manner towards patients, and to many patients in search of a doctor such a classification would be infinitely more useful than their arrangement into graduates of London, Edinburgh, Glasgow, Oxford, Cambridge, &c., or into high and low dilutionists, Hahnemannians, or specificists, or often even into allopaths and homœopaths.

I could not, without running the risk of being accused of indulging in personalities, pursue this subject into further detail, but every one must see that the moral relation in which the doctor stands to his patient must in a measure

influence the result of his medicinal treatment in no inconsiderable number of cases, and help to account for the discrepancies among practitioners in their estimate of the remedial powers of certain remedies in certain diseases.

3. Another cause for these discrepancies I conceive to be the undoubted fact that every curable disease has more than one remedy, many being perhaps equally curable by a great number of remedies. This fact is often lost sight of, and it is not uncommon to see in the writings of homœopathic practitioners expressions almost implying a belief on the part of the author that the remedy he administered in a certain case was the only one suitable for the disease in the whole pharmacopœia. But a very superficial acquaintance with our homœopathic literature, or with the practice of several homœopathic medical men, will convince us that precisely the same cases often seem to yield as readily to one remedy as to another, and to one dose as to another. Of course many of the cases recorded and observed may be such as would have recovered equally well without any remedy, but a large residuum will always remain of cases apparently requiring a remedy for their cure, and which seemed to be cured by a remedy.*

After a lengthened practice we are very apt to fall into a groove of routine in the treatment of many diseases, and a happy result confirms us in our belief in certain remedies for certain diseases. When we compare our practice with another's, we are almost surprised to find that he too has got into a groove of routine differing greatly from ours in the remedies he is in the habit of giving. Each of us is

* I think it will probably be found that diseases arising from a specific cause have a more limited number of remedies than those which may be produced by many causes. Thus, cholera, scarlatina, and syphilis, appear to have a much narrower range of remedies than pneumonia, pleurisy, and typhoid fever. And it will be observed that the success of the homœopathic treatment in those specific diseases, as compared with the best allopathic or expectant treatment, is much more decided than in these non-specific diseases. This means that our medicines are more decidedly remedial in the former than in the latter diseases, though our mortality in some of the former class, such as cholera, may be much greater than in any of the diseases belonging to the latter category.

inclined to credit those remedies we successfully employ with being the only really homœopathic remedies for the diseases they cure "in our hands," until we have the opportunity of comparing our practice with that of others, when we find that they are as convinced of the only true homœopathicity of their remedies as we are of ours, and eventually we all come to acknowledge that other remedies than those we habitually use may be equally good. Unfortunately it is mostly impossible in private practice, and perhaps even in hospital practice, to obtain absolute proof that a medicine does cure a disease, and still more that it cures this disease better than another medicine. I know of but one instance where one can see the remedy cure the disease, and that in such a complete and rapid manner as to set competition at defiance. I refer to Bolle's method of curing acute inflammation of the uvula and soft palate by means of a solution of corrosive sublimate. Here the brush is hardly out of the mouth when we see the whole of the inflamed parts turn pale, and shrink into their normal dimensions, and the patient at once feels that his disease has gone, and can swallow with perfect ease and comfort. Hirsch's cure of whitlow by *Nitric acid* is a one horse affair when compared with this.*

But as the action of all remedies is not provable in anything like this "blitz-schnell" fashion, we can readily understand how pertinaciously practitioners will stick to the

* The only remedial means I know of at all comparable to this in rapidity and certainty is no medicine at all, but the two forefingers of the practitioner in cases of fissure of the anus. I allude to the treatment of this disease by dilatation of the sphincter, first introduced by my old teacher Dr. Maisonneuve, of Paris. I have employed this method in the most inveterate cases; the last one I treated thus was in this hospital; he had been subject to the excruciating torture of fissure ever since he could recollect (he was then about thirty years old). After every motion he had to endure five or six hours of agony; if the bowels were moved in the evening he could not get a wink of sleep all night. Dilatation performed with the two forefingers introduced within the anus and kept up for about one and a half minute cured him thoroughly and instantaneously, and, to his great astonishment, he could pass a costive motion without the slightest attendant or subsequent suffering. I have elsewhere recorded cases of similar rapid cure of equally severe symptoms of fissure; but in the above case the disease was of longer standing than I had previously met with.

remedies that have hitherto seemed to them to be curative, and which I have no doubt are actually so in very many instances, though all may not agree as to the remedies most suitable for each case.

I have in the foregoing mentioned a few of the causes of the discrepancies observable in the writings of homœopathic authors with respect to the remedies for morbid states. There are many other causes doubtless, but I will not occupy your time in searching for others. A more important point would be to discover how those discrepancies can be removed, and how we can succeed in ascertaining the absolutely best remedy in each case. Our *Materia Medica* and repertories evidently do not suffice. The symptoms recorded being chiefly of a subjective character are seldom so perfectly definite as to allow us to be sure we have their exact counterpart in our patient, even though he may use the exact words of the *Materia Medica* in the description of his sensations. The symptoms of the provers—mostly intelligent men—deal largely in the character of pains, but every one knows how difficult it is to get a patient of ordinary stupidity to characterize his pain at all distinctly. He will say it is horrible, agonising, distracting, maddening; but if we ask him for a more distinct character ten to one he gets angry and exclaims impatiently “A pain’s a pain, and if that is not enough for you I can’t describe it better.”

The obvious difficulty of fitting the morbid sensations of our patients with their similia in the *Materia Medica* has elicited many plans for, if not precisely dispensing with, at least of evading a solution of this difficult problem. The latest of these royal roads is the plan of “key-notes,” as they have been called—symptoms, namely, sometimes objective, sometimes subjective, sometimes merely conditions of symptoms, which when observed in a patient are to guide us certainly to the appropriate remedy that will assuredly have all the other symptoms of the case in its pathogenesis, or if it has not that is only because its proving is incomplete. These “keynote” symptoms are sometimes taken from the proving of the medicine, but very frequently are

only drawn from clinical experience and are not to be found in the pathogenesis, and sometimes they seem to be merely evolved from the inner consciousness of their discoverer. In all periods of homœopathic practice such "keynotes" have been sought for, and many have been given us by Hahnemann himself, but recently, and especially in America, they have cropped up much more abundantly, and in very many instances it appears to me that they have been selected rather arbitrarily and without sufficient foundation. Some of them are undoubtedly useful and are truly keynotes, especially those given us by the founder of our system, but many of those recently recorded seem to me to be rather of the character of false notes, and to be of little or no practical value.

A more hopeful method of discovering the true medicinal simile appears to me to find a real objective pathological connexion between drug and disease. If we know of a true pathognomonic symptom of the disease and its counterpart in its medicinal proving we may confidently expect this drug to be the real remedy for that disease. Much has already been done towards this, but very much remains to be effected. The progress of pathological knowledge daily reveals more and more of those pathognomonic signs which we should carefully register as our best and surest data for practice, and the earlier and later provings of our medicines will often show us the exact analogue of these pathognomonic signs, and thus enable us to discover true and reliable keynotes. In connexion with this subject I may mention Dr. Bolle's alleged discovery that in almost all cases of hooping-cough he has found on one or both sides of the frænum of the tongue a small superficial ulcer. If this be a pathognomonic sign of hooping-cough may it not be the true indication or keynote for the administration of *Nitric acid* in that disease in which it has recently been found so efficacious by practitioners of both schools, *Nitric acid* having as you are aware a special power of causing ulceration and soreness in the mouth and tongue.

In this paper I have merely touched on many points which I might have treated more in detail had I not feared

to trespass too much on your patience, which doubtless I have already sorely tried, by the length this paper has already extended to. I suspect that in my weak endeavour to prove that Hippocrates was right when he declared our *experientia fallax*, I may have too forcibly reminded you that he was equally right when he pronounced our *ars longa*.

Discussion on Dr. Dudgeon's paper.

Dr. R. HUGHES, after expressing his warm appreciation of Dr. Dudgeon's paper, said he would comment on two points of interest contained in it. He did not think that the discrepancies in the recommendations of different writers were any discredit to homœopathy. When examined, the various medicines prescribed would generally be found to belong to the same group, and it was just the accident of the practitioner's experience which led him to prefer one to the other. That natural variety of the disease which corresponded to the given member of the medicinal group had come most frequently under his notice, and hence he had been led to connect malady with remedy. Believing that such correspondence always existed—that the law of similars rested on a pre-established harmony between disease and drug action—he (Dr. Hughes) could not think with Dr. Dudgeon that there might be several equally good remedies for a given case of disease. There must be (he thought) one precisely homœopathic, and therefore perfectly curative, though others might be nearly so. This was the great use of the minor and new medicines. They filled gaps in the action of the polychrests. One of the latter might fairly help the patient in every malady, but it would have its weak points as well as its strong ones, and these were just the vacant niches left in the building for medicines of more limited range to fill.

Dr. BAYES said it was very much to be desired that we could discover remedies for concrete diseases, it would greatly simplify medicine if such were possible; to some extent we are approaching this greatly to be desired goal. We have already series of remedies with which to meet the varieties of specific diseases, and in time our knowledge of remedies may include still more absolute specifics. It is often very difficult to know whether our remedies are really acting, or whether the disease is simply passing off, or whether it is only changing its "venue." In a recent case of neuralgia he had seen an example of the latter type. A patient with violent gastralgia, after having tried many means, lost her gastralgia at once on taking *Bismuth* 1, but after some hours of ease, enteralgia of the most violent form set in; this

yielded to *Colocynth* 1st, but in some hours an equally terrible neuralgia, semilaterally affecting the head, and afterwards spreading over the forehead, set in; the patient was so agonized that she could not describe the pain for some hours. After a time, during a lull, she said it was like nails being driven in. *Ignatia* 1 relieved this, but then hystericalgia equally acute set in. After obstinately resisting many remedies, *Dioscorein* relieved the uterus and its appendages at once, but only to again drive the pain elsewhere. During all this time there was obstinate constipation with scybalous masses of whitish colour passing, when an enema or castor oil were used. Neither *Mercurius* nor *Nux* did the slightest good, but *Podophyllin*, one eighth of a grain, set the liver in action, the evacuations assumed a healthy colour, and the neuralgias ceased to trouble the patient. In this case the true symptomatic indication for treatment was not the neuralgia, but the inactivity of the eliminating function of the liver. He (Dr. Bayes) must also allude, for a moment, to Dr. Dudgeon's remarks on the relative statistics of pneumonia when treated homœopathically or by expectation. Now it is perfectly true that the *death rate* in both cases is nearly similar, which proves that the disease is not one of necessarily large mortality, but the question is, Do we shorten the duration of the disease by our remedies? and this has been abundantly proved to be the case by both Tessier, Henderson, and others. As to faith, no doubt it is a great thing to have the faith and confidence of our patients working with us; but it is quite possible to cure without appealing to further faith than that which will ensure the taking of the medicines and obedience to the rules we lay down. One general argument in favour of the utility of faith is to be found in the greater difficulty often experienced in the treatment of extremely deaf patients; but this may be partly explained by the difficulty which their deafness causes to our full questioning them as to symptoms, &c. As to routine treatment, he (Dr. Bayes) thinks it is to be found far more often in the allopathic than in the homœopathic ranks, of which he cited instances.

Mr. J. H. SMITH thought that there was one part of Dr. Dudgeon's paper which needed guarding, or it might give the impression that he thought "the art of the physician consisted in amusing the patient while nature cures the disease:" an adage which he in bygone days had often heard pressed—*usque ad nauseam*—amongst practitioners of the old school. The pretence of curing cases which would probably have got well if left to themselves was, no doubt, more disgusting still; yet, if there were a rapid improvement in a disease which was not tending towards recovery, or which had resisted other treatment, he thought we were entitled to write it down as a case of *propter hoc*, and not merely *post hoc*.

Dr. HAUGHTON said that when he was a student attending the allopathic hospitals he noticed that in many cases which turned

out badly, and in some which died, the remedies administered had actually relieved the symptoms which they were prescribed to combat, showing that it was results rather than causes which were influenced by such prescription. He thought that perhaps many homœopathic prescriptions were open to the same objection, and that in many cases it was doubtful whether there were any specifics from which more could be expected. It was frequently objected to homœopathic treatment that it was not directed to the totality of the disease, but only to the *totality* of symptoms *actually existing*. Some discrepancies may be explained by supposing that the results obtained by first observers in the use of particular drugs may have been only in particular stages of disease; whilst those who repeated their experiments expected such results in all stages of the same diseases. Dr. Haughton also mentioned the influence of faith in producing particular results, in the case of an Englishman travelling in Bolivia, whom the natives required to prescribe for them, and who cured all their complaints with a common effervescing mixture.

Dr. HALE.—The paper which Dr. Dudgeon had read, he thought, contained some useful suggestions, and it is probable that in the early days of homœopathy in England we may have overestimated our success, inasmuch as many cases which came into our hands after drugging were benefited by simply leaving off the injurious treatment by drugging. Then, again, it may be admitted that there are some acute diseases whose tendency is to spontaneous recovery, if the patients are placed in such favorable circumstances as to aid in recovery; but after fairly allowing all that can be urged in support of this argument, there still remains a considerable residuum of acute and chronic diseases, whose tendency is certainly *not* to spontaneous cure, that the number of those which do so recover is limited, and forms the exception rather than the rule. Of acute diseases Dr. Hale would instance acute inflammatory *Croup*, *Peritonitis*, *Cerebritis*, &c. &c., and, with regard to chronic diseases, every homœopathist as well as himself could honestly claim for homœopathy an amount of success in cases which had resisted allopathic treatment and no treatment for years. If this success and the superiority which homœopathy fairly claimed were not real and true, then the experience of the last thirty years or more is a delusion and a snare. But this we are not by any means forced to admit; on the contrary, in the face of all that can be said to explain away the success of the homœopathic treatment by the theory of cure resulting from the unaided efforts of the *vis medicatrix naturæ*, Dr. Hale contended that homœopathy is fully justified in the claims it puts forth. With regard to the study of the natural history of disease, which is so much advocated by modern allopathic writers, there is no thoughtful homœopathist who would be adverse to such a study when compatible with safety to life. In noticing some observations just

made to the Society by Dr. Hughes, Dr. Hale agreed with him in believing that in some cases it was absolutely necessary to find the true *similimum*, and this held good, especially in the case of medicines having special action upon the urinary organs, instancing a case of very obstinate irritation of the neck of the bladder which resisted the action of several of the ordinary urinary medicines, but yielded promptly to *Copaiva*. The diversity in practice and the failures are not owing to any insufficiency in the therapeutic law, but, in not a few cases, arise from our failing to discover some simple local cause of irritation such as that caused by a carious tooth, for instance, producing most violent neuralgic pains; in one case related by Dr. Hale, and in another well-marked symptoms of intermittent fever were produced; in both cases the cure was immediate upon the removal of the local cause: such cases occur to almost every one in practice. Touching the efficacy of faith as a means of accounting for the success of any treatment, without denying that it must be allowed to have some share in the cure in a more or less numerous class of cases, such aid to cure in the case of young children and the brute creation must be entirely eliminated from the argument, for in none of the subjects which come under homœopathic treatment is the success greater than in the diseases of children and dumb animals.

Dr. ROTH hoped this would lead us to study the natural history of disease, and preferred, if convinced that medicines are not necessary, to mention this to the patient that he may trust to hygiene; he did not approve of giving nostrums, and believed it desirable that young practitioners should be taught to rely less on their power of curing merely by medicine.

Dr. YELDHAM suggested that the wide differences which prevailed amongst medical men in respect of the dose might account for some of the discrepancies in practice. The high dilutions and the mother tinctures of the same medicine could hardly be regarded as the same agents. He hoped the day was not far distant when they would be more in accord on the dose question, and then the results of their practice would admit of more exact and useful comparison. Another source of divergence of practice was, he considered, the frequent changes of remedies. This arose in a great degree from forgetfulness of the natural course of diseases. They were too anxious to obtain sudden and impossible results, and hence rapid changes or alternations of remedies. No medicine was allowed time to exert its natural and proper action, and thus men obtained unsatisfactory and uncertain results, and were led to false conclusions as to the effects of medicines. Dividing diseases into three classes would enable them to arrive at a fair estimate of the value of treatment. First acute diseases. In these, whilst they all admitted their strong tendency to spontaneous recovery under favorable circumstances, it was equally to be admitted that homœopathic treatment con-

trolled their course and progress in a most marked and beneficial way. Secondly, chronic functional diseases. This was the great field of homœopathic triumphs. Many of these diseases had little or no tendency to spontaneous recovery, but were splendidly amenable to homœopathic specifics. Thirdly there were the organic and malignant diseases, heart and lung disorganisations, cancer, and the like. It was no disgrace to homœopathy to fail to cure these cases. They were at present notoriously incurable by any known means. He must add one word on the pathological question, which was now made the fashionable basis of their treatment. Whilst he fully admitted that it was exceedingly satisfactory when they *could* prove an agreement between the physiological action of their medicines and the pathology of the disease, it was notorious that in the majority of cases the structural changes going on in disease were totally undiscernible. They were compelled to rely on that safe guide, the similarity of the subjective symptoms, and even when the pathology of a disease was well known it only went to prove the truth of the doctrine of similars. Only a few days since he had had a striking proof of the value of symptomatology. A patient applied to him for advice who had been for months under allopathic treatment for obscure renal disease. The most prominent and distressing symptom was the vomiting of all kinds of food, by which he was reduced to the most fearful state of exhaustion. Laying hold of this symptom he prescribed *Antimon. tart.* in five-grain doses of the first centesimal every four hours. After the third dose he ceased to vomit and could retain and digest food comfortably. How far this remedy might ultimately affect the whole diseased state he could not say, but he knew of no other system of medicine by the light of which he could have afforded his patient this amount of comfort, viz. by the light of the symptoms irrespective of structural pathology.

Dr. MADDEN had been much interested by Dr. Dudgeon's paper, and entirely agreed with much that he had brought forward. He also agreed with Dr. R. Hughes, that *theoretically* there must be one medicine, *par excellence*, for each case of disease; practically, however, he believed that a large number of cases were curable by several allied medicines, and that consequently medical men often differed by having been accidentally led, in the first instance, to choose a given remedy, and had continued to use it, as as they were satisfied with the results. The same remarks will apply to the use of different dilutions. One practitioner tries a high dilution, and being satisfied with the result continues to prescribe it, while another employed, in the first instance, a dose but little removed from the crude drug, and having cured his case was for the future guided by his own experience. Thus differences which originated accidentally became stereotyped, and each practitioner is confirmed in his own method of practice.

Dr. DRURY, V.P. (in the chair), felt the importance of recognising the fact that many causes, such as emotions, discomforts, &c., influenced the progress of disease besides treatment, in some cases beneficially, in others injuriously. The liking or disliking a doctor no doubt exercised an influence, but as it was generally by the patient's own desire that the doctor was called in, any feeling from liking or having confidence in him would be in his favour in treating the patient, though the fact that some patients were subject to whims and caprices, and that the favoured doctor of to-day may be the reverse to-morrow, for no very intelligible reason, should not be forgotten. Happily many patients were very true to their medical attendants, and did not forget the attention they had received, so that the friendly relations existing between doctor and patient had a mutual advantage. Though it was very true that in many cases of illness there was a natural tendency to recovery, yet when the duration of disease was shortened, when a particular line of treatment was pursued, or when a medicine was chosen in consequence of some particular symptom that yielded on the administration of that remedy, the evidence, of course, became very strong that the treatment had been effectual. Dr. Hale had laid stress on the fact that when children were admitted into hospital they had the advantage of good nursing and good food that materially aided their recovery. These things were, no doubt, excellent in their way, and when possible every advantage should be taken of such auxiliaries, but he (Dr. Drury) could testify to the immense advantage of homœopathic treatment in diseases of children, uninfluenced by the valuable aids Dr. Hale had spoken of. He frequently had to prescribe for children where comforts were unknown, who nevertheless got relief unaided by good food or good nursing; this sometimes happens where he could not persuade parents to let their children become indoor patients. Allusion had been made to rapid cures effected by well-known simple means in certain cases. He often wished to see a collection of such simple remedies, and amongst them he would include *Phosphorus* dissolved in oil in the proportion of a grain to the ounce, as an invaluable remedy for relieving the pain of whitlow. As the liniment was applied with a brush to the hot throbbing swelling, the pain seemed to be wiped away. He had used this same remedy with advantage in some larger abscesses. When the abscess was very large more care was required in its use. The *Tinctura Pyrethri*, or a piece of the root applied between the gum and cheek, when there was severe toothache, would cause a copious flow of saliva and often procured instantaneous relief. In like manner a drop of tincture of *Arnica* applied to the hollow of a painful decayed tooth would frequently succeed in at once removing the pain. Dr. Roth suggested that when there was no urgency for medicine being given we should dispense with it, and resort to hygienic means. Any doctor adopting such a universal

rule would speedily find his patients looking elsewhere for medical aid, besides the advantage gained from the patient having confidence in his remedies would be lost, so that we often would do him a positive injury. While acting with all fairness to our patient and refraining from unnecessary dosing, and appealing to his intelligence where we thought desirable, we should still hold our hands free to give medicine or withhold it, or do whatever would conduce most to our patient's benefit.

A FEW CHARACTERISTICS.

By ROBERT T. COOPER, M.D.

(Read before the British Homœopathic Society.)

THE remarks in a paper with this title ought to be, I take it, before all things *practical*, and secondarily *pithy*, and concise. I shall, therefore, in the sequel endeavour to comply with these requirements.

The first case we have illustrates Harley's remarks, that *Conium maculatum* produces a want of adaptation on the part of the adjusting muscular apparatus of the eye-ball, leading to a confusion of vision and a sensation of bewildering vertigo.

Sarah F—, æt. 12. Is long sighted; she has to hold her school books at a considerable distance from her when engaged in reading. She has a pulling sensation in both eyes, they feel as if pulled outwards from the nose, and with a force dragging in the direction of the back of the head. The eyes are very sensitive to light, and she complains of a vertical pain in the head, which is exacerbated when in the open air. She has often to leave school from a sensation "coming over her" of overpowering giddiness. These symptoms have been increasing for the last few months. Is of a relaxed habit. Ordered *Conium maculat.* 3^x.

2nd week.—The headache is much improved and her

appetite is better. The pulling sensation is not complained of. Repeat.

3rd week.—The headache, vertigo, and pulling sensations have quite ceased, and she is less long sighted.

The disorder of the adjusting muscular apparatus indicated by the pulling sensation (*external rectus muscle*) was combined with probably an irritable condition of the optic nerve, to which latter the photophobia may be ascribed; while the premature presbyopia, an affection for which, if memory serves aright, Dr. Dudgeon recommends *Conium*, might be referred to the action of the recti muscles upon the globe of the eye.

We have a symptom in Hahnemann's proving of *Conium* that somewhat bears out Harley's observation. I mean a "tremulous look as if the eyes were trembling; the symptom would point to a chronic affection of the adjusting muscular apparatus. Allied in many respects to the symptoms of *Conium* are those of *Gelsemium*. In the orbital sphere we find both causing a heaviness of the lids, dimness, diplopia, and confusion of sight. *Gelsemium* further causes a *squinting* and "a disposition to partially close the eyes as if to *steady the balls*." And yet *Gelsemium* appears to settle chiefly upon the retina; *Conium* more upon the adjusting muscles of the eye. *Gelsemium* produces a *thirst for light*; *Conium* invariably a *dread of light*. Correspondingly in the aural and nasal groups *Gelsemium* causes a dulness of hearing and smelling; *Conium* a sensitiveness of both.

The next case illustrates Teste's assertion, and was chosen, relying upon it that a child afflicted with congenital syphilis is almost always benefited by *Kreasote*.

M. A. T—, a girl æt. 9, presenting well-marked traces of syphilitic dyscrasia, was brought by her mother to the dispensary on account of deafness, which has afflicted her all her life, but increasingly so of late.

She had the snuffles when a baby, and the teeth present that well-known and characteristic feature of congenital syphilis, a wedge-shaped form; besides, she is very old-look-

ing for her years. She is subject to attacks of vomiting, but except for this the general health is good and the bowels are regular. *Kreasote* 3^x was given, and the hearing gradually improved, the vomiting ceased, and after six weeks' attendance she was discharged *quite cured*.

I cannot say my experience warrants an expression of opinion as to the value of *Kreasote* in hereditary syphilis.

A glance at the proving of *Capsicum* will show that the principal symptoms occurring in the extremities are myalgic. "Stiffness of the nape of the neck, diminished by movement." "Pain in the muscles of the thigh, resembling an aching and as if the parts had been strained." "Convulsive jerking and twitching, now of the thigh, now of the lower (? fore) arm," besides many others pointing in the same direction. It affects the skin, causing a slight itching. "Itching of the hairy scalp and of little places all over the body." It affects the rectum, causing *tenesmus* and a "hæmorrhage from the anus for four days."

We are now prepared for the following:—

Eliz. C—, æt. 33, of a florid complexion and freely perspiring skin, came under treatment in July, 1869.

She had been treated a year previously at the dispensary for nervousness. She now complains of severe pains about the hips, arms, and chest, with fluttering in the abdomen and at the breast—symptoms that are aggravated by excitement. Her skin itches and is very rough. The bowels are regular, but there is passed with the stool a great deal of bright blood. Urine is clear, and the appearance of the tongue, natural. *Capsicum* 3^x was ordered, in seven dessert-spoonfuls of water, a dessertspoonful three times a day.

This was continued with one week's intermission for a month, at the end of which time the above train of symptoms had quite left her.

I have cured cases of gonorrhœa with *Capsicum* alone, giving it by the mouth (the 2nd dec. sol.) and by injection *per urethram*. Duration of treatment, a month.

To a patient, a woman of a decidedly nervous temperament, aged 42 years, a constant sufferer from neuralgia, I gave

Helleborus niger, 3rd dec., for a sudden general swelling of the abdomen ; in a week the swelling disappeared, as well as a severe attack of neuralgia, for which *Belladonna* had been unsuccessfully prescribed a few days previous to the swelling appearing. The pain was very severe, extending all down the neck, and the left side of the face and teeth, and the parts were very tender, preventing her from chewing her food. The pain was of a tearing racking character, being much worse during the day and early in the morning.

We have but few symptoms recorded of *Helleborus* ; the neuralgic ones are particularly scanty. The following is the only one *tending* to corroborate the above :—" When biting the teeth together he feels a pain in the third molars, which are opposite each other near the roots."

We often gain valuable hints from apparently trivial observations like this, and such facts ought not to be allowed to pass unnoticed. *Hellebore* is allied in the general sphere of its action to many of our valuable neurotics. A peculiar feature in its headache is that scalp symptoms are nearly always present.

The following case of brain disease shows the power of *Hellebore* to control cerebral symptoms.

A child of eighteen months was brought in the nurse's arms to the dispensary, looking pale and lifeless ; his features were drawn, and the eyes rolled in their sockets. He was unable to partake of food ; as the nurse expresses it, " the stomach turns at it." For three weeks after he began to be out of sorts, his bowels were very much disturbed, but now they are seldom moved. He is constantly picking his lips and clothes, and he keeps continually moving his arms, except when asleep. He is very drowsy, and falls asleep at once if aroused. Although he seems hot and feverish the natural colour has quite left his face, and he never flushes. He was a strong and healthy child before these symptoms showed themselves, but is now extremely weak. *Helleborus* was prescribed in the first decimal, and I was much pleased in finding three days afterwards that a marked change had taken place, the child looked cheerful

and intelligent, and the appetite restored. He was not allowed any medicine for the next week, and consequently retroceded a little, symptoms of irritation hung about, such as turning up of the eyes, &c., and consequently *Hellebore* was again given, this time in the 3rd decimal, and with the desired effect. After taking it a week he was put upon *Sulphur* for a slight debility, with want of sufficient sleep, and a flabby condition of the muscular fibre that remained; this was his last attendance at the dispensary.

Stupor, with the eyes half open and the pupils turning upwards, obtuseness of the sensitive nervous system, and a general listlessness, must count among the prominent characteristics of the case. The continual *movement of the arms except when asleep* is noteworthy—if a symptom of *Hellebore* it will help us to differentiate between this and *Hyoscyamus* where cerebral symptoms prevail, for with *Hyoscyamus* the movements of the arms occur when the patient is asleep. In children particularly objective phenomena like this demand attention.

On the principle that we should take care not to let pass any observation calculated to throw light upon the action of any remedy I subjoin the following.

Albert Colborne, a boy of three years, treatment begun 7th February, 1868.

This little fellow was subject to extraordinary epileptiform seizures, which come on every three months or so without the slightest warning, and last for two or three weeks, leaving him at the end of this time completely prostrate for several days. At a moment's notice he falls down with vertigo accompanied by sickness; he lies like one dead. In some attacks the sickness keeps away for several days, but if this be the case, instead of the natural contents of the stomach he will bring up quantities of a green foetid fluid and will pass green motions as well.

As long as the attack lasts he can drink but cannot eat anything, and he lies in a drowsy, semi-comatose state, at times being plunged into convulsions, during which he grinds his teeth.

The last attack he had was at Christmas, and he is now expecting another.

Hydrocyanic acid in the 12th was given, and up to the end of March he continued to improve; then there set in a dry cough, similar to one which immediately precedes the seizures, but in spite of which the *Prussic acid* was repeated, with the happy effect of warding off the threatened attack. He left the dispensary quite well.

The symptom determining my selection was the grinding of the teeth, and my belief in the appropriateness of the remedy was founded upon Geoghegan's paper, "On poisoning by *Hydrocyanic acid*, in *Dublin Med. Journal*, vol. viii, p. 311. He thus expresses himself:—"I have frequently observed in animals to which this acid had been administered that they perform rapid motions with the mouth and jaws as if a powerful impression had been produced on the nerves of taste."

Whatever the cause of this symptom, may not it, and what we may suppose to be its counterpart in man, grinding of the teeth, be characteristic?

Geoghegan puts it down to a powerful impression made upon the nerves of taste, a theory that he says is borne out by the fact that an insupportably bitter taste is among its symptoms.

Supposing grinding of the teeth or, at all events, and more certainly, movement of the jaws, a genuine symptom, may it not help us in fixing upon *Hydrocyanic acid* many cases of nervous affections where the other symptoms are to be met by it rather than by *Cina*?

Discussion on Mr. Cooper's paper.

Dr. R. HUGHES, while appreciating the valuable and suggestive information contained in Dr. Cooper's paper, thought it would have been better suited for discussion had fewer different points been brought forward. As it was, the paper was difficult to follow and still more difficult to discuss. He would, however, make a few comments upon its subject-matter. The comparison between the action of *Gelsemium* and *Conium* he thought apt: but he

would suggest to Dr. Cooper whether the paralytic symptoms of *Gelseminum* (especially those of the eye) were not secondary to the cerebral congestion induced by it, while those of *Conium* were primary. He thought the word "myotic" better suited for *Capsicum* than "myalgic," as all Dr. Cooper meant was that it directly affected muscular fibre. He was sorry that Dr. Cooper had said anything about the tooth symptoms of the *Materia Medica* in reference to *Helleborus*. It seemed to him leading us astray to point to these as having anything to do with the disappearance of a neuralgia under its use, when there were abdominal symptoms present to which the drug was homœopathic, and of which the neuralgia might well have been sympathetic. So also with the *Hydrocyanic acid* case. The epileptic symptoms were quite sufficient to prove the specific relation of the drug to the disease; and he could not think that the movements of the mouth observed in the case of poisoning cited were in any way analogous to the "grinding of the teeth" so well known in children's maladies. In conclusion he hoped that his difference from Dr. Cooper on these points would not be understood as disagreement with him on the whole—the reverse of this sentiment being rather entertained by him.

CASE OF SPINAL FRACTURE WITH COMPLETE DIVISION OF THE SPINAL CHORD.

By THOMAS ENGALL, M.R.C.S.E.

(Read before the British Homœopathic Society.)

J. W. K—, aged 23, was admitted into the University College Hospital, under the care of Mr. Liston, July 10th, 1843.

He fell on that day thirty feet from a scaffold upon some kerbstones. He came down upon his belly, and the bricks from the wall he was pulling down fell upon his back. He at once felt that he could not move his legs or his right arm. He was brought to the hospital; on admission, the lower limbs paralysed; and the body, as high as the umbilicus; no sensation in these parts; bladder paralysed. There is a projection about the tenth or eleventh dorsal vertebra; on the

right side below this a depression ; the projection appears to be caused partly by extravasated blood. The right clavicle is broken. The head was not struck in the fall. He was placed in bed, horizontal position, and urine drawn off by catheter, acid ; passed his stools involuntarily. He remained in much the same state for the next ten days, about which time, July 20th, he gained command over the bowels, and passed his evacuations properly ; also some power over the bladder ; about 8 ounces could be retained, but when this was exceeded, it was passed away ; he was conscious of its expulsion, but had no power to control it ; the urine now became alkaline.

He went on in much the same state till the 30th ; complained only of the soreness of his back, when he was attacked by pleurisy, which yielded to bleeding (local), &c. He remained in much the same state till the 30th of September, when he was discharged.

He then came under my care and was laid upon the spinal couch I usually employ. The external condition of his back will be best understood by this cast (shown), which was taken as soon as some sacral sloughs had healed. He was rubbed daily with olive oil over the spine and frequently over the paralysed limbs, to which cause I attribute the general good health he had until within a short period of his death, which took place on July 22nd, 1869 ; he thus lived twenty-five years and a half with perfect inability to move the lower limbs, to retain the urine, or to void consciously the contents of the lower bowel ; perfect loss of feeling and of motion from the umbilicus downwards existed all this time. To what was this loss to be attributed ? of course it was due to some pathological condition of the spinal chord. As the deformity was not greater than that which I had met with in cases of paraplegia, which had been removed by restoring the spine to its natural form, I surmised that such might be the result here, but before the case came under my care ankylosis had taken place, and all my efforts in this direction were, of course, unavailing. I therefore placed him under the most favorable condition for preserving his general health, and I think it was no small triumph for

this treatment that he lived twenty-five years after it was adopted, especially as you will here see in this section of the spine that the chord must have been severed. By the fall

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the body of the twelfth dorsal vertebra was fractured obliquely from behind forwards and downwards, and the contraction of the psoas muscle, which has its attachment to the last dorsal and to the first four lumbar vertebræ, caused the upper portion of the fracture to slide forwards and downwards upon the lower incline formed by the other oblique portion of that body, and kept it there, thus shortening the spine, and causing, by the displacement, destruction of that portion of the chord compressed within its two portions, the subsequent deposit of bone for an inch in length closing up this part of the vertebral canal and

entirely separating the upper and lower portions of the spinal chord, or of that prolongation of it, the cauda equina, although, as a rule, this division does not take place until the spinal chord reaches the first or second lumbar vertebra. Such being the condition of the parts, how was it that the patient existed so many years? part of the good health he enjoyed was due to the friction daily employed, which, by circulating the blood, prevented the formation of sloughs. Urea was prevented entering the system probably by the constant dribbling of the urine which was taking place; the bowels, as was to be expected, were obstinately constipated. It may be a satisfaction to some of us who have cases of this kind to treat that no injury appeared to arise although the bowels were only relieved once in three weeks, by which time the distension of the colon reached above the line of loss of sensation, and then, the desire being felt, a purgative administered acting above the point of obstruction and rousing there the peristaltic action, aided by copious and oft repeated glysters below, produced the desired effect. Towards the latter part of his life his liver troubled him, and he had several attacks of bronchitis; he ultimately died of congestion of the brain.

I regret exceedingly that I cannot find my notes of this case, and have, therefore, to speak from memory. In electrical conditions of the atmosphere his legs would plunge about involuntarily; this would also be the case when he suffered from severe inflammatory affection of the brain. In this latter case it would appear as if a communication were established between the cerebral spinal centres, by a channel uniting the divided ends of the spinal chord. If this were effected it must have been by the branches sent to form the lumbar plexus through the first, second, third, and fourth lumbar foramina below the division, but chiefly by means of the communicating branch sent down from the twelfth dorsal above it, which would bring it into connection with that plexus and with the divided portion of the chord above. But, on referring to the spine, we find that the foramina, through which the twelfth dorsal nerves passed, are closed entirely by ossific deposit; hence this communication

could not exist. Neither can we suppose that if the ganglia at the junction of the anterior and posterior roots were affected by the electrical condition of the atmosphere, these acting upon the sympathetic would form a means of communication, seeing that the functions of the latter are different from those of the spinal chord, and we cannot conceive that even great cerebral excitement could make the sympathetic ganglia take on motor action.* That the legs should jump in electric conditions of the atmosphere was due to reflex action. In their usual condition, however, no motion of the legs took place, and no sensation was felt in them ; here we have, therefore, almost a complete demonstration that these two functions depend upon the continuity of the spinal chord with the brain being preserved.

Again, does not this case show that the nutritive function must be under the control of the sympathetic ? for, although the spinal nerves supplying the organs which secrete the nutritive material arise above the point where the division of the chord took place, yet the power which the sloughs possessed to heal, and to continue so healed, as well as the fact that the parts below the line of demarcation of sensation and motion did not waste away entirely or take on destructive action, must be due to the sympathetic carrying out the nutritive function, for, as we have already seen, there was not even the communicating branch from the twelfth dorsal to the first lumbar to carry on any communication between the divided ends of the spinal chord as the foramina through which they passed were entirely obliterated ; the only communication therefore would be by the sympathetic, which, lying anteriorly on the bodies of the vertebræ, would be out of the reach of compression, and which, therefore, became the only means of transmission of the nutritive power.

In the case before us the fracture through the body of the vertebra is oblique, and this (as far as my experience goes) is what usually occurs where the force does not fall directly at right angles upon the vertebræ, crushing them beneath its weight. In one case where a cervical vertebra was fractured, and where the man died within the usual diagnosed

* See postscript.

time, I found the chord apparently uninjured, the fracture of the body of the vertebra oblique, and by gentle extension the parts could be replaced, the anterior and posterior ligament of the bodies of the vertebræ acting as a kind of splint. Now, had this extension been made during life ossific deposit might have united the fracture, and in all human probability the man's life might have been prolonged.

That such union is possible was proved in the case of a youth who fell from a tree seventy feet high, alighting, as he descended, upon several branches, which, by breaking with his weight, without injuring him, lessened the momentum of his fall; he reached the ground; a fractured vertebra was the result; he was deprived of power in the lower limbs; but after a considerable time spent in the recumbent position, anchylosis took place, and the lost power was regained.

This case shows that fracture of the vertebræ is not necessarily so fatal as to forbid any attempt at cure being made. In every case, I think, that extension of the spine should, at least, be tried, and if the smallest benefit result, the parts should be kept extended by appropriate apparatus.

P.S.—In compliance with the wish expressed for more definite information upon the case of J. W. K—, especially as regarded the spasm of the legs when the brain was affected, I called upon the widow and gleaned the following additional particulars, which I give mainly in her own words.

For the first three days after he went to the hospital he had feeling in one foot, it then became restricted to the great toe, and, finally, left him altogether.

She was compelled to be careful not to touch his feet, as they would immediately be drawn up. In a thunder storm he would shake all over, and his head then became painful. At other times she knew when his head was going to be bad from a blue streak which appeared down his forehead; when this vein appeared he had dreadful headache and pain in the part of the back that was injured; sometimes this lasted for three days, not in the part where there usually was no feeling, but where the feeling was, it was increased in intensity; then the legs would be drawn up and down for an hour or two; if he was turned upon the right side that position afforded some relief. The line of feeling was lower down on the right side than it was on the left.

In the general shakings (such as those produced by the thunder) his arms never moved as his legs did. The legs were often affected

without the head aching, and often the head would ache without any motion in the legs. The spasm of the leg was never known to occur in sleep. In the November preceding his death he had a kind of fit, lost his senses for five or six minutes, and after that his right arm would shake so that he could not write or crochet* so well as he had done before; but my impression is that this was, however, from weakness, since the arms never plunged as the legs did.

For twenty years he lay comfortably, but the last five the legs wasted much more, and during this time his wife had to use great caution in cutting his toenails, as they would burst out bleeding from around the nails, even when she had previously softened them by the use of warm water,—a proof that the nutritive power suffered, as by increasing years the vital powers had less force to contend against the fearful odds to which they were opposed.

In this case the legs were never affected with spasm during sleep. I have seen the same in other cases of spinal distortion; in one the convulsive movements never ceased except during sleep. May I ask my colleagues to observe whether this be a general law? if so, it would form a diagnostic symptom of value regarding the question whether the brain or spinal chord is the seat of the irritation in cases of spasm, and would lend increased importance to the distinction observed by Dr. Cooper between those medicines which induce spasm during sleep, and those which produce them when awake; the former acting probably on the brain,—the latter, probably on the spinal chord.

The only other remark I would make is, that although the spasms of the legs at times existed with severe pain in the head, yet the result of further inquiries leads to the conclusion that they probably were not correlative. Though mental worry would apparently bring on the spasms, yet hiccough and shortness of breath did not accompany them, and sickness was not a uniform attendant upon them.

* To such an extent did he excel in this art, that at the Exhibition at the Agricultural Hall, Islington, he was awarded the second prize, the first being given to a blind girl.

REMARKS ON THE DOSE.

By FRANCIS BLACK, M.D.

(Read before the British Homœopathic Society.)

THESE remarks I do not reprint, as they are merely a resumé of the paper read to the Society in May,* and I proceed to discuss the observations of various members, merely stating my thesis. The therapeutic action lies so near the limit of physiological action that *the doses extending from the crude substance to the 3rd dilution fulfil all the requirements of practice*. I need not allude to the remarks of various members who agreed with me, but limit myself to answering the objections which were raised.

1st. Question of dynamization. I believe that this theory is unscientific, and that as an hypothesis it has done great harm to the cause of homœopathy, and therefore the word ought never to be used in practically studying the dose. As an instance of the confusion it leads to, let me refer to Dr. Ransford's remark. He said, "he was not quite decided upon the question of dynamization, so called, because he is convinced by experience that certain medicines act more beneficially in higher dilutions than in the lower ones. For example, take *Baryta carbonica* in inflammatory affections of the tonsils and pharynx, with the 12th dilution of this medicine he has never yet been disappointed; but both he and some of his colleagues who have tried it at his recommendation have failed with dilutions lower than the 6th."

It may be that certain medicines act more beneficially in higher than lower dilutions, but this may be a fact quite independent of Hahnemann's theory of dynamization. I shall afterwards allude to the alleged insufficiency of *Baryta* lower than the 6th.

The next speaker who alluded to dynamization was Dr. Bayes. He said, "Excepting up to a certain point, with in-

* Vide *Brit. Journ. of Hom.*, July, 1871.

soluble substances, he (Dr. Bayes) does not believe in dynamization. The more soluble a substance becomes, the greater its physiological power. It is from its greater solubility that *Mercurius corrosivus* is, in the lower dilutions, more potent than the same dilutions of *Mercurius vivus*. But though trituration and dilution do not increase the physiological power of medicine, he was not prepared to say that the curative power did not become greater, in some cases, by dilution."

This is not Hahnemann's theory, for it is the old and simple explanation *Corpora non agunt nisi soluta*.

Dr. Hale said, "Into the question of dynamization at so late an hour in the evening it was impossible to enter, but this much Dr. Hale might venture to say, that without accepting Hahnemann's theory of dynamization, the fact of dynamic force being developed by the process of trituration and succussion would still remain a fact, Dr. Black's attempted refutation notwithstanding. In confirmation of such developed forces there are several analogous facts in physics, and did time permit these might be adduced.

To this I need only reply that Dr. Hale fails to answer the argument which I brought to bear against Hahnemann's *Theory of Dynamization* as given by Dr. Hale in the instance of *Graphites*. I have no hesitation in repeating that this theory has been most pernicious to posology as giving a bias in favour of certain doses which, I think, would never be arrived at were the dose viewed objectively, and scientifically, and adapted to the strict exigencies of practice.

Some of the speakers, *e. g.* Dr. Leadam, objected to limitation of the dose. Let me be clearly understood on this point. I recommend the limitation of the dose from the crude substance to the 3rd, in order that the ground for inquiry be circumscribed, otherwise the trebled age of Methuselah would not be sufficient to come to a scientific conclusion. After years of careful inquiry I find that all the requirements for practice are met within these limits. I know this is the experience of a great many of us. Let the subject be worked out experimentally within this circle. If found to be too confined let the instances be brought

forward and discussed. I am the last person to enforce any dogmatic rule, but I am grieved to see the injury done to our cause by the dose, and therefore I desire discussion in the hopes of bringing about a general practice which will place us in more scientific *rapport* with our brethren of the ordinary school.

In this first discussion the following remedies were mentioned as among the class whose curative virtues were better manifested in dilutions exceeding six than under it. 1st. *Baryta carb.* Dr. Ransford says he is never disappointed with the 12th dilution of this medicine in inflammatory affections of the pharynx, but he fails to produce its effects with dilutions lower than 6. It is not a remedy which I employ in acute tonsillitis; therefore from my own experience I cannot answer the observation; its efficacy, however, has been doubted, but here is a fair case for further observation.

Mr. J. H. Smith observed that "ever since he became a homœopathist he had acted on the principle so clearly expounded and so ably enforced by Dr. Black, of not diminishing the dose more than he believed to be necessary to avoid the physiological action of the medicine, and yet to secure the curative effect. This object in practice he had generally been able to secure by doses varying from the 1st to the 3rd decimal dilution. There were certainly, however, exceptions to any general rule of this kind, and he should like to ask Dr. Black whether he had *advisedly* included *Chamomilla* in the list of remedies which Hahnemann had originally used successfully in mother-tincture? Dr. Hughes, who is certainly not a high-dilutionist, indorses the statement of Dr. Holcombe, that this medicine 'is of no more value in disease in the low dilutions than catnip or mint tea,' and that it begins to manifest its power at the 6th dilution, and may often be given advantageously as high as the 18th. This he gives as the general experience of homœopathic practitioners."

My answer to Mr. Smith is that I purposely mentioned *Cham.* as one of the remedies which fully enters into the limitation I assigned, and I cannot agree with Dr. Hughes

in his experience of *Cham.* His work on *Pharmacodynamics* is one so ably executed, and so calculated to gain us a favorable hearing from our brethren of the ordinary school, that I am exceedingly anxious to have this point fully discussed. I shall quote in full his opinion.

“The facts about the dose of *Chamomilla* are among the most curious that homœopathy presents. Of very little pathogenetic activity in its crude state, ‘the low dilutions’ are as Dr. Holcombe truly says, ‘of certainly no more value in disease than catnip or mint teas.’ *Chamomilla* begins at the 6th potency to manifest its great curative powers, and may often be given with advantage as high as the 18th. The 12th is my own favourite dilution. The facts are vouched for by homœopaths generally; their significance is at present doubtful.”*

This view of *Cham.* is opposed to my own experience, and I venture to say it is not generally vouched for by homœopathic practitioners. Turning to the *Brit. Journ. of Hom.* I find, in the series of very interesting papers by Dr. Hirsch, of Prague, the following remarks on *Chamomilla*.

“Besides the above-mentioned peculiar specific character of the medicine, which, as a rule, demands only small doses in order to develop considerable and even great effects, there is also the individuality of the specific medicine, which experience tells us must help to determine the size of the dose.

“It is hardly necessary to say that we ought not to administer, *e.g.*, *Belladonna* and *Viola tricolor* in the same doses, nor yet *Hyoscyamus* and *Sambucus niger*. Moreover, I have been convinced by innumerable trials and repeated experience that, *inter alia*, these same *Viola tricolor* and *Sambucus*, and also *Chamomilla* and *Sarsaparilla* manifest their greatest efficacy, and curative power in cases for which they are strictly suitable—the three first in the form of weak infusion, the last in that of decoction.

“With respect to *Chamomilla*, I make a cup of tea out of three to four small flowers; and of this I administer to new-

* Hughes's *Pharmacodynamics*, p. 209.

born infants a few drops every one or two hours, according to circumstances. To older children and adults I administer the infusion in the dose of half or a whole teaspoonful at similar intervals, or where the violence of the symptoms is not so great, at longer intervals.

“The effect of this method leaves nothing to be desired. Of course this dose is small enough to appear ludicrous to the allopath; but it is too large to be mentioned in the same breath with the 6th or 12th dilution. But I would recommend a trial of this medicine in the above manner in the case of catarrhal cough in children, when it is accompanied with hoarseness or great mucous rattle, or is especially troublesome at night; and the infallible efficacy of this mode of administering *Chamomilla* in the painful bowel complaints of children, and likewise in the diarrhœa accompanying dentition.”*

In the 25th vol. of the same Journal Dr. Watzke, of Vienna, reports his experience, and gives a good case of neuralgia of a month's standing, which was quickly cured by a few doses of *Cham.* 1.

Noack and Trinks in their well-known *Arzneimittellehre*, give the dose of *Cham.*, as from one to two drops of the mother tincture, or of the 1st to the 3rd dilution. Dr. Clotar Müller in his contribution to the dose question states his dose of *Cham.* to vary from the 3rd to the 6th.

If Dr. Hughes' views be correct, then the sources from which Hahnemann drew his knowledge of the curative powers of *Cham.* are fallacious. Hahnemann precisionised its therapeutic uses, but long previous to this *Cham.*, in large doses of infusion, had been employed in domestic practice, and then, and since then, by various physicians in all the diseases where Hahnemann's proving indicates its use to the homœopathic school. In *Merat and De Lens' Dictionnaire Therapeutique* a list of the diseases are given in which *Cham.* has been administered by the ordinary school. Turning to an excellent homœopathic authority, let any one read in Noack and Trink's *Arzneimittellehre* the long list of

* *Brit. Journ. of Hom.*, vol. xxv, p. 391, Dr. Hirsch “On the Dose.”

diseases in which *Cham.* has been and is used by the old school, and compare this with the list given in the next page of the affections in which it is employed by the homœopathic school, you will be struck by the very close similarity, and you at once accumulate an immense amount of evidence against the opinion that *Cham.* has little or no curative powers until the 6th potency is reached.

I shall now discuss the dose of *Bryonia*; this medicine was not alluded to in the Society's meeting, but last year, at the Birmingham Congress, its superior efficacy in dilutions higher than the 3rd was stated by Dr. Madden and Dr. Bayes.* The former said his attention was drawn to this circumstance by Dr. Bayes, and he again was led to use it from being struck with Dr. Tessier's experience of it given in the higher dilutions in pneumonia.

The observations made by Drs. Bayes and Madden applied to acute rheumatism, and I had just read a paper on the treatment of this disease, and I advocated the employment of the lowest dilutions, giving then, as I do now, the answer that *Bry.*, in dilutions above the 12th or 15th, had produced, in Drs. Wurmb's and Watzke's experience of acute rheumatism in the Vienna Hom. Hosp., simply negative results. They admitted they were not better than purely expectant practice, whereas I tried to show that rheumatic fever treated with low dilutions of certain remedies, especially *Bry.*, gave decidedly better results than the purely expectant treatment. But the duration of rheumatic fever treated homœopathically, as compared with judicious expectant treatment, is as yet so nearly similar, only a difference of a few days, that it is not a suitable affection on which to test the efficacy of a particular dilution, but as far as that evidence yet goes it is in favour of the lowest dilutions.

The same may be said of pneumonia, where the results of expectant and homœopathic practice are still more closely alike. I wish now to show that the source from which Dr. Bayes first drew his advocacy of the higher dilutions of *Bry.* is fallacious.

* *Monthly Journal of Homœopathy.*

Let us turn to Tessier's work. One might suppose, as Tessier has been quoted on the subject of dose, that he had made comparative trials, and as the result of these had arrived at the superior efficacy of the dilutions ranging from 12 to 30, but such was not the case.

Tessier commenced the homœopathic treatment of pneumonia with such dilutions as the accidental association with certain homœopathic practitioners suggested. In the first case he treated he gave *Acon.* 15 and *Bry.* 30. At this period he had not complete confidence in homœopathic treatment, for in case No. X, in the course of which meningitis supervened, he found *Bell.* 18 produced no effect, and therefore he had recourse to what he considered the most energetic treatment, *i. e.*, cold affusion and five grain doses of *Musk*. After these had warded off the cerebral symptoms, he returned to *Bell.* because it was strongly indicated, and then it acted promptly; that is, after cold effusion and *Musk* had succeeded, "elle (*Bell.*) a promptement agi." I quote this because in connection with some other remarks it appears to show a rather credulous tendency on Tessier's part.

Take this remark, together with the observations to Case XVIII, where he says the treatment was so remarkably efficacious, that under the use of *Bry.* 12 and 24, the pulse at last fell to 44; and again at p. 164, when in describing the extraordinary influence of *Bry.* 12 to 30 over the pulse, he mentions one case where the pulse fell under *Bry.* to 36.

Now, is it not more likely that the unusual reduction of the pulse far below the normal standard was a mere coincidence, and had no relation to the administration of *Bry.*? Even as a physiological effect the published provings of *Bry.* would not assign it to that remedy. Tessier states he has no belief in the natural cure of pneumonia; he writes this in 1850, a date prior to the experiments of Dietl in Vienna—experiments which led to the serious study of the natural history of pneumonia.

Tessier's carefully recorded cases, then, may be claimed by some as contributions to the expectant, by others to the

specific treatment of pneumonia, but as crucial experiments on the point of dose they are of no value.

In one case where he gave the 6th dil. the recovery was as quick as when he administered the 18th,—the natural question arises, Why, then, not keep to 6?

Dr. Clotar Müller in his "Contributions to the Dose Question," says, "*Bryonia* has always shown itself more and more efficacious the lower the dilution given. Especially in rheumatism and pleurisy I employ it as much as possible in dilution 1; in catarrhal and gastric affections, on the contrary, I not unfrequently give it in the 3rd and 6th."

Having now answered the objections which have been brought forward against my thesis, I leave the matter in your hands for further discussion, hoping that we may soon arrive at such a general expression of experience as will place the question of dose in a scientific position.

Discussion on Dr. Black's paper.

Dr. YELDHAM need hardly say that he entirely concurred in every word uttered by Dr. Black. He wished simply to make a few remarks about *Baryta carb.*, *Cham.*, and *Bry.* As to the first of these he could not say that in his experience he had realised the superior virtues attributed to the 12th dilution over other dilutions, or over other medicines, in treating cynanche tonsillans. He had heard no valid reason why it should be preferable to the 11th or 13th dilution. These assertions of the superiority of one particular dilution of a medicine over all others in every case of a particular disease, were, he believed, mere assertions, unsupported by any careful comparison with the effects of other dilutions. Secondly, as to *Chamomilla*, which it was now the fashion to condemn as useless below the 6th dilution, his own experience was utterly at variance with that opinion. So thoroughly dissatisfied had he become with the dilutions of that medicine that for many years he ceased to prescribe it altogether. When, however, he was led to the more general use of *larger* doses he resumed the use of *Chamomilla* in the mother tincture, and had every reason to be satisfied with it. Then as regards *Bryonia*, especially in rheumatism, he was rather disposed to prefer the 1st and 3rd dilutions to the mother tincture, which, moreover, was very bitter and disagreeable to take. He thought

Dr. Black's proposed limitation to the 3rd decimal very judicious. Within that limit the physiological effects of even the most potent medicines might be avoided even when prescribing for infants. It was not of course intended to restrict the members of the Society to the 3rd dilution, and he hoped that in every suitable case they would submit this question to a practical test.

Dr. HAMILTON is sorry he cannot agree with his friend Dr. Black. He does not think that the dilutions from 1st to 3rd would suffice to cure in all cases, that not enough stress in the argument has been laid on the susceptibility of different constitutions to different dilutions, and that having the power to regulate the dose if necessary to high dilutions is in his opinion one of the greatest advantages in our practice, and we should lose much by limiting the dose. We must put out of the question the supposition that we hurt the feelings of our allopathic brethren (if we may call them so) by giving high dilutions and so prevent their reformation. The confining our dose to the crude material as some would is going back to the darkness and uncertainty from which Hahnemann had emancipated us. He (Dr. Hamilton) had in the course of an experience of nearly thirty years seen in a great number of instances remarkable effects of cures from high (*i. e.* as high as the 30th) dilutions after the low (*i. e.* to the 3rd) had been tried in vain and *vice versa*; why, therefore, prevent any such result? He considers any discussion on such subject much loss of time, ending in no good and probably in much harm.

Dr. DRYSDALE.—We don't doubt the efficacy of the higher dilutions, but we have no *proof* that a low dilution below 3 would not do just as well. In high dilution practice you get exceptional good cases, but as a general rule low dilutions do best. The superior efficacy of the low dilutions had been in a manner forced on his attention. He began practice by using as a rule the dilutions above 3rd. But in course of time he found that a number of medicines were gradually falling out of use in his practice from his finding no trustworthy action to be expected from them. These were the weaker class of vegetable medicines, such as *Sambucus*, *Sarsaparilla*, *Taraxacum*, *Chamomilla*, *Viola*, &c. Then he began to give those as a rule in moderate doses of the undiluted preparation, and has had frequent occasion to find confirmation of their reputed good effects. Afterwards he applied the principle of increasing the dose to the polychrests and even the insoluble medicines which require trituration with milk sugar to make them active, and now he is inclined to agree with Dr. Black that no advantage is to be gained by diluting any medicine above the 3rd dilution. He regretted that the real difficulty of the matter had not been elucidated by Dr. Black, which was that, in the contingent symptoms, we had no means of knowing the dose required to evoke them in the healthy body, and, therefore, Dr. Yeldham's law of giving a dose just below the limit of physiological action would not suffice, and we are thrown back still on clinical observation.

Dr. HALE in reply to Dr. Black, who had in the paper just read referred to some previous observations of his (Dr. Hale), said that he thought that Dr. Black's endeavour to limit the dose so that it should not ascend above the 3rd decimal would be found impracticable. One of the arguments in favour of such restriction was that by doing so we should lessen or remove the prejudice which exists on the part of our allopathic brethren as to the minuteness of the homœopathic dose. Dr. Hale questioned the soundness of such an argument and for the following reasons. Two views may be held to explain the action of medicines administered according to the law of *similia similibus curantur*, one being that mere dilution or attenuation increases the curative power of a drug, the other holding that in addition to mere minute division of the particles of drug a dynamic *force* is brought into a state of activity by the processess of trituration and succussion. Dr. Hale's contention being that the hypothesis of dilution or attenuation alone being the cause of the increased energy of drug action sounds almost like an absurdity, and would fail to appeal with any force to the minds of our more enlightened or scientific allopathic brethren; but let a rational theory of dynamisation be presented to them, based as it is upon a number of analogous scientific facts and experiments in the domain of physics, and the apparent absurdity of minute division ceases and a more rational and scientific basis is obtained upon which to found a rule for the dose, supposing it possible to arrive at any practical rule of dose at all. Adducing one out of many analogous facts from the science of physics, Dr. Hale instanced the remarkable dynamic effect upon metallic *Mercury*, stating upon the authority of the late Sir John Herschell, that by alloying *Mercury* with a *millionth* of its weight of *Sodium* a power of not less than 50,000 times that of gravity is instantaneously generated when the alloy is submitted to galvanic influence. Did time permit Dr. Hale said that many more facts of a like kind might be instanced, showing that latent power is brought into activity by mechanical as well as chemical or electrical processes. To talk of the dynamic action of drugs and yet deny or explain away dynamisation was a self-destructive argument. Dr. Hale was glad to have the opportunity of replying to some strictures of Dr. Black's upon some observations of Dr. Hale's in a paper he had the honour of reading to the Society on a former occasion, in which it was stated that *chemically* there was no difference whatever between *Graphites* and *Carbo vegetabilis*, but that the specific dynamic action of each depended upon the different molecular arrangement of their particles. To this it was objected by Dr. Black that there was a chemical difference, inasmuch as *Graphites* contained *Iron*. This mistake Dr. Hale begged to correct by proving that the *Graphites* employed for years by homœopaths was prepared, he believed, according to Gruner's process, which carefully removed all traces of the mechanically adhering *Iron* by washing the *Graphites* with

Hydrochloric or Sulphuric acid. With regard to the vexed question of dose no one would be more rejoiced than he if it were possible to arrive at some satisfactory guiding rule, but as long as we had to meet the ever varying conditions under which disease has to be combated,—conditions of age, sex, temperament, and idiosyncrasy, acuteness and chronicity, the rule of the dose must necessarily be as elastic to meet these conditions as the conditions are ever varying. A rule has been suggested that the dose should be just short of producing the physiological action of the drug, but this cannot *a priori* be known, and even if known for one case could not be determined for another. It seems to be forgotten by those who advocate a fixed scale of doses that we have to deal with vital phenomena, and until we know what vitality is and the laws which govern its manifestations in disease we shall be very far from arriving at any fixed principle to guide us to the most suitable potency of the medicine. Dr. Hale believed that in the present state of our knowledge the only practicable method is to prescribe the dose that to the best of our judgment is most suitable to each individual case, being guided in our choice by such circumstances as the following: the nature of the disease and of the tissues or organs affected—if a tissue or organ be endowed with a low vitality, and whose vascular and nervous organisation does not endow it with much reactive energy or susceptibility to drug-action, a low dilution would probably be the best. If, on the other hand, the organ or tissue be furnished with a large vascular and nervous supply, and whose susceptibility was exalted in disease, in such a case it would be reasonable to suppose that the medium or higher potencies would be the best; in each and every case being guided by the before-mentioned circumstances of age, constitution, temperament, &c., and in conclusion Dr. Hale contended that each practitioner's own experience, corrected or enlarged by the experience of others, if applied upon some such principle as he endeavoured to advocate, always taking it for granted that the law of homœopathy be strictly adhered to, would be found eminently successful without the necessity of fixing upon any such limited range of dose as Dr. Black had in his paper so ably advocated. Speaking for himself he was in the habit of prescribing medicines from 1st to 30th dilutions and very rarely higher, and he protested against being tied down to any rigid rule in prescribing in the way he considered best for the cure or alleviation of disease.

Dr. R. HUGHES thought that the speakers who had differed from Dr. Black had misunderstood his position. He did not apprehend him to deny that the dilutions above the third had curative power. Had he done so he (Dr. Hughes) would have been utterly unable to follow him. But he maintained that there was no evidence that they acted better than those below the 3rd; and that, if it were so, there were abundant reasons why we should prefer the lower potencies in practice. Dr. Black invited us to an experi-

ment,—an experiment which he himself had made. He asked those who had been in the habit of using the dilutions above the 3rd to try those below it for a time, and see what success they obtained. He (Dr. Hughes) would certainly accept the invitation. He already used most of his medicines below the 3rd: but there were a certain number from which he daily obtained excellent results in the dilutions from 6 to 30. He was much struck with Dr. Black's experience with medicines of the *Lycopodium* and *Sepia* type in the 3rd trituration. He was under the impression that they were practically discarded in exclusive low-potency practice. With regard to the observations upon the dose of *Chamomilla* contained in his *Manual of Pharmacodynamics*, he could only say that they represented the recorded experience up to the time of its publication. Hirsch's and Watzke's observations had appeared since.

MR. J. HARMAR SMITH said he fully accorded with the recommendation of Dr. Black as to the desirableness of our coming to an understanding that we would for a certain period test practically the range of dose which he suggests. There was, however, a *caveat* which he felt it desirable to put in. He was convinced that the frequent employment of mother tinctures tended towards the formation of a habit of reversing the principle of our treatment, and of prescribing antipathically instead of homœopathically. For example, the employment of a few drops of *Tincture of Podophyllum* or *Nux vomica* as aperients; of *Belladonna* or *Morphia* as sedatives; of *Quinine* or *Cinchona* as tonics was no more homœopathy than the exhibition of massive doses of the same medicines for the same ends. But it might be, and he believed frequently was, substituted for more painstaking and efficient homœopathic treatment. He had both felt and observed the danger of getting on this *sliding scale* to the position which we have left.

DR. LEADAM thinks there would be no objection in making the experiment of the dose in this hospital, and while he still holds to the arguments so well laid out by Dr. Hamilton and Dr. Hale, and would be sorry to give up the greater limits of homœopathic practice and be confined to the three first dilutions as proposed by Dr. Black, he is willing, so far as it rests with him, to put all his patients in the hospital under the use of the 1st, 2nd, and 3rd tinctures alone, and report the result.

DR. BAYES would call attention to a fact which seemed to have been overlooked by some of the speakers, that a very large number of men of age and of ability have given their testimony to us as to the efficacy of the higher dilutions. Is their experience to go for nothing? Are we to begin *de novo*, and to ignore their labours? For his own part, his experience led him to the conclusion that it is by no means a matter of no moment as to what dilution is given; and while he admitted that there are many cases admirably treated by doses varying from mother tinctures

to the 3rd dilution, he felt equally compelled to assert that there are other cases where the higher dilutions are the most curative. Believing, as he does, that the true explanation of the rule *similia similibus curantur* resides in the power inherent in each medicine to stimulate the nerves of a part, tract, or organ of the body; believing, also, that *those diseases which yield to homœopathic medicines* are of that class which are characterised by an adynamic state of the part, tract, or organ affected; believing, further, that our art consists in an *exact adjustment* of the dose of the stimulating drug-force to the want of force present in each individual case,—he cannot bring himself to believe that one, or two, or three dilutions, whether low or high, will suffice for successful practice. It seemed to him that it is our duty to ascertain, as nearly as we can, the exact want of adjustment in the lost balance of health, and then to apply just such an amount of drug-force as shall restore and adjust the balance of function to a healthy standard. With regard to the question of medicinal aggravation, he would observe that if the theory of specific stimulation be true, as he believed it to be, we shall at once have an explanation of aggravation. For all drug stimulants are paralyzers if given in doses larger than is needed for the restoration of health, and therefore the over-action of a specific stimulant would increase the symptoms of the disease. This principle has been asserted by Claude Bernard, who lays down the law that “every substance which in large doses abolishes the property of an organic element stimulates it if given in small ones.” Large and small are, of course, relative terms, and in very sensitive persons, or in parts rendered sensitive by disease, a comparatively small dose may prove sufficient to embarrass vital processes and to increase the symptoms of disease, where an infinitesimal dose might restore health. Hence his experience would lead him to reject any proposition which would confine him to the 3rd as the highest dilution to be given, and he would propose that only those gentlemen whose belief in the superior efficacy of the lowest dilutions in all cases should carry on this experiment. Probably half a dozen experimenters might thus be found who could conscientiously thus limit themselves. When they bring the result of their experiments they can be compared with those which have preceded them. Perhaps, too, some physicians who could conscientiously confine their practice to the dilutions above the 3rd would experiment in the same way and at the same time, and thus we might obtain some comparative statistics worth having. Dr. Bayes then mentioned a case where the 3rd dilution of *Argent. nit.* has induced aggravation, relief followed the 30th, but cure resulted on leaving off the medicine. The patient in this case was a physician of good standing and a competent observer. He also mentioned a case of acute rheumatism rapidly cured by *Bryonia* 18th. As a general argument in favour of the curative powers of the two extreme limits of the dose, he had

observed that the consulting rooms of those physicians who either were noted for using the lowest dilutions, on the one hand, or the highest on the other, were the most crowded. This must result from their successes and the fame of their successes, for people would not crowd year after year into the room of him who made empty promises. Patients soon find their level, and those cases best cured by crude drugs or low dilutions would be attracted to the physician so practising, and *vice versa*. With regard to the physical effect of the infinitely little on the hugely great, he named the influence of a drachm of gold when melted with a ton of iron as improving the quality of steel, while double the quantity deteriorated it, a proof that the size of dose was very important.

Dr. MADDEN was very anxious that this question of the dose should be fully and fairly discussed. As it at present stood the chief objections raised against the low and crude doses were, 1st, that giving larger doses looks like a return toward allopathy; 2nd, that these large doses cause aggravation. While, in reply, it may be asserted that the apparent return towards allopathy will be counter-balanced by the disarming of prejudice, which is chiefly founded on the use of infinitesimals. And as regards the causing of aggravations they can be easily avoided; and, in point of fact, so-called aggravations are more frequently met with after the use of *high* dilutions; on the other hand, the positive advantages of the larger doses are that those who use them can satisfy themselves that they are really giving the medicines they prescribe, and it is asserted, though not proved, that their action is less easily interfered with. The objections to infinitesimals are—1st. No person who has not made the dilution himself can feel certain that he is giving any medicine at all. We are absolutely dependent on the chemists and their assistants for such preparations. 2nd. The frequent use of high dilutions increases the sensitiveness of patients to medicinal agents, and also to other disturbing causes, and hence acts injuriously on unstable nervous systems. 3rd. Their action is supposed to be easily interfered with, and hence, during their use, one is precluded from employing many palliatives lest they should interfere with the treatment. 4th. The warning is often repeated by the advocates of the high dilutions against repetitions of the dose, and allowing time for the effect to be produced, &c., looks as if unassisted nature was to be allowed the best possible chance of completing the cure. Over-riding all these objections, however, is the assertion made by the high-dilutionists that they cure better and more permanently than the larger doses can. Of course, should this be *proved* all controversy must cease; if better cures can be made there should be no longer any question as to the doses to be employed. Meantime, however, it is incumbent on all who prefer the larger doses to accumulate evidence so that the question may be fairly

brought to the test of experiment, and this is what we may expect as the result of the present discussion.

Dr. ROTH said in 1850 he was acting for several months as physician to the West-end Dispensary of the German Hospital, and, although he prescribed merely solutions and 1st triturations of remedies as found in an (allopathic) *usual* pharmacy, but chosen on the principle of *similia similibus*, his success was as good as with higher dilutions. The number of patients increased considerably, and he received the thanks of the Committee. He mentioned this as an additional testimony to the value of Dr. Black's experience.

Dr. DUDGEON approved of Dr. Black's plan for attaining to a settlement of the dose question, but it should be borne in mind that diseases such as tonsillitis (which had been alluded to to-night in connection with this question), which naturally tended towards quick recovery, would throw no light on the subject. The only cases that could decide the point were such as had little or no tendency to spontaneous recovery.

R E V I E W.

On Chronic Diseases of the Organs of Respiration ; being a series of Clinical Observations on Diseases of the Air Passages and the Lungs. By JOHN MEYHOFFER, M.D., &c. Vol. I. Diseases of the Larynx and the Bronchial Tubes. Turner & Co., Fleet Street.

IN our last number (p. 607) we spoke a few words of welcome to this production of our colleague, but deferred a fuller consideration of his pages till we had had time to thoroughly digest them. We now fulfil the pleasing duty of reviewing them at length.

Dr. Meyhoffer's previous contributions to this Journal and our *Monthly* contemporary had prepared us to expect a good deal of a volume from his pen ; nor have we been disappointed. We have never, indeed, read with greater satisfaction any publication of the Homœopathic School. It is the kind of thing we most want ; *i. e.*, a monograph on a definite class of diseases ; and it is done with a thoroughness which leaves nothing to desire. It is rich alike in literary research and in practical observation. The author is abreast with the most recent advances of physiology, pathology, and diagnostics ; while his reports of cases show him to be a painstaking and successful physician. He is a sound homœopath as to drug-treatment, while freely availing himself of the dietetic and hygienic resources at our command, especially (as we shall show anon) of the use of mineral waters. His book is replete with instruction for ourselves ; and is of no small value as a testimony to our opponents of what homœopathy can be and can do.

To descend now to particulars. Dr. Meyhoffer's book makes no pretence to be exhaustive. It is a chapter of a volume of clinical lectures, not of a practice of physic. It gives no account of the recorded homœopathic treatment of the diseases it discusses, but simply the result of the

writer's own experience. It does not, therefore, supersede the systematic treatises, but supplements and enriches them at this point of contact. The author's opportunities of observing chronic respiratory disorders have been eminently favourable. Practising for nearly thirteen years at Nice, to whose salutary air so many invalids of this class resort, he has collected some four hundred cases of chronic disease of the respiratory organs. The present volume gives us his observations on laryngeal affections and on bronchitis. The second will treat of pulmonary congestion, pleurisy, emphysema, asthma, and tuberculosis pulmonum. A goodly list! and when we say that forty-two of the cases observed are related at more or less length, it is evident that Dr. Meyhoffer has not let his opportunities slip by unutilised. That he has brought to them the perceiving eye as well as the recording pen is also abundantly evident throughout his pages.

The introduction strikes the key-note of the whole volume,—viz. the importance of constitutional treatment in (apparently) local disease. The frequency of innutrition, absolute or relative; the misinterpretation of febrile, inflammatory, and other conditions of over-action as implying increased vitality; the really depressing effects of luxurious alimentation:—these are no new truths, but their enforcement well paves the way for a discussion of disorders very limited in geographical range, but having wide physiological relations. Dr. Meyhoffer would have us not think more of elective affinity than of constitutional suitability in our choice of drugs, and not forget the removal of causes and the arrangements of surroundings in a search after the similar remedy.

The diseases of the larynx discussed are—

Catarrhal laryngitis.

Follicular laryngitis.

Plastic or hypertrophic laryngitis.

Chronic inflammation of the muscles of the vocal cords.

Perichondritis laryngea.

Tubercular laryngitis.

Laryngitis syphilitica.

Spasmus laryngis.

Paralytic aphonia.

The pathology and diagnosis of each of these forms of disease are fully stated, the latter always including laryngoscopic inspection. We cannot give a better illustration of our author's matter and manner than by sketching his chapter on Tubercular Laryngitis.

It begins with the following narration of a case.

"Mdlle. R—, a French governess, æt. 22, of a slender, delicate figure, came for advice on November 2nd, 1867. She complained, in a hoarse voice, of an extremely annoying frequent cough with copious expectoration, great muscular weakness, and total loss of appetite. The symptoms had developed gradually within the last five months, while, at the same time, the catamenia grew more scanty, and for the last two periods had altogether ceased.

"She had been subject from childhood to sore throat and hoarseness, while the red and thick edges of the eyelids, partly deprived of the projecting lashes, betrayed that she had been affected by what is termed scrofulous ophthalmia. Her mother died of consumption; her father, still living, was reputed to be strong and healthy. There were all the symptoms of anæmia; pale skin, almost white lips, jugular murmur, and the bones seemed to have no other covering than the transparent cuticle. The tongue was smooth and white; the bowels sluggish. During the last fortnight she had had perspirations towards the morning. The pulse was small and quick—90 per minute; going upstairs brought on palpitation and dyspnœa. The breathing was regular—twenty-four inspirations; percussion of the chest elicited no dulness, but at the apex of the left lung the respiratory sound was sharp, and not vesicular, accompanied by sibilant rhonchi, and prolonged respiration. The lining of the throat was rugged, reddish, and partly covered with thick yellow mucus. The puffy ventricular bands were of a dingy red colour, the right one had lost the epithelium on its posterior section, thus laying bare the swollen follicles, some of which exhibited small cup-shaped ulcerations, discharging profusely a yellow matter. The left ventricular

band began also to show signs of a similar degenerative process, as the epithelium was abraded in a few places ; the posterior wall of the laryngeal cavity was swollen, and dark red. Pressure on the cricoid cartilages seemed to cause pain.

“ Two drops of *Pulsatilla* 3, morning and evening ; milk for breakfast and supper ; meat and green vegetables for dinner ; no potatoes, and complete rest of the organ, were the directions given, and strictly followed by the patient until the 12th ; the report then was better appetite, less cough, expectoration less copious, absence of perspiration in the night, and more inclination for walking exercise.

“ *Prescrip.*—*Seleniate of Soda* 3, two drops ter die. The same regimen as before, with the addition of fried bacon in the morning.

“ December 14th.—Contrary to my advice, the patient continued this salt without interruption to the above date, and stopped my expostulation by ‘ cela m’a fait tout de bien ’ (‘ it did me so much good ’). Truly her voice was clearer, the cough seldom ; the pulse had gone down to 80, and was fuller, while an excellent appetite and better complexion, with more softness of form, displayed improved nutrition.

“ The drug was then omitted, and *Cod-liver oil* prescribed ; this she took, however, for only a few days, alleging that it always caused sickness, and took away her appetite. She was advised to take a cup of cream instead, once a day, as well as to use more butter.

“ 20th.—No progress of improvement in the laryngeal symptoms ; rather more cough and expectoration. Examination of the larynx exhibited the left ventricular band covered on its largest surface with new epithelium ; but a few of the follicular ulcerations had melted into one, thus leaving a jagged ulcer with abrupt edges, involving the sub-mucous tissue ; no other change in the lungs could be detected, except absence of rhonchi in the left apex.

“ *Seleniate of Soda* was resumed, and used also for inhalations, half a grain of the pure salt to an ounce of water.

"January 24th, 1868.—For the last ten days she had employed, for inhalation only, common salt. The only remains of the laryngeal lesion were a scar. Mdlle. R—'s general health was greatly improved, she had had return of the menses, and her appearance presented all the signs of good nutrition."

The author then discusses the question—whether this morbid process had a tubercular origin, or was only a follicular inflammation? This leads him to the differential diagnosis of tubercular and follicular laryngitis,—his excellent account of which latter disease has appeared in the pages of this Journal (vol. xxvii, p. 521). The *destructive* tendency of the former is first insisted on. In simple catarrh there is no demolition of the epithelium, the mucus and pus-cells exuding through it; but in phthisis laryngea the appearance of purulent expectoration implies that the epithelium is already destroyed. Now in follicular inflammation the mucous cysts became obstructed, and so tumefied, through multiplication and thickening of the epithelial cells. Hence we have the following comparative view:

"Incipient stage of

<i>Follicular laryngitis.</i>	<i>Tubercular laryngitis.</i>
1. Epithelium preserved.	1. Epithelium destroyed.
2. Absence of secretion from obstruction of the ducts of the mucous follicles.	2. Dilatation of the ducts of the mucous follicles, and copious purulent discharge.
3. Hypertrophy of the follicles; seldom ulceration.	3. Ulceration and destruction of the follicles as a rule."

Applying these facts to the case narrated, its tubercular nature can hardly be doubtful.

The prognosis of the disease is then considered, and shown to be relatively unfavourable in that the presence of tubercle in the larynx almost always implies its coincident development in the lungs. A case in point is related, and, *à propos* thereof, hoarseness or loss of voice is noted as one of the earliest and most persistent symptoms of incipient

laryngeal phthisis, and therefore of grave omen in predisposed constitutions.

Next comes a description of the ordinary course of the disease, depicting the terrible havoc wrought by the rodent tubercular ulcer. The character of the cough and expectoration is minutely specified, and the presence or absence of pain accounted for.

Last, we come to the treatment. Reserving that of the tubercular diathesis till he comes to phthisis pulmonalis, he requires here that "the remedies selected be in specific relation to the local as well as general condition, as in no form of chronic laryngitis is local combined with general treatment more efficient. We have done all in our power to satisfy ourselves of the respective value of the local medicated inhalations, or general application of drugs, and have arrived at the conclusion that their simultaneous direct and indirect action conduces in a shorter time to a satisfactory result than when limited exclusively to the one or the other." It will be seen, however (as his words imply), that his local applications are not of the allopathic order, but of a strictly specific nature.

In accordance with these principles there follows an account of the author's experience in tubercular laryngitis with *Acidum nitricum*, *Argentum nitricum*, *Arsenicum*, *Belladonna*, *Iodine*, and the *Iodides of Mercury* and *Potassium*, *Phosphorus* and *Selenium*. The action of the last-named (which Dr. Meyhoffer prefers in the form of *Seleniate of Soda*) is well shown in the case cited. Some experiments by one Rabuteau are mentioned, which show its elective affinity for the respiratory organs.

We pass now to the chapter on bronchitis, which occupies nearly half the volume. The chronic varieties of this disease are considered under the following headings :

Chronic catarrh of children.

Mild or intermittent variety (winter cough).

Catarrh with scanty mucous secretion.

Bronchorrhœa.

Putrid bronchitis.

Bronchitis with copious sero-mucous expectoration.

Bronchitis from plethora vera.

Bronchitis resulting from organic disorders of the circulatory organs.

Bronchitis caused by the swelling of the tracheal and bronchial glands.

Under each of these titles we have much valuable matter presented to us. Chronic bronchitis in children is shown to be always the result of deficient general vitality. The frequent association of atrophy of the lungs with the "winter cough" of old people is noted. Under bronchorrhœa we miss a reference to that form of it where the bronchial membrane secretes pus largely, so that hectic is set up; "stone-cutter's phthisis" being of this nature. But bronchiectasis is duly appreciated, and shown to be the most frequent cause of putrid expectoration, to which symptom a good section is devoted. The author confirms the researches of those who have found cryptogamic sporules in the foetid sputa. The bronchial and other symptoms accompanying enlargement of the tracheal and bronchial glands are well described, and the importance of recognising the presence of this *origo mali* enforced and illustrated.

We have next a brief section on the diagnosis and an extensive one on the treatment of the disease. In the latter we have first suggestions for the medicinal and general management of the various forms of chronic bronchitis, and then comments upon each of the medicines (in alphabetic order) which have found place among its remedies. While preserving the now recognised landmarks of homœopathic tradition, Dr. Meyhoffer strikes out many a bye-path of his own into which we may follow him with advantage.

And now we have only a short space in which to speak of a special feature in Dr. Meyhoffer's book—his large employment of mineral waters in the treatment of disease. The chronic affections of the respiratory organs are so often dyscrasic in their origin, or involved in general constitutional disorder, that the profoundly "alterative" influence of these natural remedies lends us valuable aid. The volume before us is an emporium of information as to the

springs of Germany, France and Switzerland ; and not only in their relation to these particular diseases, but as to their general influence on the system. This element alone would make the book most serviceable to us. But it is altogether most admirable and useful, and we shall look forward with eagerness to the second volume.

One word only of complaint ere we conclude. Dr. Meyhoffer expresses his thanks to Dr. Hutchinson (late of Manchester) for " converting his Germanisms into palatable English," but we wish he could have secured the services of some such friend to correct his proofs before publication. They have evidently passed under the eye only of the reader for the press, so that clerical errors are few enough, but those which only the professional eye could detect abound. Some of these are rectified in the " Errata : " but though Dr. Reith is thus no longer written " Ruth," yet Dr. Porges, of Carlsbad, remains as " Parges," and Dr. Marey, of sphygmograph celebrity, as " Marcy ; " rhonchus is written " ronchus," and so on. Should the book reach (as it ought to reach) a second edition, it will need a careful revision in these matters.

CLINICAL RECORD.

Two Cases of Tonic Spasm treated (mainly) by Nux vomica.

By J. HARMAR SMITH, M.R.C.S., L.S.A.

CASE 1.—Mrs. W—, æt. 30, seven months pregnant, otherwise in a normal state of health, when I visited her, had been for a day or two suffering from the following symptoms:—The muscles of the fingers and thumbs of both hands were firmly and painfully contracted, the thumbs and fingers being strongly pressed against the palms, so that she was unable to open her hands, and the attempts to separate them on the part of another person caused severe pain. She had also pains in the calves of her legs.

Nux vomica (1) one drop every quarter of an hour until relieved.

I found my patient quite well when I visited her on the next day. She informed me that after the first dose of the medicine the painful pressure on the palms of the hand had ceased, and that after the second, which was taken a quarter of an hour after the first, she was able to open her hands. She had no return of the spasms either of the fingers or legs, and she went normally through her confinement two months afterwards.

CASE 2.—Mrs. F—, æt. 50. I was called up hastily in the middle of the night, several years ago, to visit this lady, who was then in a delicate state of health, and has since died of phthisis. I found her labouring under the following symptoms:—Complete opisthotonos, the back being arched like a bow, and its weight resting partly on the head and heels, but in part supported by the right arm, which was bent back and under the spine; the left arm was contracted upon the chest, and the fingers firmly clenched upon the palm; jaw locked and teeth firmly pressed together; clonic convulsions of muscles of face; lips pursed up and frequently drawn to right side. The flexors and extensors of the lower extremities were both in a state of spasmodic contraction, but the greater power and more advantageous insertion of the former would account for the partially bent position of the thighs and legs, so that they completed the arch of which the spinal

column formed a part. The pain was so severe during the height of the paroxysm that Mrs. F— afterwards compared it to all her limbs being placed on a rack. She was, however, calm and sensible all the time, and the pulse was quiet. After the spasm had continued for about half an hour there was an attempt at vomiting, and this was a distressing aggravation of the symptoms, for, owing to the locking of the jaws, the mouth could not be emptied, and there was a danger of suffocation. A portion, however, of the fluid which had been rejected by the stomach was ejected through the aperture left by a tooth which had been extracted. The same circumstance enabled me to give her medicine in small quantities, consisting of *Nux vomica* 1 and *Belladonna* 1, given in alternation every quarter of an hour.

In about an hour the spasm of the muscles of the jaw partially relaxed, and she was able to swallow wine and brandy to meet the exhausted state of the system. The spasm of the muscles of the jaw did not entirely cease, however, for several hours. The spinal rigidity gradually lessened, and, in two hours from the commencement of the attack no longer existed; the muscles of the left arm also became relaxed, the finger joints getting at liberty before the elbow, but the muscles of the right arm continued contracted for several hours longer. On the next day she appeared no worse than she had been for some weeks, although muscular pain was complained of for some days.

She expressed herself as conscious of sensible relief from each dose of the medicines.

She had a similar though slighter attack some weeks subsequently. Mrs. F— also informed me that she had an attack of opisthotonos many years ago. On this occasion the symptoms continued for three days, although she was blistered all down the spine.

I append to these cases a reference to the only allopathic authorities in my possession who allude to *Nux vomica* or *Strychnia* as in any degree applicable to the treatment of tetanus. The following remarks are those of Sir Thomas Watson in his *Lectures on the Principles and Practice of Physic*, vol. i, 8rd edition, pp. 586—7.

“*Strychnia* has been suggested as a remedy for severe tetanus, not in infinitesimal doses as Hahnemann would, I suppose, pre-

scribe it, but in sufficient quantity to produce a sensible effect. The principle upon which this has been recommended is the same with that on which the *Nitrate of Silver* ointment is applied to the inflamed conjunctiva in purulent ophthalmia. We know that *Strychnia* acts upon the spinal cord, affecting apparently those parts and those functions of the cord which are affected in tetanus; and in so fatal a malady it would be justifiable, I conceive, to give the *Strychnia*, in the hope that it might occasion a morbid action of the disease, and yet be less perilous and more manageable than it. But it would be right to try such a remedy as this, in the first instance, *in corpore vili*, upon one of the lower animals. This, were it successful, would be a cure according to the Hahnemannic doctrine, *similia similibus curantur*—a doctrine much older, however, than Hahnemann."

Dr. Symonds, of Bristol, in his article on "Tetanus," in the *Cyclopædia of Medicine*, also suggests a trial of *Strychnia* in Tetanus.

"We quite coincide in the opinion that it is incumbent on us to look about for new remedies since the old ones have all failed. Upon this ground we are disposed to suggest a trial of *Strychnia*; not that we have become followers of Hahnemann, but that it is a simple and undeniable fact that disorders are occasionally removed by remedies which have the power of producing similar affections. It is quite unnecessary to explain this fact by an arbitrary principle, that an artificial irritation excludes a spontaneous irritation of the same kind. A more rational ground for an expectation of benefit from homœopathic medicines may be found in the consideration, that such agents prove by their occasional production of symptoms like those of the disease to be treated, that they act on the part which is the seat of that disease, and, consequently, that there is a probability that, in their operation on that part (whether it be in a sufficient degree to produce a similar disease or not), they may effect a beneficial change. *Oil of Turpentine*, for instance, having been known to produce a discharge of bloody urine, might be rationally administered in a case of spontaneous hæmaturia, not because it has a tendency to produce this disorder, but because that tendency shows it to have a specific action on the vessels from which the hæmorrhage takes place."—*Cyclopædia of Practical Medicine*, vol. iv, p. 681.

MISCELLANEOUS.

Opium in Diabetes.

According to Dr. Pavy, *Opium*, *Morphia*, *Codeine*, all possess the property of checking the elimination of sugar by the urine. *Codeine* is most powerful, commencing with one half-grain dose and going on to ten grain doses thrice a day. A man, aged fifty, after restricted diet and alkalies, was passing urine of specific gravity 10·38 and 26·64 grains of sugar per ounce. After thirty days of *Opium* there was no sugar, and the specific gravity of the urine was 10·25. On leaving off the *Opium* again, the specific gravity rose to 10·48, and 10·65 grains of sugar were passed in the twenty-four hours. On renewing the *Opium*, the sugar immediately lessened, although he continued to take ordinary food. A woman, aged sixty-eight, passed five pints of urine of 10·40 specific gravity, and sugar to the extent of 32·72 grains daily. By the use of *Opium*, with ordinary diet, the urine in three months was of specific gravity 10·16 and quite free from sugar. She continued well for twelve months; then anxiety and over work renewed the disease. Again three months of *Codeine* completely cured her. This is a great discovery of Dr. Pavy's.—*Med. Press and Circ.*

Results of the Injection of Fungus-spores, &c., into the Blood of Animals.

E. SEMMER confirms as a matter of fact authenticated by Hallier, Zürn, Chauveau, Delafond, and others, in modern times, that the blood contains in contagious diseases great quantities of *Micrococcus-fecula*, *Mycothrix*- and *Leptothrix*-threads, and that these forms always appertain to different funguses in the different diseases. He says, "I have repeatedly, as to the funguses, examined the blood of the animals, which, at the veterinary institution of Dorpat, sinking of contagious diseases were dissected, and with the following results.

“In glanders there are found in the blood, the lymph and the glanders matter numerous *Micrococcus*-cells and *Mycothrix*-threads, in the (splenic) cattle-diseases (*Milzbrand*), and in *septicæmia* the blood contains numerous *Micrococcus*-cells, *Mycothrix* and *Leptothrix*-chains and threads, which long ago, no doubt, have been known as rod-like corpuscles. But that these rod-like corpuscles are fungus-threads there may be adduced in evidence the instance of five pigs dead of *septicæmia*, in which the origin of the little rods from the *Micrococcus*-cells could be followed up, for single short little rods consisted of *Micrococcus*-cells united together in rows; elsewhere of a *Micrococcus*-cell with a little rod-like process; yet other larger little rods showed distinct jointing as indication of sprouting, and in some even the jointing had disappeared, and these formed the inarticulate little rod-like corpuscles. The little rods appearing in the (splenic) cattle-disease are for the most part shorter, smaller, more faintly outlined, and more numerous than those appearing in *septicæmia*, which latter are of very various lengths, distinctly outlined, singly articulated. But also the little rods seen in the (splenic) cattle disease are not always of the same nature, and there also variations appear, and they often approach to the form and size of the little rods of *septicæmia*. The little rods appear in yet other blood-decomposition diseases besides the (splenic) cattle-disease and *septicæmia*. In the blood of healthy beasts are found little *Micrococcus*-cells of the *Penicillium glaucum* and little rods moving about in the intestines and liver.

“Now, it was of much importance to prove whether the contagious diseases were induced by the funguses which in those diseases are found in the blood. For this purpose I have made a series of experiments, which have led to the following results:—*Penicillium*-spores of the size of the blood-corpuscles were mixed with distilled water, and a small quantity thereof injected into the jugular of two foals by means of a little syringe, with needle-like canula, which was introduced into this vessel on account of the exposed condition of the jugular. The beasts remained perfectly healthy after the injection. Some weeks afterwards *Micrococcus*-fecula from cheese and saliva were in like manner, in the same animals, injected without the foals after the injection even showing any symptom of disease; so also they continued in good health after the injection of *Arthroccoccus*-fecula from acid

liquor. A second series of injections was made with larger masses of spores and fecula of the *Penicillium glaucum*, whereupon the animals evinced slight fever, that terminated in complete recovery. When the foals subsequently were put to death for anatomical purposes it was found that all the organs and tissues were left in normal condition.

“Then I cultivated funguses in well-boiled media from the (splenic) cattle-disease blood in proper flasks, recommended by Hallier, closed so as to be air-tight, with an india-rubber stopper, through which a twice bent U-shaped glass tube is carried. The spores of these funguses were mixed with distilled water and a drop of it injected into the jugular of a foal. The foal after that continued apparently in good health. Five days after the first injection about two ounces of the same fluid, which contained numerous fungus-spores and *Micrococcus fecula*, were injected into the jugular of the same foal with a larger syringe with a fine needle-shaped canula. It brought on fever; the foal, however, kept a good appetite, and apparently recovered, till on the tenth day after the injection its appetite was gone, a staggering gait and considerable fever appeared, and in the night of the tenth-eleventh day it died. In the dissection the following observations were made.

“In the neck, at the place at which the incision for the purpose of injecting had been made, the connective tissue was thickened, brawny, infiltrated, interspersed with ecchymoses; the jugular filled up with black pitchy blood, not otherwise changed; further under in the neck considerable brawny infiltration of the connective tissue, which did not stand in connection with the place of injection; on the left shoulder a large blood-extravasation under the skin in the connective tissue and between the muscles. In the abdominal and thoracic cavities reddish transudation in small quantity; in the pericardium similar transudation in considerable quantity; ecchymoses in the peritoneum, the intestines, the pleura, the heart, and in the lungs; the spleen enlarged, full of blood, easily broken down, interspersed with blood extravasations; the liver of yellowish-brown colour, liver-cells involved in fatty granular degeneration; the kidneys enclosed with a large, yellow, brawny mass, easily broken down, infiltrated, of yellowish-grey colour from fatty granules, degeneration of the epithelial cells of the urinary canaliculi; lymphatic glands enlarged, softened, infiltrated, some of a brownish-red colour, which originated in

blood extravasations. The brain and spinal marrow injected with blood, sodden; the brain-ventricles filled with clear colourless transudation; the blood, dark brown, viscous, pitchy, without firm clot, contained great quantities of the corpuscles, characteristically little-rod-like for the (splenic) cattle-disease.

"Here, then, by injection of fungus-spores and *Micrococcus*, which had been cultivated from the (splenic) cattle-disease blood, was the (splenic) cattle disease induced afresh; the ninth day after the injection came the outbreak, and on the tenth day the foal died."—*Virchow's Archiv*, vol. 1, part 1, pp. 158-60.—*Brit. and For. Med.-Chir. Review*, April, 1871.

Election of a Homœopathic Medical Officer by the Southampton Board of Guardians.

WE have much pleasure in announcing that our colleague, Dr. Archer, of Southampton, was elected Medical Officer of the Second District by the Board of Guardians on the 7th September. Opposed to him was an allopathic candidate, a long resident in Southampton, and though Dr. Archer was comparatively a stranger, his election was carried by 13 votes against 3, four of the guardians remaining neutral. The election of Dr. Archer is all the more gratifying, as it was to fill the post vacated by that bitter, though imprudent, antagonist of homœopathy, Dr. Griffin.

Treatment of Purpura.

Dr. Bauer (*Deutsche Klin.*) adds his testimony to that of Hensch as to the efficacy of *Secale cornutum* against purpura. He employs it in substance 8 to 10 grains, from one to three times daily. Of course no action is exerted upon the blood already effused, but, according to Dr. Bauer, in from one to three days, or at latest after six days, no new spots of purpura are formed, as far as his experience and observation have shown him.—*Med. Press and Circ.*

BOOKS RECEIVED.

Report of Surgical Operations performed in connection with the Clinic of the Hahnemann Medical College of Philadelphia, 1869-70, by MALCOLM MACFARLANE, M.D., Professor of Clinical Surgery.

Oxygen, Nature's Remedy in Disease, by GEORGE BARTH.

Weekly Hampshire Independent. Sept. 9, 1871.

The Dublin Quarterly Journal of Medical Science.

Australian Homœopathic Progress.

The Monthly Homœopathic Review.

The Hahnemannian Monthly.

The American Homœopathic Observer.

The Western Homœopathic Observer.

The Chicago Medical Investigator.

The North American Journal of Homœopathy.

United States Medical and Surgical Journal.

The Western Homœopathic Observer.

The New England Medical Gazette.

The American Journal of Homœopathic Materia Medica.

El Criterio Medico.

La Reforma Medica.

La Homœopatía.

Bibliothèque Homœopathique.

The Calcutta Journal of Medicine.

La Revista Omeopática.

The Food Journal.

The Chemist and Druggist.

Bolle's Populäre Homöopathische Zeitung.

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